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Office of publication: Curtis Reed Plaza, Menasha, Wisconsin.
Second class postage paid at Washington, D.C., and at additional mailing offices.
Composition by TypoGraphics, Columbia, Maryland.
Printed in the United States of America by George Banta Company, Inc., Menasha, Wisconsin.
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INDEXING: Articles and notes appearing in the *Review* before the June 1953 issues were indexed in *The Reader's Guide to Periodical Literature.* Current issues are indexed by The International Political Science Abstracts, the *United States Political Science Documents,* and the *Social Sciences and Humanities Index.* Microfilm of the *Review,* beginning with Volume 1, may be obtained from University Microfilms, 313 North First Street, Ann Arbor, Michigan 48106. A *Cumulative Index of the Review,* Volumes 1-62; 1906-1968, may also be obtained from University Microfilms. Articles appearing in the *Review* are listed regularly in *ABC Pol Sci* and *Current Contents: Behavioral, Social & Management Sciences.* Book reviews are indexed in *Book Review Index.*

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The Role of Research in the Unification of a Discipline

WARREN E. MILLER
University of Michigan

Presidential Address, American Political Science Association, 1980

Large-scale complex research project designs are providing a new impetus to the elimination of subfield boundaries within political science. Major projects are taking advantage of the methodology and technology of contemporary social research to include comparisons among institutions, across cultural boundaries, and extending through time. As a consequence, traditionally narrow field and subfield concentrations on segments of the political process are giving way to intellectual interests that bring together hitherto separate concerns. The full potential for discipline-unifying research will, however, not be realized until there is a strengthening of the organizational infrastructures for research, a broadening of training in research design and administration, and an increase in funding for large-scale projects. The execution and subsequent intellectual exploitation of large research projects will carry additional problems that will be solved only with substantial changes in the workways of the political scientist, but those problems are greatly outweighed by the positive contribution that such research will make to the future of the discipline.

The growing capacity of American scholars to study politics and governance systematically rather than segmentally has brought us to the beginning of a new era. We should contemplate the future, understanding that political science can and should become a unified discipline. Methodological differences no longer define nor are defined by substantive interests. Many of the boundaries between traditional subfields are sustained only by the inertia of custom; the boundaries can and should disappear. The intellectual challenges of the discipline are increasingly, and appropriately, centered on complex interrelationships that are being explored in major research endeavors. And, because inexorable trends in all of the social sciences, as well as in national science policy, are fostering large-scale research that purposively brings many interrelated interests together in single projects, I believe political science should prepare for a new era in which large-scale, programmatic research activity will give a new and more coherent shape to the discipline.

The historically separate origins of the various subfields of the discipline are now less relevant than the intersections that today connect the subfields. Drawing from my own field of electoral behavior, one can ask how, in a day of presumed single-issue politics, the study of electoral behavior can be intrinsically separated from the study of bureaucratic clienteles that help shape the public policies that are reviewed by citizens engaged in retrospective, issue-oriented voting? How can judicially mandated educational policy be understood or appreciated with no regard for the politically potent demands of affected voters? How can the ebb and flow of foreign policy be comprehended without a basic recognition of the bounds by which public outrage defines the permissible? How can the performance of an office of personnel management be appraised without regard for the electorally inspired concern for civil rights?

More generally, it seems to me there are two logical imperatives responsible for the movement away from the specialization and compartmentalization of intellectual activities that has characterized political science. One of the continuing impulses toward unification of the discipline comes from the very nature of our subject matter. Political science is unique among the social science disciplines in its preoccupation with the institutions of politics and government, their interaction, and the relationships between the institutions and the citizenry. The very concepts of political system, political process or governmental structure properly imply aggregations of activities that are by their nature interconnected and interdependent. The separateness of subfields which have focused on various dimensions and components of the political process has existed despite, and not because of, the interconnectedness of the phenomena around which subfields have been organized.

A second impetus toward disciplinary unification comes more recently from the appreciation and implementation of the basic logic of social inquiry. The development of new approaches to social research, relevant to many problems of political analysis, has diminished subfield parochialism in research and promoted research that is responsive to the interconnectedness of the phenomena we study. In the absence of a practicable mode of carrying out research on large-scale, complex institutions and widely dispersed, mass populations in laboratory settings, the systematic analysis of political phenomena has self-consciously turned to two different modes for testing those parts of political theory that have an empirical as well as a normative referent. The more familiar mode is comparative, searching across more or less similar institutions for insight into that which makes them different despite their similarities.

The second mode, with an equally long tradition, is that of diachronic analysis in which the passage of time (or the accumulation of history, if properly documented) produces change in individual institutions and in the interaction among institutions. The understanding of such change through time complements synchronic analysis of institutions across cultural space. Both modes direct attention to the context provided by functional interrelationships and thereby force a broadening of perspective and analytic grasp; description of the discrete, bounded institution ultimately must give way to an expanded scope of inquiry.

The increasing unification of our discipline as a scientific enterprise has been made possible largely because of the development of new methods of social research. In particular, the post-World War II growth of methods and techniques, often associated with the behavioral mode, has been vitally important, less because of the intellectual ferment surrounding questions of epistemology and more because those methods and techniques have increased our powers of observation. The techniques of contemporary social research have expanded the scope and refined the accuracy of our observations of the real worlds of government and politics. They have made it possible to create evidence that is needed to establish empirical regularities and to test the explanatory theories that transform those regularities from intellectual trivia into matters of important intellectual concern.

In the absence of our generation's research methods, the older, traditional empirical components of political science were derived almost entirely from "process produced" information. Apart from personal memoirs and collections of anecdotes, the evidence on which the discipline's traditional research rested was created for the purposes of the participants in the political process. The study of comparative politics, for example, was largely limited to the textual examination of constitutions, legal codes, tables of administrative organization, financial reports of budgets and expenditures, and tabulations of aggregated election returns. These "hard data" were not developed to serve the purposes of the research scholar, but to facilitate the functioning of this or that institution. Even the discursive textual materials, the treatises that articulate political philosophy or the narratives that describe political experience, were usually produced by self-interested participants whose rhetoric was intended to serve personal or political, value-laden ends rather than the needs of the scholar.

With the development of the new modes of social research, the scientist-scholar has been freed from a dependence on information that is created for the participants in the political process. We can now elicit additional information and thereby create relevant evidence for the relatively precise and very specific needs of systematic inquiry. The development of sampling theory and the methods of applying it to very large human populations, including populations of organizations, make possible an efficiency in data collection that increases the researcher's observational power by many magnitudes. The structured personal interview and the questionnaire provide vehicles for highly standardized and highly comparable descriptions of personal experiences, perceptions, attitudes and behaviors. These new methods and techniques of eliciting information, driven by hunches, curiosities, perceived anomalies of explanation, or, formal theory, and controlled by increasingly sophisticated elaborations of classical experimental design, make it possible to create new knowledge at a rate that challenges our capacities to think new thoughts and achieve new levels of understanding.

It is true that the initial application of the new methods proceeded very unevenly. Following the so-called law of "the child and the hammer," new research tools have often been used rather indiscriminately, and with too little recognition of their lack of relevance for some of the intellectual constructions that scholars have envisioned. At the same time, the invention of new tools for research has also led many of our most creative scholars into a sustained preoccupation with the assessment of existing methods and the invention of still more new methods. From one perspective, these preoccupations with methodology have been criticized as a diversion at the expense of a continuing interest in the substantive problems which should impel and justify methodological innovation. It is undeniably true that methodological domains often acquire a life of their own, and the continued growth of these activities is still very much with us. However, very often methodological developments, particularly in domains concerned with data analysis, are stimulated by the methodologists' recognition of the inadequacies of the data available for scientific use.

It should also be noted that the use of the new tools of research has often led to the posing of new substantive questions—often questions quite remote from the immediate study of political institutions. The sheer ability to study individual behavior more directly, systematically, and comprehensively has led to an emphasis on theoretical problems confronting our understanding of individual behavior. Indeed, for a time, microanalytic concerns threatened to supplant macrolevel, institutional analysis. More recently, and more happily, our abilities to investigate at both the micro and macro levels have been recognized in the design of research that is more faithful to the real world of people and institutions. This is vitally important for the ultimate unification of the discipline because it emphasizes the relevance of empirical, social science for the central questions of both the normative and empirical theories of the discipline.

The last two decades have witnessed the production of a number of prototypic studies that, taken one by one, have added to our understanding of political phenomena and, taken collectively, demonstrate our intellectual and methodological capacity to carry out research projects of substantially greater power and broader relevance than those of earlier years. These are projects that, in scope of complexity, at least start to match the scope and complexity of the real world of politics and government. Moreover, these studies demonstrate that it is possible to do scientifically rigorous work that is sufficiently complex to be commensurate with the intricate problems we are trying to understand or solve.

For example, there is now a long catalog of studies that, by design, embrace two or more different sets of political actors participating in the patterned behavior of two or more political institutions. These studies can be illustrated by Herbert McClosky's pioneering investigation of agreements and disagreements between delegates to nominating conventions and their parties' rank-and-file constituents (McClosky, Hoffman and O'Hara, 1960). The same era produced the first study of the direct linkages between members of a national legislature and their constituencies (Stokes and Miller, 1962; Miller and Stokes, 1963), and the ensuing years have seen this basic design replicated many times over. More recently in the field of electoral behavior, studies have been designed to bring together in integrated analyses the confluence of decisions of candidates and their campaign managers (Goldenberg and Traugott, 1980), political reporters who present the campaigns through the mass media (Clarke and Evans, 1980a, 1980b; Evans and Clarke, forthcoming), and the voters who are responding to the election campaigns as presented by the media (Center for Political Studies, 1978).

Because of the larger problems of logistics and culture, truly comparative synchronic studies of similar political institutions are fewer in number. Nevertheless, the study of comparative politics was clearly reshaped by the work of Gabriel Almond and Sidney Verba (1963) in their pathbreaking study of civic cultures. The fact that comparative studies need not go to the extreme of crossing national lines was demonstrated early in the 1960s by the work of Wahlke, Eulau, Buchanan, and Ferguson (1957) in their interstate study of legislative systems.

Diachronic studies that exploit the passage of time are more numerous. In latter days they are better supported than ever before because of the creation and maintenance of data bases that extend through ever increasing spans of time. Here one can hope that the emergence of new communities of scholars, such as those identified with the Social Science History Association, will add interdisciplinary richness to the task of unifying our study of politics and government.

Many studies have come into being that combine all three themes of inter-institutional, cross-cultural, and longitudinal analysis. As with a number of the earlier studies, the newer studies also foretell an important aspect of the future in their presentation of the results of the active cooperation of a number of scholars as co-investigators and coauthors for each project. The most inclusive of such studies now under way is exemplified by a cross-national, intergenerational, longitudinal study of political participation in eight Western democracies, carried out by a dozen collaborating colleagues under the general leadership of Samuel H. Barnes, M. Kent Jennings and Max Kaase (Barnes, Kaase et al., 1979).

In the same spirit, with varying emphases on the interinstitutional, cross-cultural and longitudinal themes, one can note similar work under way or completed in the Verba, Nie, and Kim international study of mass-elite linkages in local government (1971), the studies of parliaments being undertaken by the Comparative Legislative Research Center at the University of Iowa (Kim et al., forthcoming; Loewenberg et al., forthcoming), the Converse and Pierce study of representation in the French National Assembly (1971, 1979; Converse, 1975), the Cotter, Bibby, Gibson, and Huckshorn study of American state political party organizations (Cotter and Bibby, 1980; Cotter et al., 1980), or the Cain, Ferejohn, and Fiorina comparative study of the American Congress and the British House of Commons (1979a, 1979b, 1980a, 1980b).

Perhaps it is no more than coincidence, but our demonstrated ability to carry out very complex research designs that produce relatively faithful mappings of the interrelated elements of the real world is occurring at the same time that new intellectual foci promise a further integration of political scientists' concerns. Both the new emphases on policy analysis and on political economy seem to me to demand great intra-disciplinary integration. Indeed, much of the nonideological excitement in both developments comes from the new scope they bring to the work of those doing research as well as those committed to action and application. The concept of the political process as a continuing process fits very neatly the theme of studying persistent, normatively important, social or economic problems. When problem-solving policies are examined in the context of the social and the economic, as well as the political structure of a policy, the need to know more and more about more and more becomes pervasive.

Although the time seems ripe, there are a number of obstacles to overcome before political science will come of age as a relatively unified social science discipline. If disciplinary scholars are to be provided with the information and evidence that is needed for major advances in our understanding of politics and government, institutional changes within the discipline will have to occur on at least three different fronts. As things now stand, political science as a discipline cannot respond to its intellectual challenges, or take advantage of the opportunities that those challenges present, because of inadequate personnel, inappropriate institutional arrangements for meaningful research, and limited financial resources with which to carry out truly significant research.

The prime challenge presented by our new capacities and the new potential lies in the fact that their exploitation rests on individual scholars. My remaining comments center largely on problems associated with the institutionalization of complex, large-scale research, but they are pertinent only if such research is guided by the concerns of intellectually creative individuals. The significant contributions of the research that is reshaping our discipline all have their origins in single minds and the capacities of single individuals. Their unique contributions are too often lost as credit is distributed among all of those individuals and organizations required for the execution of a major research project.

The point must be made, and is here recognized, that the scope of large-scale research does not limit, but enhances the importance of the individual scholar. In each subfield the creativity of numerous individual scholars could be identified as the ultimate source of our new knowledge. And it is not the direct participation of these individuals in large-scale social research projects that is crucial; but without their guiding insights and their synthesizing wisdom, our new methodological and organizational capacities would not have remade those portions of the discipline to which their work has contributed. The impact of the research of the future will likewise depend at least as much on the ability of individual scholars to utilize and exploit our new research capacities as it will on the continued and expanded existence of the institutions which provide them.

Although political science training relevant to large-scale social inquiries is infinitely better today than it was two decades ago, it still has major deficiencies. First of all, too few political scientists are trained in the tasks of designing research and collecting data. Whether to take advantage of those new data that have been produced, or to overcome the limitations in those data, our training in empirical studies has emphasized data analysis and data manipulation rather than the generation of new data. Too few political scientists have first-hand experience in designing research that will faithfully reflect the real political world. Numerical theory, and data analysis, will always be vital, but their exploitation must be complemented by expertise in research design and in the conduct of complex, multipurpose data collections rooted in familiarity with politics in action.

There are also too few political scientists who are professionally trained for multilingual, cross-cultural research. Although there has been a certain logic to treating research methods and statistics as research tools that are equivalent to the command of a second or third language, the equivalency is relevant only in those cases where one finds within a single language or cultural area the historical record or the inter-institutional variation that makes rigorous research possible. And, of course, for many inter-institutional comparisons the other institutions are necessarily found in other national settings. I am thoroughly persuaded that the comparative method lies at the heart of our discipline and the power of the comparative method can be fully exploited only by those who command the linguistic and other cultural understandings necessary to specify the real equivalencies in institutions across cultural boundaries. The study of empirical research methods is crucial, but it is no substitute for the training needed to study institutions in cultures other than one's own.

Perhaps the greatest deficiency in the ranks of today's research scholars is the absence of the appetite and the imagination needed to launch and then carry out large-scale research. Coming out of an era in which complex research projects could be mounted only upon rare occasions, we have taken a nettle for a rose and have emphasized the exploitation of data collected by others. As all can appreciate, I am personally persuaded that there is indeed much to be said for the data archive movement (ICPSR, 1979) and fuller use of data which it has made possible. I suspect, however, that the ability to do meaningful research with data collected by others has seduced many scholars into "making do," into molding their research interests to those tasks which are more easily accomplished, and thereby forestalling the commitment that produces the extraordinary effort that is required to launch the most relevant and most needed new research efforts.

Today, unhappily, limitations on the capacity of individual political scientists are matched or exeeded by limitations in the institutional settings needed for the conduct of significant new research. A second obstacle exists in the fact that the discipline is still served almost exclusively by academic institutions that are admirably suited for the conduct of library research or the secondary analysis of archival data, but not at all equipped to support the generation of significant new evidence appropriate to the testing of important social-political theories.

Despite repeated efforts over the past two decades to establish large social science centers to be the equivalent of the astronomer's observatories or the physicist's accelerators, no new centers of first magnitude have come into being. Indeed, many smaller local institutions have withered and disappeared. Given the magnitude of our research needs, and the importance of the social, political, and economic concerns they would ultimately serve, it is hard to understand why the National Opinion Research Center at Chicago and the Institute for Social Research at Michigan continue to stand as relatively unique institutions in a nation as large and as rich as ours. Past efforts of leaders in the social science community to develop and implement policies that would see these two venerable institutions joined by others of similar capacity have failed almost completely. Both the BASS Report (National Academy of Sciences, 1969) and the Simon Report (National Research Council, 1976) which followed it recognized the fact that the institutional infrastructure for social science needed massive expansion if our claims to intellectual or social relevance were to be redeemed. Both reports have been ignored by those who shape national science policy as well as by those who control access to the financial resources that would be needed for such a change.

In like manner, there has not been an adequate response to the need for increased social science research capacities at the university level. Some collective efforts, such as represented by the Inter-University Consortium for Political and Social Research, have persisted with minimal commitment of resources by the participating universities. Occasional departments of political science, blessed with unusually energetic and tenacious leadership, have developed data laboratories. Significantly, however, these and other limited tales of success across the nation pertain almost entirely to facilities for the secondary analysis of data collected by others for other purposes. Although it is no longer extraordinary to find departmental facilities for training in data analysis, the number of departments served by anything more than the most rudimentary, ad hoc capacity for the design and conduct of new research has not increased. To my knowledge, no new major academic resource for conducting original research and generating new data has come into being within the past 10 years.

As a consequence of the limited number of sites where new research projects can be initiated, there has been virtually no training base for research careers. The post-doctoral research assignment is virtually unknown in our discipline. Because of the same poverty of institutional resources, there is no base for the training of research administrators.

The third obstacle to the realization of the promise of a unified, rigorously scientific discipline that is moved and shaped by new research lies in the ubiquitous problem of funding. Whether cause or effect, the level of funding now available for basic political science research is totally inadequate. It is not adequate to maintain a high level of secondary research effort, to say nothing of supporting a portfolio of significant new data-collecting enterprises.

When one examines university support for research, the prevailing norm for the social sciences is still appropriate, at best, for individual efforts modeled on classical library research. Apparently, it is generally considered adequate to pay for a bit of released time for a scholar which, when added to uncompensated evenings and weekends, is sufficient to permit the writing of articles or a book. A serious attempt to collect a significant body of data as new evidence can be mounted only if the researcher seeks funding elsewhere or devotes months, if not years, of individual effort to data collecting. Funding to permit the gathering of evidence which will make possible the subsequent serious intellectual endeavor is extremely limited.

When one turns to the private foundations, the generally low level of support available to political scientists is exacerbated by the maintenance of the philanthropic "do-good" tradition. Those foundations that are interested in significant political problems are most likely to support the writing of a solution-promoting book. Only on rare occasions can a private foundation be persuaded to support a research project that does not promise amelioration but, instead, has data collection and theory testing as significant components. In many instances, of course, foundation resources are limited, and a significant social science research program would quickly spend the staff out of its job. For whatever reason, the private foundations seem wedded to a policy of providing "seed money" to the social scientist, while delegating to unspecified "other" sources the responsibility for providing "harvest" money that would transform planning and thought into research and new thought.

With neither the universities nor the private foundations willing to supply funding for large-scale research, the quest for money usually turns to some agency within the federal government. Limitations on funding levels there also remain disastrously low within such sources of basic research support as the National Science Foundation. When one moves beyond NSF, the philanthropic "seed money" theme of the private foundations is replaced by the need for "cost-effective" investment in problem-solving research supported by government agencies. Mission-oriented agencies within the federal government do indeed support large quantities of research done by social scientists, even by political scientists. However, the emphasis on application is pervasive. It is seldom possible, from the researcher's perspective, to put first things first, and first conduct the appropriate basic research. The problem is doubly severe because there is such widespread pressure, greater now than before the social and political revolutions of the last decade, to demonstrate that one's work is immediately relevant to solving some part of the nation's problems.

Political research is perhaps also uniquely vulnerable when it seeks funding from government agencies that are, in turn, dependent on the political process for their very existence. In part this is true because political self-interest often dictates the response to proposals for research programs, and the worlds of the politically sensitive and the social scientist are poorly knit together in the resulting conflict of interests. We are also perhaps uniquely vulnerable in that those involved in the chain of decisions that would produce support for our research are often the very actors whom we would study. Arguments that we need more knowledge are often dismissed on the grounds that we are simply outsiders trying to find out what insiders already know through personal experience.

The reduction or elimination of the obstacles presently posed by inadequacies of personnel, institutional structure, and funding could bring political science into a new age of intellectual ferment and maturity as a discipline. The excitement of such a prospect can scarcely be underestimated, and yet the realization of such a development would bring with it new problems that are only dimly apparent at this stage of disciplinary immaturity. Many of the problems would result from the fact that, for the foreseeable future, large-scale research will remain multipurpose research. Several correlates of multipurpose research will have to be accommodated by the workways and the norms of the discipline.

In the first place, the risks are greater. Complex research poses new problems from the beginning to the end of the research process. In planning and designing a large study, in carrying out the data collection for the study, and in the analysis of the evidence amassed by the study, there will be opportunities for delay, disappointment, and disaster of a greater magnitude than are now associated with research projects of more familiar scope. Under any circumstance, those who make their living by engaging in large-scale research are aware of the high risks involved. The most exciting research tests significant theories. Since, over the years, it has seemed virtually impossible to disconfirm any important piece of social theory with a single test, it has not been possible to publish null findings. Confirmation and extension are the hallmark of "good work," and the risk of devoting substantial time and energy to produce "nonpublishable" results has dissuaded many from attempting new and more venturesome enterprises and has turned them to less threatening avenues of intellectual activity.

In carrying out a complex research design, there is often a very real possibility that the researcher will not even get to the state of data analysis. Because of our lack of experience in research administration, the sheer management problems involved in large-scale studies offer new opportunities for failure. The large and complex study is not only extremely expensive, but also whenever such a study is innovative in any important aspect of design, the researcher's ability to estimate the ultimate cost is limited. In general, the financial costs of planning with sufficient care, executing with sufficient control, and analyzing to a satisfactory level of closure are very hard to estimate without a background of experience. Individual scholars, at least those with tenure, who confront unexpected delays and expenses in the conduct of personal research can often shift personal timetables and accommodate themselves to the unexpected. A massive research project that runs out of money before the data are collected cannot always be suspended until new resources are found. When time and money are consumed on a very large scale, and where anything short of completion leaves one no further ahead than if one had not started at all, the potential cost, whether out-of-pocket cost or opportunity cost, comes very high.

Quite apart from the intellectual and logistic problems of organizing large-scale research, another set of problems results from the fact that the magnitude of multipurpose research usually demands the cooperative participation of multiple investigators. If the number of scholars responsible for the final intellectual product increases interdependence, and the unification of knowledge, it also poses a further challenge. Unexpected interruptions and delays in the participation of any one investigator have immediate and very human implications for the others. It is often difficult to sustain a steady state of cooperation among multiple investigators whose fates are joined together by their project. Moreover, familiar problems found in smaller if not lesser enterprises are exaggerated in the large project. These originate in the initial division of labor and they continue with efforts to reflect that division in an ultimate division of rewards. At the present time, multiple authorship of articles and books is seen by many as more of a curse than a blessing. Particularly where junior authors are concerned, the question of the value to be attached to a third authorship (particularly out of alphabetical sequence) is threatening at best.

Among senior figures, serious problems can develop as the division of labor may distinguish the intellectual leaders from the chief administrators. In the instance of the national election studies conducted by the Center for Political Studies, for example, it is not clear (and will probably not be clear for some years to come) whether the principal investigator should be the intellectual first-among-equals as a scientist, primarily an expert in the politics of multipurpose research that is intended to serve many intellectual constituencies, or should be primarily an administrator who, as chief executive officer, translates the legislative wishes of advisors and other principal participants into research operations that, when carried out by the research staff, remain faithful to the conceptual superstructure by which others have defined the intellectual destiny of the project.

Whatever the internal division of labor and allocation of rewards, all of those for whom large-scale research is relevant must be prepared for a third change in our workways, a change in the time expectations associated with the completion of significant research. Given proper training, and a year or two for careful thought, a more traditional scholar may well hope to complete a book in three or four years. The number of person-years needed to produce the final results of a complex research project may increase this figure by a number of magnitudes. Junior personnel, in particular, may find the time lag in publication highly disadvantageous for their own career needs. Of course, it is also true that the continued maturation of our theoretical interests may add its own force toward encouraging the publication of the journal article rather than the extended monograph or book. In any event, our disciplinary reward system based on the timely publication of books may have to change, as it has already changed for economics and psychology.

At this remove, none of the problems that would follow from an increased disciplinary role for very large research efforts seems as formidable as the obstacles that stand in the way of that increase. The subsequent problems will complicate the lives of those directly engaged in research, and it will take some major changes in our workways and our professional norms to resolve these and related problems. Nevertheless, the costs of this kind of problem solving would appear to be insignificant compared to the benefits that would accrue to the entire discipline if it were instituted.

Political science has the intellectual capacity and the methodological and technical competency to make a massive contribution to the welfare of the nation while evolving into a conceptually coherent scholarly enterprise. Knowing many of the colleagues who will be engaged in creating a new era for our discipline, I am quietly confident, and I think realistically confident, that to dream the appropriate dream is not to dream the impossible dream.

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"Citizenship" in "Industry": The Case of Georges Sorel

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Georges Sorel was of course a critic of "citizenship" in the sense current then as now. But behind his ideal picture of industrial organization there lay a conception of citizenship which derived from an older tradition. This conception in turn modified the themes which Sorel drew from a second tradition, that of thought about "industry." The result was a vision of industrial organization which displaced civil association by taking some of its typical features for itself. What is represented by notions such as the "myth of the general strike" is not, as some have held, a romantic reaction against modern organization but, on the contrary, a thoroughly modern doctrine of organization as the "sublimation" of civil life which continues to pose problems of relevance today.

In the concluding chapter of his *Politics and Vision* (1960), Sheldon Wolin regrets the contemporary substitution of "organization" for "politics." The individual's attachments and concerns, as pictured in modern social theory, have been deflected from the traditional sites of citizenship to the enclosed and particular sites of corporate life, which, Wolin suggests, have thus come to assume for themselves themes once reserved to the polity itself. In this process of "sublimation," however, the idea of citizenship has necessarily lost a crucial part of its meaning: what a particular association cannot do is to provide its members with that sense of *generality* which the polity, as it was once understood, ideally supplied. Wolin writes: "To contend that individual participation can be satisfied in a political way within the confines of nonpolitical groups is to deprive citizenship of its meaning and to render political loyalty impossible. When used in a political sense, citizenship and loyalty have meaning only in reference to a general order" (Wolin, 1960, p. 433).

Georges Sorel appears no more than fleetingly in Wolin's account, and then only as an example —together with rebellious critics such as Kierkegaard and Nietzsche—of the "last-ditch efforts to secure some place for unorganized individual action," or of the "last gasps of a romanticism doomed to expire before the age of streamlined organizations" (p. 423). His work has such a character, it is claimed, by virtue of that "heroic impulse" which he perceived in the myth of the general strike, through which the industrial proletariat was to achieve a spontaneous and *un*organized unity. Now such a view of Sorel as essentially a romantic outsider is by no means uncommon; but I believe it is wrong, and, in Wolin's case, so *precisely* wrong that it is worth correcting. Far from exemplifying the last gasp of a romanticism doomed to failure by organization, Sorel is among the most striking and interestingly complex examples of Wolin's "sublimation of politics" itself. It is in his writings, more clearly and relevantly than in any others of which I am aware, that one can detect the gradual absorption of a classical picture of citizenship by an ideal of industrial organization. If there is a single developmental point in modern political thinking at which the idea of citizenship began to lose its traditional moorings in the polity, it is in Sorel that we are likely to find it.

There is more to this question, however, than the correction of what amounts to little more than a footnote in Wolin. For Wolin's "sublimation" thesis confronts another well-developed and forceful recent theme, one which is directly contrary in its logic. According to Wolin, the migration of citizenship from polity to enterprise necessarily represents a loss; for other theorists of democracy, from G. D. H. Cole to the present, it represents a clear gain in reality, since, they maintain, a citizenship reserved to the polity can be no more than a fiction. Cole wrote: "The Guild Socialist conception of democracy, which it assumes to be good, involves an active and not merely a passive citizenship on the part of the members. Moreover, and this is perhaps the most vital and significant assumption of all, it regards this democratic principle as applying, not only or mainly to some special sphere of social action known as 'politics,' but to any and every form of social action, and, in especial, to industrial and economic fully as much as to political affairs" (1920, p. 12). Later writings in this vein have often been severely critical of those who, like Wolin, have argued for an essential difference between general and particular contexts of action; in ascribing reality to an illusory "general" order standing over and above the concrete and specific relations of productive life, Wolin, it is contended, is guilty of "reification" (Pateman, 1975). In all of this there are hints, of course, of Marx, whose essay "On the Jewish Question" is sometimes invoked; but Sorel, none of whose writings is ever invoked by theorists of citizenship in industry, anticipated some themes of that school with rather more precision, in seeking to transpose the realm of citizenship rather than (as in Marx) to obliterate or transcend the category. Indeed, if the term had not been put to other uses, and made to stand—misleadingly—for a voluntarist and romantic mood, one might well describe as "Sorelian" all those trends of recent thinking in which citizenship has been located in the industrial realm. In examining the paths of thinking which led Sorel himself to such a vision, we may arrive at some general suggestions about the coherence of the vision itself.

The Citizen, the Warrior, and the Worker

How, though, does the idea of citizenship enter into Sorel's central concerns? Paradoxically, perhaps, it does so by way of a conception which, on the face of it, eliminates citizenship altogether: that of an "industrial society." What Sorel (and his disciple (Edouard Berth) understood by *la société industrielle* was an order following the organizational contours of industry itself, in which no political structure mediated the relations between one productive grouping and another, professional ties replacing civil ones as objects of loyalty. Sorel and Berth were not the originators of such a conception, whose beginnings are often seen in Saint-Simon; but the use which they made of it lent it a quite new edge, and brought into play a most interesting and unexpected convergence of disparate themes. While Saint-Simon had contrasted a society organized on industrial principles with an aristocratic or ascriptive society, Sorel and Berth contrasted it, rather, with a commercial society, *la société marchande* (Sorel, 1935, p. 67). In offering this contrast, they implicitly link hands with an older tradition which likewise had often concerned itself with the relations between "commerce" and "virtue" (Pocock, 1972; Horne, 1978): the civic tradition in political thought.[1]

Montesquieu, Hume and de Tocqueville, among many others, had noted the contradiction between the classical ideal of the self-repression and self-forgetfulness of the citizen and a social order in which the predominant motive was profit-seeking (Richter, 1970). While some—including all three of those named—had resolved or eased this contradiction to their own satisfaction, offering grounds for supposing that citizenship could indeed be preserved and fostered in a society of "merchants," they had in doing so perpetuated the long-standing distinction between a life of business and civic life; in particular, they and others had done so in contrasting the essentially military virtues of the classical citizen with the pacific values of *le doux commerce*. And here one can hardly fail to be struck by the continuity of Sorel's argument with that of these earlier theorists. For, in the first place, Sorel expressly (and unfavorably) contrasted the commercial bourgeoisie of his day with the citizen class of Athens: "The ancient Athenians were much superior to our envious, ignorant and greedy bourgeoisie. . . . The citizens were not merchants, demanding guarantees for their transactions and protection for their industry, or seeking favours from government. They were soldiers whose very life was linked to the greatness of the city. The slightest weakness placed the state in danger" (1889, p. 172). Second, as this quotation itself indicates, the idea of loyalty and devotion, conceived of upon an essentially military pattern, was quite fundamental to Sorel's conception of order: and in proposing to replace the "envious, ignorant and greedy bourgeoisie" with a society of industrial producers, Sorel also assimilated the latter to a soldierly image—"Socialism returns to ancient thinking: but the warrior of the city has become the worker of advanced industry; weapons have been replaced by machines" (Sorel, 1933, p. 311). It was, of course, the supposed warrior-like character of the syndicalist worker that Sorel was to take as his starting-point in his notorious defence of violence and of "class *war*"—an expression which, as the text of *Réflexions sur la violence* makes very clear, is no mere synonym for generic conflict or struggle, but is to be taken quite literally (Sorel, 1930, pp. 434—35). The syndicalist worker represented the citizen in Sorel's mind, while the bourgeois represented the citizen's negation. Austerity and self-abnegation are contrasted with frivolity, luxury and dependence; and it is this essentially moral contrast, so characteristic of a long-standing tradition of civic thinking, that figures in Sorel's picturing of the relations between worker and bourgeois, rather than any Marxian thesis of exploitation, in which Sorel displays little if any interest.

The essential connection between war and citizenship is evident as early as 1889 in Sorel's remarkable book, *Le Procès de Socrate*, in which Socratic philosophy is condemned, as it had been by Socrates' prosecutors, for its damaging effects upon tradition and convention, by which alone the intense solidarity of the city-state could have been maintained. Convention unites: philosophy divides, for dialectical skills are very unequally distributed, and thus separate the philosophical few from the unphilosophical many (1889, p. 176). That, in essence, is Sorel's radical critique of Socrates, who, he contends, had cast potentially ruinous doubt upon the unquestioning loyalties and faithfulness to routine upon which classical citizenship depended. "Socrates strove mightily to break the chains which bound the citizen to the ancient city. These chains were those of military discipline. The citizen was a soldier, closely confined and watched; he was obliged to submit himself to a system of education designed to integrate him and to prepare him well for war" (1889, pp. 6-7). This education was provided above all by a civic poetry and drama; and it is interesting to note that exactly these vehicles of civic education were to reappear, transformed, in Sorel's socialist phase, as the "social poetry" of Marx (1919, p. 189) and as the "drama" (1930, p. 173) of the general strike, the *educative* force of both of which is stressed especially. Moreover, even in *Le Procès de Socrate,* which displays little interest in, let alone any inclination towards, the socialist position, Sorel was already disposed to efface any clear distinction between the realms of war and of work, noting approvingly the military analogies for production in Xenophon's *Oeconomicus* (1889, pp. 378-79): a theme which, as we shall see, was later to be very amply developed.

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[1] In Pocock's discussion it is the *rentier* rather than the merchant who emerges as the enemy of "virtue," for "independent proprietorship" of land *or* of capital was accepted, he shows, as a qualification for citizenship. But it was evidently Sorel's view (1889, p. 172) that the merchant lacked the crucial quality of "independence."

When we turn to Sorel's conception of contemporary citizenship, we find that here too essentially military themes are stressed. In *Le Procès de Socrate* he had written: "It is impossible to understand the ancient constitutions without relating them to military institutions" (1889, p. 167). Likewise, he later insists that the political spirit of Republican France draws what force it has from "military institutions": with the rediscovery of the citizen army by the French revolutionaries, "a quite new notion of the City was born, with strong analogies to that of antiquity, and patriotism became a force of hitherto unsuspected importance. Democracy still lives off this tradition" (1933, pp. 317-18). And as before, this living citizenship, military and patriotic as was that of antiquity, is sharply contrasted with a vacuous and rationalistic understanding of the city. What the revolutionaries *did* gave birth to a new order; what their theorists *said* displayed no understanding whatsoever of the nature of order. "Once the *believer* of the Church, then the *subject* of the King, man was to feel his strength and become a *citizen*; but what is to be understood by this term? Theorists asked what would be left in man after the ties subjecting him to Church and King had been dissolved; this residue was natural man, an abstraction formed out of the virtues and faults of eighteenth-century educated man." This abstraction could be made real only by the application of political power: "Discussion was continued in the manner of the old political theorists; the doctrine which summed up the fundamental principles of government was taken to be of prime importance" (1902, pp. 6-7). The French Republican tradition rested, therefore, upon a wholly deluded self-understanding in which the real sources of civic attachment are overlooked in favor of "abstraction" and political "doctrine," abstractions and doctrines no less dry, corrosive and uncreative than those for which Athens, and Sorel, had condemned Socrates.

It is with the true source of citizenship, the military and patriotic consciousness of the citizen army, that Sorel later expressly links the syndicalist movement. It is the revolutionary syndicalists, he maintains, "who represent at the present time the spirit of the revolutionary warriors who, against all the rules of war, so thoroughly thrashed the fine armies" of France's enemies (1930, p. 376). Nothing could make it plainer that the substance of the city, in Sorel's view, has been wholly severed from the political realm, and that the concrete experience of citizen-like devotion and solidarity is no longer to be found in the official manifestations of *civisme* but in the industrially based institutions which stand in opposition to them. By this device, Sorel transforms the theme of civic solidarity into a thesis of class war. Where theorists of civic virtue had seen integrity in the transcendence or forgetfulness of particularity, and in the recognition of an interest of a general kind,[1] Sorel locates integrity in the class identification displayed in and reinforced by an internal war. Inter-class solidarity, as he came to say, was "hypocritical" (1911, p. 21), resting as it did, he believed, upon self-interested collusions and not upon any vivid communal or cooperative experience capable of bringing into being a passionate and unreflective loyalty.

But Sorel's historical imagery may seem very imperfectly lucid, for in his description of the revolutionary syndicalist movement he also employs an apparently contradictory analogy. He provocatively describes syndicalist militancy as "barbarous," and thus appears to invite us to imagine the revolutionary movement not in the light of the ancient city but in that of the barbarian invaders who destroyed it. This provocation has not been lost on his interpreters, among whom Wolin himself draws heavily upon it (1960, p. 223); and certainly, on the surface, it lends plausibility to the reading of Sorel as a theorist of sheer *élan.* But it should not be overlooked that in the civic tradition the distance between ancient citizen and barbarian invader was much closer than mere historical fact would suggest. As Pocock has reminded us, theorists of civic virtue had appealed at once to "Spartan," "Roman" *and* "Gothic" images (Pocock, 1960, pp. 94-95). Sorel's "Goths," too, are essentially orderly beings, more civic in fact than the citizens themselves;[3] they brought new forms of juridical and social order to a world which had forgotten the "quiritary virtues" (Sorel, 1922, p. iv). They did not destroy the city, for, as Sorel explains at length in *La Ruine du monde antique,* there was no city left to destroy, its spirit having been ruined by processes of internal corruption. Likewise, the syndicalists, as heirs to the "barbarians," bring to the world a new source of *droit,* which is to be found in the practical cooperation and solidarity of the factory, and not in the patched-up compromises of interclass relations upon which the present city expends its declining energies (1919, p. 73 n.). To the extent, then, that Sorel regards the syndicalist movement as a movement of barbarians rather than of citizens, he does so just to the extent that his picture of the barbarians contained in advance that theme of the civic spirit's migration to a new bearer, a theme to which his syndicalist writings were to give a new application and a fuller development.

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[1] There is, of course, Machiavelli's discussion of the benign effects of class conflict in the *Discourses* (I. iv). But Sorel's "classes" are not Machiavelli's (see Sorel, 1930, pp. 73-76), and conflict between them is designed to be terminal.

The Idea of Industry

One problem which Sorel confronted, therefore, was that of distinguishing the true *civisme* of a movement which he admired from the sham citizenship of an order which enjoyed the symbolism and formal trappings of a city. The problem was not new: one might compare Cicero's remark, "Through our vices we retain only the name of a republic, and have long since lost the reality" (see also St. Augustine, *City of God* II. 21, where Cicero is quoted). Sorel likewise contrasted the "vices" of the city-in-name, especially the "voracious appetite" (1930, p. 221) or political passions and ambitions of its inhabitants, with the self-repression and devotion to common ends which the idea of citizenship demanded. A second problem, to which we must now turn, required a more novel adaptation of tradition. In seeking to locate citizenship within the realm of industrial life, Sorel was attempting to put that idea to a historically incongruous use. While his dislike of commercial society was wholly traditional, his admiration for industrial society was not; and in seeking to enforce a radical distinction between production and trade (1902, p. 12), investing the former with virtue, the latter with its corruption, Sorel was doing something almost wholly unprepared for in the civic tradition, whose exponents—from Aristotle on—had been no more inclined to value the producer than the merchant. Here Sorel's thinking brings into play a quite different train of antecedents, many of whose themes had been in some respects profoundly opposed to those of the classical doctrines of citizenship.

As Michael James has shown in an excellent article (1977), the immediate roots of the notion of *industrie* are to be found in eighteenth-century political economists such as Pierre-Louis Roederer and Jean-Baptiste Say, who sought to establish the managerial, productive and inventive skills of the *industrieux* as a factor of production distinct from capital, labor and land. If their principal target was the Physicratic doctrine that land was the primary source of wealth, they were led also to dispute the doctrine that it was the capital of the entrepreneur that constituted his productive function, and also the labor theory of John Locke, for it was not only the *peine* (labor) which one suffered in acquisition but the *adresse* (skill) which one exerted that constituted a property title. *Industrie* no less than land or capital was something in which one could have *propriété,* and the *propriétaires d'industrie* therefore rightfully deserved the citizenship traditionally attached to property, and should be admitted to political participation. It was this that lent the notion of *industrie* its ideological edge: it figured in a demand for the extension of the franchise to the *industrieux,* by establishing *industrie* as a property.

Industrie therefore emerged initially as a criterion of *civisme* or of inclusion in the civic body, a criterion which fundamentally challenged the Aristotelian view (*Politics,* I. 7) that the direction of labor enjoyed no higher a status than labor itself. But it is the further extension of this argument that has a particular relevance here. Initially conceived of in terms of the Aristotelian model of inclusion—who should participate?—the idea of *industrie* passed over to revising Plato, in proposing *les industriels* as the exclusively legitimate bearers of authority, by virtue of the crucial importance and privileged character of their special knowledge and skill. It is in Saint-Simon's celebrated "Parable" of 1819 that this transition is most clearly revealed. At first a liberal who sought to modify the criteria of citizenship and to include *les industriels* within the polity, Saint-Simon eventually moved to a radical demand—"socialist," or "managerialist," or somewhere in between the two—that industrial, technological and creative competence should supersede all other criteria of membership. In the later Saint-Simon, as also in Comte (who added to the doctrine some features peculiarly his own) what began as a project of political reform ends up as the far-reaching enterprise of founding a new social order upon the basis of a newly recognized principle of capacity. In his "Parable," Saint-Simon imagines the sudden elimination of France's present governing class, as a mental experiment designed to convey the uselessness of ascribed status and unearned wealth; how marginal would be the impact of this loss, he says, compared with the devastation that would be caused by the sudden loss of an equal number of scientists, artists, and artisans. The passage is striking not only because (as one commentator has put it) it is a "concise and vivid expression of the theme of an industrial society" (Markham, 1964, p. xxvi), but also because it expresses so clearly the transition from the notion of *estate* (defined by the nature *of* what one does) to the modern notion of *élite* (defined by capacity *in* what one does); for the qualities of *industrie* are, it seems, concentrated disproportionately in the hands and minds of a few thousands of *industriels.* Increasingly, in both Saint-Simon and Comte, the idea of the *industriel* comes to be separated from that of the *prolétaire.*

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[3] It is no surprise that Sorel (1933, p. 138) should have found Macaulay's notion of revolutionary "paganism" appealing, for "pagan" embraces "Roman" and "Gothic" in an ideologically convenient fashion.

"Industrial capacity, that of the arts and professions, is what must be substituted for feudal or military power," Comte wrote (1929, Appendix, p. 64). This theme, common to both Saint-Simon and Comte, is explained by Comte as follows: "There are only two ends that may be sought by a society, however large, just as there are for a single individual. These are violent action directed against the rest of mankind, or conquest, and action upon nature in order to modify it to human advantage, or production. Any society which is not clearly organized for one or other end will be nothing but a hybrid and characterless association. The military end was that of the old system, the industrial end is that of the new." The pregnancy of this remark is evident, for the military organization of the city had been essentially tied, in a long tradition of political thinking, to the very notion of citizenship; bearing arms for the city was conceived of both as an expression of and a prerequisite to full membership in it, as a city necessarily confronts other cities, and the valuing of its identity implies the readiness to defend it. It is no surprise to find, then, that in the industrial and pacific vision, as it reaches its epitome in Comte, it is no longer the city, nor even a state imagined as the bearer of city-like properties, that is to be the primary focus of loyalty: it is the town, *commune,* imagined as an essentially social and productive entity linked pacifically to other such entities, by ties of economic interdependence, on a European and eventually a global scale. This common "action upon nature," as Comte calls it, supplies them with a shared interest, an interest institutionally embodied in their acceptance of the authority of an elite of savants, of scientists and sociologists whose universal "theory" is a necessary foundation for the local "practices" of productive life.

Now Sorel despised Comte and his disciples with real vehemence. What provoked him was in part the positivist view of history as the march of enlightenment, for Sorel was a shrewd methodologist and hence a sharp critic of notions of inexorable historical progress, on grounds that have since become quite familiar. What also provoked him was Comte's view that practice was the child of theory, that technology depends essentially upon a chain of abstract sciences which are the province of the savant, and which find their ultimate expression in a set of rationally connected and stable theoretical doctrines. In truth, Sorel believed, science is the child of technology, and the theoretical tenets of science are no more than abstractions performed upon the concrete operations of the factory (1894, pp. 201-11). It is therefore no accident that Comte (1975, Vol. 2, p. 73) should have called for the harmony of "hands" and "heads," while Sorel (1908, p. 60) called for a "philosophy of the hands" which would exclude a "philosophy of the head." But Sorel's evident rejection of the Comtrean tradition, as of the Saint-Simonians—whom he called "bookworms"—should not blind us to the fact that what he was offering was not at all a rejection of the idea of *industrie* but an internal modification of it. Saint-Simon may or may not have been a socialist, Comte was not; but when Sorel defines a socialist order as one which is "organized according to the requirements of production," he is clearly echoing the theorists of *industrie* in constructing his picture of socialism. Sorel's socialism, no less than Comte's technocracy, was motivated by a dislike for the political class in general, and for lawyers in particular—for abstract and manipulative skills which do nothing, as Sorel put it, to keep the factories running; and the ties binding one *polytechnicien* to another, one suspects, go deeper than mere ideology.

What made Sorel's picture socialist was precisely the fact that he differed from Saint-Simon and Comte with respect to the identity of the *propriétaires d'industrie*; in Sorel's view, which is certainly eccentric, and which may very well be plain wrong, the conditions of avanced technology call for a wider and wider diffusion of skill and technical knowledge (1947, pp. 282-83), so that, in effect, the criterion of *industrie* tends to become increasingly inclusive, embracing those whom earlier theorists had called *prolétaires.* Nor can there

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be much doubt, despite the fact that Sorel described himself with perhaps surprising persistence as a Marxist, that it was this French tradition, rather than a Marxian one, that lends intelligibility to what he wrote. For whereas Marx primarily stressed the dispossession of the proletariat, its reduction to bare laboring humanity (*peine*), Sorel stressed above all its acquisition of technical capacity (*adresse*), hence not its dispossession but its proprietorship of productive skills. In fact, in a most intriguing passage, Sorel suggests that property itself is valuable only as a medium for the development of skill (1947, pp. 282-83) and he thus shifts the focus of concern from the Marxian question of ownership to the question of the opportunities presented by the work process.

Sorel's belief that modern industry enhanced the skills and technical capacity of the workers is so odd a view, and cuts so sharply across a powerful and familiar trend of social criticism, that we may well speculate about the reasons for it. Here Comte's rigorous distinction between what he called "theory" and "practice," between the hierarchy of abstract sciences and the loyal but merely laborious application of the worker, may provide a valuable clue. Such a distinction, which Sorel found so offensive, recalls the central theme of his early book on Socrates, who, Sorel maintained, had ruined the ancient city by separating the able and knowledgeable thinker from the simple and unreflective doer. By separating thinking from doing, he had destroyed the civic unity founded upon the shared central symbols expressed in poetry and drama; the city's judgment against Socrates (technicalities apart) was in the last resort correct, for Socratic philosophy was indeed destructive of the city. It is surely not fanciful to suggest that Sorel's critique of Comte should be read in the light of his earlier critique of Socrates. The technocratic division of industrial society into scientists and managers on the one hand and laborers on the other echoes the Socratic division of the city into philosophers on the one hand and warriors and artisans on the other. In criticizing Saint-Simon and Comte, Sorel was recalling implicitly an archaic vision of the city, with an undivided life and a common end; and we may speculate that his astonishing picture of the industrial workplace expressed his need to find a contemporary bearer for this longstanding image.

Comte, moreover, had contrasted "conquest," the "violent action" of a city "directed against the rest of mankind," with "production," or "action upon nature in order to modify it to human advantage." It is of the greatest possible significance that where Comte spoke of "action upon nature" in contradistinction to "conquest," Sorel spoke, rather, of the conquest of nature (1930, p. 96), bringing to the technological exploitation of nature a language strongly infused with military images. Sorel imagined "men" and "nature" as antithetical, men's efforts being constantly thwarted and obstructed by the resistance of the natural world. The producers' virtues are citizen-like, too, not only in their belligerence but in the fact that they display that "excellence in the mass" of which Aristotle had spoken; they require heroic solidarity, not heroic personality, and learned practical competence, not esoteric knowledge. It was in part the reminiscence of this image that led Sorel to depart so sharply from the Saint-Simonian or Comtean picture of science and society, just as it intensified his distaste—itself inherited from such as Saint-Simon and Comte—for the elite of orators and intriguers who were ruining France just as they had once ruined Athens. And with Sorel's adaptation of the notion of *industrie*, that notion undergoes a further transformation: at first a criterion for citizenship (in Roederer and Say), and then a substitute for citizenship (in Saint-Simon and Comte), it eventually becomes a new model of citizenship, the industrial world bearing all the idealized properties of the city itself.

These idealized properties, as a necessary corollary, are drained out of the *political* city, which comes to bear all the antitheses of order. It is evident that this logic points strongly towards the images of war which were to figure so prominently in *Reflections on Violence*, for it tended increasingly to assimilate the relations between classes to the relations between hostile cities. Revolution becomes internal war. It is no longer a transition of the kind imagined by earlier theorists of *industrie* such as Comte, a developmental process undergone by a whole society, in the course of which all the parts of an order are operated upon by factors which eventually culminate in a new and total way of life and thought: it cannot have this character, for a *city*, unlike a historical tendency, requires acknowledgment and loyalty, and cannot be the cumulative product of successive unwitting contributions. Nor, on the other hand, is revolution a catastrophe, an extraordinary and destructive event *after* which the form of a new order of things will eventually come to light; it cannot have this character either, for a war presupposes an already-existing recognition of solidarity and identity (Vernon, 1978, pp. 23-37). In opposition to both of these broad conceptions of transition, Sorel insists that a revolution is the displacement of one order by another which has already developed its institutions and loyalties, and that the *civisme* of the new order should have emerged in advance among the members of the insurgent movement.

The Sublimation of the City

The centerpiece of Sorel's revolutionary syndicalist thinking is of course the "myth of the general strike": and the analysis sketched above should help to shed light upon the political meaning of this construct, which may indeed be seen as the culmination of Sorel's effort to transpose the values of *civisme* to the world of *industrie.* Much attention has been given by Sorel's commentators to its "mythic" character; much less attention has been given to its equally significant "general" character. But in both respects, its features evidently derive from what we may call the civic dimension of Sorel's thinking. The idea of myth has an altogether incongruous place among the highly rationalized conceptions of *industrie,* which, according to Sorel, brings about the progressive extension of what is made at the expense of what is given, and thus inexorably reduces the realm of mystery. In this respect it belongs to Vico's "third age" of "men," and not to the mythic ages of "gods" or even of "heroes" (Vico, 1970, pp. 285-98). Sorel's civic theme, however, allows an ample and quite fundamental role to myth, for, he writes, "It is impossible to express ideas about the *patrie* except in a mythical form" (1933, p. 245). "Generality" belongs likewise—as Wolin stresses—to the civic ideal; and we may suggest tentatively that its application to the claims and imagery of the socialist movement forms an essential part of Sorel's effort to understand an industrial organization as a city.

During the 1890s, Sorel had espoused a highly individual but not wholly unorthodox social-democratic position, which allowed a significant place for political reform and for the political institutions permitting reform to be pursued. After the turn of the century, as is well known, he abandoned this position for a version of socialism in which all political instrumentalities were condemned root-and-branch, and everything made to hinge upon the autonomous or even perhaps antinomian development of a revolutionary movement. This transition is most economically viewed in terms of the shifting relations which Sorel perceived between the two cities with which he was preoccupied. The Augustinian echoes of this phrase are not out of place: for, like Augustine, Sorel stressed the fact that the two cities, the true and the false, were "mixed" together (1919, p. 17), their members—though destined for different ends—spatially interspersed and highly interdependent. Indeed, it is only by a "mental effort" (1930, p. 189), or by a process of abstraction which Sorel calls *diremption* (1930, p. 407), that the lines of demarcation between the combatants can be clearly grasped, so manifold and concrete are the ties between one and the other. Though Sorel's cities, like Augustine's, have different "times," different ideal schemes of beginning and destination, the industrial city being defined by its "socialist future" (1919, pp. 77-167), the two share for the time being a common "history." What is still more to the point is that Sorel admits, as Augustine did, that the false city may offer a field of experience in which the loyalties of the true city are tested, and also an analogy or model through which the virtues of the true city may be taught. In the writings of his social-democratic phase, Sorel did not claim anything more than that for political participation in Third Republic France. When, in the course of the Dreyfus affair especially, he had advocated political involvement on the part of the socialist movement, he had done so on the grounds that such involvement would stimulate and render habitual a sense of justice of crucial long-term importance to the movement itself. "The only manner in which we may cultivate a sentiment is by exercising it whenever an opportunity presents itself. Since we propose the effacement of classes, we must exercise the sentiments of justice, especially in circumstances in which they conflict with class divisions. In fact, we cannot be sure that we are pursuing justice unless we are quite certain that we are disinterested" (1898, pp. xv-xvi).

What justifies political action by the socialist movement, therefore, is the resemblance between the struggles internal to the political city and those to be waged against it, a resemblance which is sufficiently strong that participation in the one reinforces commitment to the other. At this stage, indeed, Sorel is prepared to see both as involving the cause of a universal principle of "justice," a notion which at that point makes one of its rather rare incursions into Sorel's argument. Ten years later, all such appeals to principles or ideals supposedly transcending class boundaries are roundly condemned, and commitment to the struggle against the Third Republic is held to exclude all participation in the struggles internal to it. But there is no fundamental change in the structure of Sorel's thinking here, for it is the *resemblance* of one city to the other that now displays a second facet of its ambiguous potential. Resemblance may supply an argument for continuity, the experience of action within the political city coinciding with and reinforcing the ideals which are eventually to lead to its destruction: or it may supply an argument for displacement, the two cities posing a choice precisely because of their resemblance as orders with sovereign claims, which excludes all accommodation between the two. As this second version of the argument evolves in Sorel's thinking, we find the language of civic loyalty penetrating his discussion still more deeply. The working class becomes "a people" (1902, p. 28); the institutions of the Third Republic become "foreign" to it (1919, p. 69); and, most notably, the notion of war against another class is added to the metaphorical war against nature in which the industrial city is said to be engaged.

This new "people" is no longer to form and model itself by participation *in* the institutions of the city which confronts it, but by participation in an act of revolutionary opposition *to* them. Nevertheless, it is still in the city-like activity of war that the movement is to define itself, and to that extent Sorel's new strategy still insists upon the need for the city of industry to draw upon the logic of the political city in order to disentangle itself from it. And just as, in the manner of other sovereign entities, it must define itself by war, so too, in the manner of other sovereign entities, it must define and found itself by myth, a conception which, in the history of political thought, bears an especially close relation to the question of beginning or founding (Tudor, 1972). The loyalties upon which an established city depends are secured by the force of convention or tradition transmitted over time; but a process of transmission requires a beginning, and here myth seems a necessary complement to tradition, for there must have been a time when the loyalties later to be sustained by convention and habit required to be sustained by vivid prospective anticipations, and by epic events which were to reveal their full meaning only with the passage of time (Pocock, 1960, pp. 242-43). Sorel's myth is above all a foundation myth, describing events in the light of which an order is to understand its identity, and from which later generations will draw their traditions of understanding and behavior (1930, p. 436). As in his social-democratic phase, Sorel's central concern is still with "the genesis of morality," with providing the institutional and psychological conditions for that spirit of devoted self-sacrifice upon which the new order will eventually rely.

What is meant by the "irrationality" of Sorel's myth, therefore, may need to be revised. Its irrationality is not, as some have supposed, that of a romantic gesture, an attempt to keep the flame of commitment burning without regard to the consequences of doing so: Sorel's approach is thoroughly (and ambitiously) consequentialist in its assessment of the manner in which institutions and practices exercise their long-term impact upon belief and action. Its irrationality would appear to be, rather, that residual irrationality of the city itself, a feature which has migrated to industrial organization along with the other features of *civisme*. Simply because it is a bounded and exclusive entity, a city draws limits to the scope of reason: it accepts an intensity of violence in its external relations which it cannot tolerate internally; and it must take as given the accidents, the force, and the sentiments, prejudices or legends which make it one city, for its boundaries are not determined by anything more than that. The apparent romanticism of Sorel's argument, one suspects, springs wholly from the fact that it transfers claims from a context in which we are accustomed to see them to a context in which we do not expect them to appear. His warrior-like syndicalists, unified by a colorful epic of struggle and triumph, parody the idea of the city; but like all parodies, Sorel's picture retains some significant faithfulness to the original.

As for the "generality" of the mythic strike, may we not suspect here, too, a parody? The *grève générale* displaces, yet also imitates, the *volonté générale* of citizenship. Like Rousseau, who saw in the "art of generalizing" the ability to link particular with civil ends, so too Sorel values the general strike as the means of overcoming local particularity of interest. The workers' movement, as he insisted, rested upon essentially local organizations, upon the networks of personal and professional ties among the inhabitants of provincial towns (1902, pp. 27-28); and the function of the idea of the general strike is precisely to link such complexes of local interest and attachment to a nationwide movement. "Strikes would be no more than simply economic events of negligible social significance," he wrote, "if the revolutionaries did not intervene, changing their character and making them episodes in the social struggle. Each strike, however local, is a skirmish in the great battle known as the general strike. The association of ideas here is so simple that one has only to point it out to striking workers in order to make them socialists" (1919, pp. 60-61). By virtue of this "association of ideas," "each," in Rousseau's words, is to include himself in the "all," so that the ends of all are motivated by the interests of each. For Sorel, no less than Rousseau, was worried by the menace of particularity, and also by that of the "will of all" which substitutes a self-interested coalition of forces for the expression of the general ends of the movement (1919, p. 69). And even more like Rousseau, Sorel insisted that the solution to this problem lay in the effective organization of communal will, not in subordination to government; just as an effective *volonté générale* is to render government a mere instrument of a community made one by its own consciousness, so too the *grève générale* is intended above all as an alternative to the imposed and artificial unity of bureaucratic organization: syndicalism is to reduce "central committees" to mere "administrative offices" (1902, p. 26).

But the critique of bureaucratic organization does not imply a critique of organization as such. On the contrary, what is eventually to substitute itself for the artificial organization of government

—or for that of political socialism—is the organization of industrial production itself. The Rousseauean general will of citizenship collapses into the "generality" of production, which is, Sorel says, *générale, anonyme, inconscient* (1933, p. 272). It is "general" in that each producer figures only as a small part of an immense and interdependent order, whose time is constituted by the patient, methodical and cumulative progress of achievement, the scale of which altogether exceeds individual control. It is "anonymous" in that the producers—like Rousseau's citizens, who recognize that any law bears equally upon themselves and others—are interchangeable, each filling a role which another might fill equally well. Since it is also "unconscious," it requires for the time being an organization of consciousness which draws directly upon the model of the city, a class war which transposes to the workers' movement the themes and images of the political city itself: the myth of the citizen-army, of the revolutionary *levée en masse*, is to be detached from the Jacobin tradition and attached to the socialist movement. But after victory, as class war in turn fades into memory and legend, it is the endless struggle against nature that is to supply the new order with its organizational principles, and the "generality" of productive organization is to displace the generality of will.

Citizenship in Industry?

Wolin views Sorel as a romantic opponent of the organization which has displaced the city. What has been suggested here is that, on the contrary, Sorel romantically reduced the city to an organization. Any notion that he revered the unorganized should rapidly be dispelled by this brief account of what he took the perfected industrial order to be like:

> It must be understood that the socialist factory will be a grouping of producers who will always be on the alert in criticizing received practices, who will be guided by supervisors comparable to the demonstrators employed by professors of chemistry, and at the head of whom will be engineers who will speak to their men as a master speaks to his students (1947, pp. 354-55).

Whatever this is, it is not a cult of spontaneity. On the contrary, it is connected with a long-standing view of Sorel's that the strictest control of the individual by order does nothing to diminish freedom (1894, p. 179). That view was itself an element in his classical and civic reminiscences—as he continued to make clear even in his last writings (1921, p. 432)—in which it was the citizen's devoted self-abnegation that was especially stressed. I have tried to show above that, in the general logic of his thinking, his admiration of industrial organization sprang principally from a highly selective picture of the nature of citizenship: what he regarded as its fundamentally military character came to be connected in his mind with an equally selective picture of the nature of industrial work, and as these themes progressively fused the political city came increasingly to be dispensable, no longer of value even as an exemplification or reinforcement of moral experience. It is in terms of this fusion of themes, it was argued above, that Sorel's relation to both the civic tradition and Saint-Simonian "industrial society" should be understood; and it is in terms of this fusion, too, that the central concepts of his syndicalist writings acquire a degree of coherence, and that the nature of its irrationalism may be defined.

As noted above, however, the themes touched upon here may offer more than a revised interpretation of Sorel. What has been at the center of this discussion is Sorel's *pars pro toto* argument, and this is an argument which, as Wolin stresses, interestingly underpins so much of modern social and political thought. In particular, it is one which later theorists of participation and self-management have emphatically reinforced, in taking the industrial enterprise to be an analogue of the polity itself. To be sure, a discussion of Sorel's position can shed light only indirectly upon that of later theorists who have not shared in many of Sorel's central assumptions. But the general theme which Sorel played a neglected part in introducing has very far-reaching implications.

Sorel, as we have seen, drew attention to certain resemblances between the (industrial) part and the (civic) whole. Here he has been followed by later theorists (Cole, 1920; Bachrach, 1967; Pateman, 1970) to the extent that they too have denied that any qualitative gulf exists between the values attached to the citizenship available at the level of political institutions and the values which may be taught and learned at the level of the industrial enterprise. In several respects, it may be noted, Sorel has not been followed. First of all, Sorel spoke the classical language of self-repression or self-discipline, while the later writers have employed the more modern and appealing language of "self-development" and "self-realization." Second, Sorel distinguished between the "civic" and the "political," regarding the former with favor, the latter with contempt; later theorists, however, have entirely abandoned this distinction, investing the industrial enterprise with political no less than with civic properties. Third, the later writers have connected citizenship essentially with democracy, while Sorel—as his view of the socialist factory makes quite clear—did not. We shall consider shortly how much difference this makes to the theme at issue.

In Sorel's notion of the equivalence of industrial and civic identifications, we have observed a series of conflations or elisions. In particular, what has been elided is the distinction between the features of a thing and the definition of it. Participation in *industrie,* initially proposed as a criterion of membership in civil association, comes to define the nature of the city itself, and what began as an idea of qualification *for* citizenship comes to determine what citizenship *is*; what originated as a test becomes an analogy. Likewise, military capacity had once been viewed as a qualification for citizenship, serving either as a test or as a preparation for the enjoyment of political rights—those who fought *for* the city were entitled to be members *of* it. But Sorel's "war" against nature is not a preparatory but a definitive value, and it defined what the industrial city is in a manner in which war did not define what the political city was, however essential it may have been to it. In this respect, we may note, Sorel's proposed analogy between warrior-citizen and producer-citizen breaks down entirely, for the former is a citizen by virtue of being a warrior, the two statuses being distinguishable if inseparable, while the latter is citizen-like in being a producer, the two terms being merely interchangeable descriptions. Finally, we have seen that Sorel sought to make a transition from political to pre-political analogies: initially valuing the socialist movement for its capacity to participate in political struggles over issues of concern to a whole society, he had quite rapidly transferred his allegiance to a movement which rested upon an exclusive myth. Political struggles, as he contended, require "disinterestedness," the setting-aside of exclusively sectional attachment; myth, on the other hand, requires the identification of sectional and general ends, the setting-aside of particularity and the recognition of a common attachment now being accomplished within and not beyond the socialist movement. And the movement now retains its equivalence with the civic, not by virtue of expressing a comparable mode of action, but by virtue of resembling the city in its extra-political aspects, that is, in relying likewise upon violence and myth.

The consequences are revealed in Sorel's sketch of the factory of the future. The engineers are to "speak to their men as a master speaks to his students": the factory has become a school. It is not, however, a school of citizenship in the sense that what it teaches is preparatory to some further activity; it is not a school "of" *anything.* Here it contrasts with those other educational devices which Sorel had so admired in the ancient city—poetry, drama, myth and war—whose function was, after all, to equip the citizen for something else. Sorel has abstracted the experience of civic education from the ends which had made it "civic," and is thus led to his disturbing picture of a lifelong and intense preparation which is not, however, a preparation for anything. Now if all that concerns us here is the transmission of experiences of a certain kind, then indeed the relation between part and whole ceases to pose any problems at all. We can readily imagine that an experience of collective attachment could occur no less easily at one level than at the other, that such a value is wholly mobile between city and enterprise, and that differences among levels are, as it were, merely quantitative. But we may wonder if the notion of citizenship, thus understood, has any specific meaning left to it at all, and whether what is being discussed might not better be placed under the heading of moral education or personal development.

It is here that what needs to be said about Sorel may also need to be said in the context of later thinking. There can be no doubt that various attributes of the polity are also to be found elsewhere, whether we choose to stress (with Cole) a sense of responsibility, or (with Pateman) a sense of personal efficacy in the making of decisions which affect one, or (with Bachrach) the existence of power and authority relations, or the making of socially relevant decisions. But in these cases, no less than in Sorel's, one may wonder if the nature of a polity is altogether exhausted by analogical arguments; for analogy by its very nature is selective, and the resemblance of one institution to another—even in important respects—cannot establish their mutual substitutability. In those cases in which the analogy drawn bears upon the occurrence of some experience, the acquisition of some skill, or the learning of some attitude, we may indeed, as in Sorel's case, take this experience or skill or attitude to define what citizenship is; but then, as also in Sorel's case, we may wonder whether citizenship could not be exchanged for some other and more precisely descriptive term.

Unlike Sorel, as noted above, later theorists speak of "self-development," and also of "democracy" as a value. The idea of self-development may carry with it the implication that the attitudes or skills acquired in the context of the enterprise can then be applied by the individual elsewhere (Pateman, 1970, p. 110). Similarly, to speak of *democratic* citizenship is to introduce an appeal to a set of values which can and presumably should be brought to bear in any relevant context, and thus again to suggest that the principles learned in the enterprise may form the basis of action within, or reform of, a larger context of institutions. Unlike the various analogies offered above, such suggestions provide causal assertions which are open to empirical test, and do not simply assert a more or less plausible resemblance; the claim that what is learned or acquired in one context is subsequently transferred to another is, on the face of it at least, a convincing one, and one that has important implications for democratic theory. But this version or aspect of the participatory argument seems curiously at odds with the analogies which are held to support it. For if indeed an enterprise is an arena of citizenship, or a polity, why should it be so important that the experiences gained within it should be capable of transfer beyond? It is important that such a transfer should take place only to the extent that the further context of action is seen to be *un*like the context of original acquisition; for if the two were not in some respect different, the original experience would be complete (as Sorel, apparently, actually came to believe).

This conceptual difficulty is compounded by the transposition not only of "citizenship" but of "politics" itself from whole to part. Here, as noted above, later theorists have departed drastically from Sorel, who had no wish at all to claim the mantle of "politics" for the renewed "citizenship" of *industrie.* He evidently did not feel that the restriction of "politics" to the institutions of the state hampered in any way the cultivation of "civic" properties in the enterprise. The later theorists, however, dissent sharply, and take the view that their case for enhanced participation at the level of the enterprise hinges fundamentally upon the recognition of its "political" status. But this seems doubly unfortunate. If "citizenship" is to become a category of moral experience, then we clearly need another term for the category of relations among those institutions within which that experience occurs; and if "politics" follows "citizenship" along the Sorelian path of transposition, it is hard to see what term is left. If, on the other hand, the "political" character of the enterprise is taken to refer directly to its structural position—for example, to its impact upon general questions of distribution (Bachrach, 1967, pp. 80-81)—then once again the argument from analogy loses its point, for the internal similarity of enterprise and polity becomes irrelevant to the argument (Barnard and Vernon, 1975).

Sorel's contribution to modern political theory is usually regarded as malign. I have tried to show above that it is often condemned (or dismissed) for the wrong reasons, and that his central contribution bears a closer relation to some important current views and assumptions than is usually thought. He is the nearest and clearest ancestor of a transposition of language and of image which since his time has become increasingly pervasive. Perhaps, thanks to our emotional distance from Sorel, his writings offer an opportunity to assess not only the political but also the conceptual costs of his revised conception of what it means to be a citizen.

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Industrial Self-Management and Political Attitudes

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The purpose of this article is to test the widely shared view that the experience of industrial democracy, the experience of direct decision making at places of work, necessarily leads to the enhancement of cooperative and egalitarian orientations among participants. Based on data collected through in-depth interviews and questionnaires in Pacific-Northwest plywood cooperatives, the article argues that in market societies, such experiments in industrial democracy do not have the hypothesized results. Indeed, they seem to encourage the development of values most closely identified with classical liberalism or what MacPherson terms "possessive individualism."

It is widely believed that the experience of direct workplace democracy has profound effects on people's politico-social orientations and behaviors. More to the point, it is almost universally assumed that experience with cooperative and democratic procedures at the workplace, even in liberal-capitalist societies, tends to encourage the development of cooperative and democratic inclinations among people who are active in various forms of industrial democracy. This view is held by scholars and political commentators who are both friendly toward industrial democracy and those who are decidedly unfriendly.[1] The purpose of this article is to test the validity of this view by examining the observable effects of the experience of industrial democracy on people who are actively engaged in workplace decision making. Does the experience of cooperative and egalitarian forms of productive activity at work translate into the enhancement of cooperative and egalitarian values and behavior in a society (the United States) whose cultural benchmarks are individualism and competitiveness?

By far the most pronounced expression of the view that cooperative and egalitarian work relations nurture generalized cooperative and egalitarian values and behavior may be found among various shades of socialist opinion, both in the United States and abroad. In one variant of this view, industrial democracy represents an approximation of a humane socialism that might exist and even expand within the context of liberal-capitalist society (Bernstein, 1974; Blumberg, 1969; Cole, 1919, 1920; Pateman, 1970; Vanek, 1975). As such, industrial democracy is valued in its own right and as an embryonic vision of the good society. It offers the possibility of creating islands of partial socialism, defined as worker and community control of productive enterprises, within the very body of capitalist society. While the idea remains unstated, one gets the impression from this view that the transformation to socialism might be accomplished through a peaceful process in which workers and communities gradually take over an ever-larger proportion of enterprises.

A commitment to the most extensive form of industrial democracy, namely, complete workers' self-management, is now an integral part of the articulated program of virtually every socialist and communist party in the industrialized West. In the United States, groups like the New American Movement, The Democratic Socialist Organizing Committee and the Campaign for Economic Democracy have also embraced the principle of self-management. While I do not have space to explore the roots of this interest in self-management, it is clear that it is compounded out of a

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This research is supported by grant #SOC 76-11897 from the National Science Foundation. I would also like to acknowledge the invaluable assistance at all stages of the research of Thaddeus Tecza.

[1] A range of literature supports various forms of workplace democracy. Commitment to very limited forms may be found among business and political leaders disturbed by the so-called "productivity crisis" in the American economy, hoping that small-scale participatory reforms might serve to "cool-out" labor conflict, enhance work enthusiasm, and diminish absenteeism, turnover, and product call-backs (Gooding, 1972; HEW, 1973; Jenkins, 1974; Sheppard and Herrick, 1972). Industrial democracy is also advocated by some who perceive it to be an intrinsically superior form of human work existence. Humanistic psychologists like Argyris, Maslow, Horney, and Rogers have long called for new forms of work organization that more fully involve the worker in enterprise decision making as a way to transcend the alienation and stunted personality development all too typical of conventional work organization and to allow human beings to fully develop their powers of creativity, self-esteem, and self-actualization (Argyris, 1965; Horney, 1950; Maslow, 1965; Rogers, 1951). Another strong voice calling for industrial democracy comes from the participatory tradition in democratic theory, based on the contention that a democratic political life must necessarily be built upon the bedrock of a democratic citizenry trained in all of the major institutions of the social order (Bachrach, 1967; Bay, 1958; Blumberg, 1969; Kariel, 1970; Pateman, 1970).

complex interaction among the traditional Marxist analysis of alienated labor, the recurrent and seemingly spontaneous phenomenon of worker-councils and soviets appearing in the midst of virtually all twentieth-century social struggles, and the apparent poverty and inappropriateness of most of the traditional strategies of the left in the advanced countries. Out of this confusing combination of developments, the model of the worker-managed enterprise has emerged as a key element in new thinking about the transition to socialism based on the conceptualization of worker-management as a training ground for nurturing socialist relations and values.

Much of this fascination with industrial self-management can be traced to the rediscovery and popularization of the work of the great Italian Marxist theorist Antonio Gramsci, who, more than any other theorist of the left, viewed organization and struggles at the point of production as the energizing forces for breaking capitalist domination and catalyzing the process of revolutionary social change.[2] Perceiving the transition to socialism as a very long-term process dependent upon the readiness of the working class for its historic task, Gramsci places the struggle for democracy at the workplace at center stage as the great classroom for the self-education of the working class.

Gramsci's great theoretical contribution was to grapple, with some success, with the question of why socialist revolutions had not occurred in the developed capitalist countries where presumably all of the objective material conditions existed for such revolutions. Gramsci suggested that the continuation of capitalist domination might be explained by the existence of both coercive power, situated in the institutions of the state, and the *hegemony* of civil society, or domination of capital through the instruments of ideology, legitimation, and false consciousness. In the Western nations where the state is merely the outer "ramparts," it is hegemony (the spontaneous loyalty that dominant groups gain by virtue of their social and intellectual prestige and by their leadership of productive and political life, and conversely, the resignation and confusion that arises out of the experience of being dominated [Williams, 1960]), that must be opposed by a working-class counter-hegemony as a prerequisite to social transformation.

In his earliest works, in particular, Gramsci placed great reliance on worker-councils as the seed of a new working-class counter-hegemony. Impressed by the ubiquity of soviets in all revolutionary struggles and particularly impressed with their role in the October Revolution, Gramsci conceived of worker-councils as natural proletarian institutions in which workers might enter into socialist relationships and incorporate socialist values and activities. He argued that by organizing at the workplace, by taking over production from management, and by producing cooperatively, workers would be educated to socialism and gain the capacity to rule society. In these institutions of self-education, workers would acquire skills, capacities, responsibilities, and confidence in their abilities as a class. In these councils were the tools for enabling the formerly subordinate class to gain a belief in itself as a ruling class.[3]

Despite their many theoretical differences, these positions have in common the belief that even in the midst of a larger capitalist society, worker councils and other forms of cooperative, collective organization at the point of production might represent powerful schools for the self-education of the working class, that such institutions might provide a setting in which values and behavior appropriate only to wage-labor under capitalism would be dissolved and those more appropriate to socialism would be given birth. What is so striking about the presumed relationship between self-management and the nurturance of socialist orientations, however, is the almost absolute paucity of data that would enable any reasonable judgment to be made about its veracity. I address myself to that shortcoming by generating data that should enable us to begin to answer the question of what happens to people who work in self-managed enterprises, to ask what value and behavioral lessons are learned in them, and to ask how people emerge in a political sense from such experiences.

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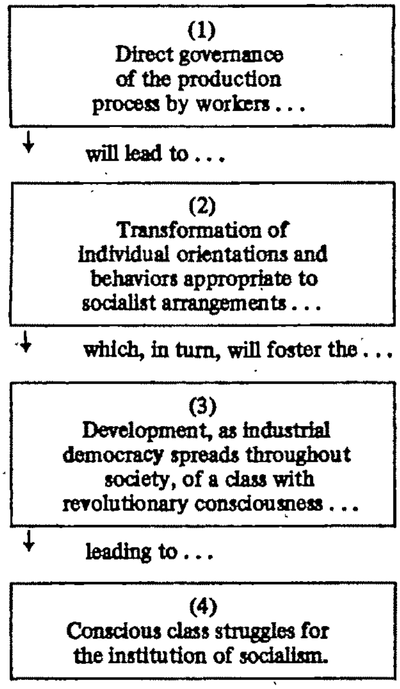
[2] Since the rediscovery and translation of Gramsci's works in recent years, there has been a virtual avalanche of Gramsci scholarship. Among the very best secondary works are Anderson (1976), Boggs (1976), Cammett (1967), Fiori (1970), and Williams (1975). For primary sources see Gramsci (1954, 1957, 1973).

[3] Gramsci's views on worker-councils differ dramatically from many other enthusiasts of self-management, particularly those in the United States, in two important respects. First, is his position that worker-councils are incapable of completing the revolutionary task by themselves. It is striking that from the earliest days of his euphoria about the prospects of the councils, through the collapse of the Turin Council movement, to the writing of his main theoretical work in Mussolini's prisons, Gramsci always linked the councils to the larger struggle for socialism and to the necessary efforts of a vanguard party as general educator and coordinator. Second, Gramsci conceived of worker-councils as spontaneous organizations that arise out of struggles at the point of production—as natural proletarian institutions which are not only tools of class struggle but also the effect of such struggle. Councils are not, in short, formal organizational divisions created by unions, management, or government. Nor are they producer-cooperatives, the organizations which form the basis of the present study. In more contemporary times, similar positions on councils and the larger revolutionary movement have been taken by Gorz (1967), and Mandel (1970, 1973).

It might be helpful to present the preceding theoretical claims in the following schematic representation:


(1)
Direct governance of the production process by workers . . .
will lead to . . .
(2)
Transformation of individual orientations and behaviors appropriate to socialist arrangements . . .
which, in turn, will foster the . . .
(3)
Development, as industrial democracy spreads throughout society, of a class with revolutionary consciousness . . .
leading to . . .
(4)
Conscious class struggles for the institution of socialism.

The limited purpose of this article is to examine the linkage between (1) and (2) by comparing the effects on the basic orientations of those employed in worker-managed enterprises with the orientation of people whose productive work is confined to conventional, hierarchical enterprises. What the article cannot do is to examine either the link between (2) and (3) or between (3) and (4), which require an entirely different form of scholarly investigation.[4] Taken as a whole, however, should no relationship be found to exist between worker self-management and the transformation of values and behavior (linkage 1→2), then the case for industrial democracy as a tool for radical social change must be seriously doubted.

The Research Population and Data Collection

In most advanced industrial systems, the internal organization of productive enterprise more closely approximates authoritarian political forms than democratic ones, a generalization that holds for both capitalist and socialist societies, the democratic rhetoric on both sides notwithstanding. The most notable exceptions to this state of affairs in the United States have been producer-cooperatives. As the only large-scale industrial enterprises in the United States where the people who work in them are in complete control, producer-cooperatives offer an unusual and timely set of examples for examining some of the effects of cooperative productive labor from which we might cautiously generalize. While such cooperatives are not necessarily models for all of industry, nor sites where the aforementioned theories of the political left can be directly tested, they can serve to highlight basic questions about the democratic governance of industrial working life and its various effects. Cooperatives are, in short, useful sites in which to observe some of the outcomes of collective decision making within an individualistic market society.

Our research sites are locted within a rather remarkable concentration of 14 worker-owned and worker-managed industrial enterprises in the Pacific-Northwest plywood industry. Although elaborate descriptions of these firms already exist (Bellas, 1972; Berman, 1967; Bernstein, 1974; Greenberg, 1979), I will introduce them briefly. Most of the worker-owned plywood firms, which account for over 10 percent of all plywood production and are generally conceded to be the source of the industry's highest quality product, were founded in the years immediately following the Second World War. Several were started from scratch by lumber workers worried about the dismal employment prospects in their industry, several were the product of workers seeking to prevent the closing of a privately owned mill, and more than a few were created by promotors who took a healthy slice of initial profits for their efforts. Without exception, studies of the plywood industry indicate that the worker-owned firms, which range in size from 80 to 350 members, are characterized by higher levels of productivity than are conventional firms.

The data for this study were generated over a two-year period in three ways. First, overall descriptions and assessments of the operations of

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This article examines the status of individual attitudes and not class consciousness per se. It is not appropriate to simply aggregate individual attitudes to understand the latter, for class consciousness is an attribute of a class not of individuals. More concretely, class consciousness involves the formation not only of shared attitudes but also of communication networks, distinctive institutions, and politiccal organization among structurally located population groups. For the outstanding historical works that sensitively examine the formation of class consciousness in this sense, see Thompson (1963) and Trotsky (1959).

producer cooperatives and conventional plywood plants were gathered by means of interviews I conducted with people familiar with both the industry and/or particular companies. Second, I concluded in-depth interviews over a four-month period with 22 worker-owners in three producer-cooperatives; with 5 hired workers in these same companies; and with 11 workers in a neighboring, conventional, non-producer-cooperative plywood company, all with an eye towards gaining a richly textured sense of the particulars of the work experience, the social dynamics internal to the plants, and the felt effects of such an experience. Finally, I collected data from 551 people (worker-owners in four cooperatives, hired workers in these same companies, and workers in a single large, conventional, unionized plant)[5] by means of a lengthy mail questionnaire.[6] All of the companies involved are in close geographical proximity, are identical in terms of the technical organization of production, and are practically indistinguishable in terms of the demographic characteristics of the work force. That being the case, these firms provide an unusually well-defined scientific sample in which enterprises vary primarily with respect to the central variable or set of variables—the nature of the participatory experience—but in no other important respects. Such a sample of companies serves to isolate the effects of participation in decision making by diminishing the possibility that observed variations between the companies might be explained by differences in industrial processes, work routines, geographically specific cultures, work-force demographics, and the like.

The Governance of the Plywood Cooperatives

I am concerned with the effects of self-management on orientations and behavior relevant to social change rather than with the self-management process itself, which has been extensively elaborated elsewhere. Nevertheless, it is worth noting that the cooperatives are, in fact, truly self-governing institutions and not what some would call partial or pseudo-participatory institutions (see Bellas, 1972; Berman, 1967; Bernstein, 1974; Greenberg, 1979).

In worker-owned firms, ultimate authority and responsibility on *all* policy matters are vested in the general membership of the cooperative, expressed concretely in the annual or semiannual general membership meeting. While the membership in practice may choose to delegate many of its responsibilities to the general manager and/or the Board of Directors, in a formal sense the purview of the shareholders' meeting is unlimited, responsible as it is for the total governance and direction of the enterprise. If they so choose, shareholders at these meetings may decide to fire the manager and hire a new one, alter hourly wage rates, build a new plant, or do whatever else seems appropriate to the general membership. In such membership meetings, each shareholder is entitled to a single vote even if, as in a few exceptional cases, a member owns more than one share.

At these meetings, a board of directors is elected from among the shareholders, a body for which all members are eligible. This board is charged with general policy making in the interim between general membership meetings. The board is not entirely free in policy matters, however, since all of the firms require the board to gain permission from the general membership for any expenditures beyond some specified maximum figure.

Other features common to the formal organization of the plywood cooperatives are worth noting. All, for instance, operate on the principle of equal hourly wage rates among the shareholders no matter the number of years on the job or level of skill. All divide the annual surplus (the amount that remains from total income after hourly wage rates, operating costs, taxes, investment, and savings have been deducted) to the shareholders *as workers* rather than as dividends on capital. That is to say, nonworking shareholders are not entitled to a portion of the surplus. All hire a management team (a general manager and assistants) that is responsible for the day-to-day operations of the firm, subject to the guidance of the board of directors (which usually meets biweekly) and the suffrance of the general membership meeting. All insist that potential worker-owners spend a probationary period working in the plant and that their membership be formally accepted by either the general membership meeting or the board of directors.

The democratic character of the formal organization of the plywood cooperatives is enriched by the democratic and participatory social interactions in the everyday operations of these enterprises. Interviews focusing on internal social relations show the existence, for instance, of a strong sense of collective/mutual responsibility among co-op members as compared to conventionally organized firms. Co-op workers consistently demonstrate a significantly greater tendency to cooperate informally on production problems, to rotate jobs, and to create innovative solutions to production bottlenecks.

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[5] The union (control) plant that was selected is characterized by excellent non-management relations and relatively high morale as indicated by the absence of strikes over the past decade and the very high average tenure of its workforce (equal to that of the cooperatives).

[6] The return rate on the mail questionnaire was a surprisingly high 63 percent. Preliminary analysis of data gathered during the summer of 1978 from a sample of the remaining 37 percent shows no systematic variation on major variables in comparison with those in the main data set.

Profound differences between plant settings are also evident in the workers' relationship to their supervisors. The cooperatives are easily able to manage production with no more than *two* foremen per shift, and often with only *one,* while conventional plants often require *six* or *seven.* Since there are significantly fewer of them in the co-ops, supervisors are not "on the backs" of the production workers, but are forced to be concerned with broader, plant-wide issues having to do with the flow of materials and machine use.

Besides the general looseness and distance of supervision, a state of affairs that allows a great deal of free space for individual initiative and informal cooperation between production workers, there is also a marked contrast between cooperative and conventional mills in the style of interaction between supervisor and worker. In the conventional plant, communication is hierarchical and one-way, with orders coming from the top, and compliance (whether willing or unwilling, enthusiastic or begrudging) coming from below. In the producer cooperatives, communication is two-way, open, and relatively free-wheeling, characteristic of communications between relative equals.

Enterprise-level policy making is also marked by democratic social relations. One is immediately struck in the cooperatives by the degree to which information is widely available to the worker-owners and the extent to which working shareholders are generally informed about and interested in enterprise policy. Also noteworthy is the level of discussion about enterprise concerns. About 65 percent report that they regularly discuss production, finance, sales, and investment policies with other working shareholders. The cooperatives are also characterized by high attendance at shareholder meetings (about 95 percent) and active involvement of their members. Thirty-one percent of shareholders report that they "very often" or "always" participate in the discussions at shareholder meetings.

As to the board of directors, an elected representative institution comprised solely of working shareholders and in many respects the key decision-making body in the cooperatives, we find a continuation of the democratic social relations found in other operations of these enterprises. For one thing, no single clique seems to run the board. All are characterized by significant membership turnover. An impressively high number of shareholders, moreover, have run for the board (31.5 percent), have been elected and have served (17.5 percent). Most importantly, however, the board seems to avoid the problem of distance from the constituency so characteristic of representative institutions because of the inescapable fact that board members continue to hold jobs in the mill during their tenure and are completely accessible to the shareholders.

The Effects of Self-Management

It can be said without much hesitation, then, that the producer cooperatives in plywood are genuinely self-managed enterprises, being institutions in which all policies are subject to the control of the work force. Having established their credentials as institutions that embody most of the features lauded by theorists of industrial democracy and self-management, we now turn our attention to delineating the effects of these arrangements on the orientations and activities of those involved in them, particularly as they relate to the possibilities for social change. The data will demonstrate that the hopes of many leftist theorists in the educative possibilities of self-management operating within a capitalist-market economy are *not* fulfilled. Indeed, the data unambiguously indicate experiences within such enterprises have precisely the opposite results, cementing and firming-up the orientations and behaviors most appropriate to life in a market society.

Joining a Producer Cooperative

Perhaps the first key to understanding the social and political meanings of the cooperatives is to note the selection process at work. Almost without exception, people join the plywood cooperatives for financial rather than political reasons. That is to say, both in the interviews and in the questionnaire responses, the number of people reporting that they joined because of the work environment, or the nature of the relationships with supervisors and other workers, or to participate in the processes of industrial self-government, is conspicuously low. The primary motivation for joining the producer-cooperatives in plywood is connected, in one way or another, to the desire for financial gain and job security. It should be remembered, of course, that the people who join such cooperatives are distinctly working-class in origin, many of whom have had periods of economic difficulty in their lives, and have experienced significant periods of unemployment—47 percent have been unemployed, with 46 percent of that total experiencing over 5 months of unemployment. The fact that once one becomes a member of a cooperative, one is guaranteed a job if the firm remains in operation is no small attraction to people who worry about financial insecurity. Furthermore, given workers' limited ability to save and declining opportunities in the small-business sector, the producer-cooperative becomes one of the few outlets that working-class people have for making a small investment. Jobs and investment are, thus, the recurrent themes in workers' explanations for joining the plywood cooperatives. These themes are evident in the figures below which represent the responses of shareholders when asked to judge the degree of importance of the following factors in joining a cooperative. Table 1 shows that financial investment is the strongest motivation for joining a cooperative, closely followed by the importance of good wages and job security; conversely, those reasons related to the internal governance and political processes of the cooperatives make a dismal showing.

These figures are strongly buttressed by interview responses from shareholders. Not a single respondent, spontaneously and without prompting, mentioned even vaguely political or ideological grounds for joining these enterprises. Almost all were quick to mention the attractions of financial investment and job security, a surprisingly large number mentioned the desire to escape labor union/company tensions and uncertainties, and not a few seemed to have accidentally wandered in for no particular reason. Political motivations were notable for their absence:

> Buying a job . . . it was a job. The plant's running 7 days a week, 12 hours a day ever since I've been here. So, if a guy wanted to really get in to work, he could make all the overtime he wanted. So he could actually bring home a pretty good check. . . .
>
> Well, I thought I wanted to buy into a small business . . . be my own boss. And my brother-in-law convinced me that would take too long and it would be more risky than [this].

What is implied in the above comments is that a rather special stratum of the blue-collar working class finds its way into the cooperatives, namely, those most oriented to the world of business and those financially able to make a down payment on a share. This hint that cooperative membership might be characterized by a small-business or petit-bourgeois[7] background and outlook is confirmed by several additional pieces of information. First, when compared to workers in a conventional plywood plant, shareholders are significantly more likely to have owned a small business in the past or to own one concurrently with their present work. Furthermore, this more pronounced small-business linkage exists among the newest members of cooperatives when compared to workers in the conventional plant (Gamma = .45 for those working in their present firms for four years or less), suggesting that those who *enter* into the former are a particular element of the working class. Another indicator of the small-business tendency in the cooperatives is the slight propensity for their membership to identify with the Republican party, traditionally the party of *small-business* ideology in the United States. To be sure, given the distance that most respondents feel themselves to be from the political system as a whole, the data on party identification, taken

Table 1. Reasons for Becoming a Shareholder in a Producer-Cooperative

Question: "How important were each of the following in your decision to *first* become a shareholder in this cooperative?"

	Percent Answering "*Very* Important"
Heard that workers in the plant got along well with each other	17.9
Heard that supervisors and workers got along well	17.1
Thought it would be a good financial investment	66.4
Wanted an opportunity to help run a company	12.5
Wanted a guaranteed job in case times got hard	42.1
The wages were good	48.2
There were no other jobs available at the time	15.0
Total = 280	

Source: Collected and compiled by the author.

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[7] In its pure form, the petite bourgeoisie is that class centered in the simple-commodity sector of the economy. In contrast to both the bourgeoisie and the proletariat, it own property, produces goods and services directly but does not hire and live off the labor of others. Politically it has traditionally been characterized by its support for a free market of small producers and its hostility to both labor unions and giant corporations. In this article, the term "petite bourgeoisie" will be used in this technical sense, not in the frequently encountered perjorative sense.

alone, are not overwhelming. Nevertheless, combined with the information on small-business ownership, it is suggestive of a form of small-business mentality among this section of the working class. This impression is given substance by the class identifications of the respective work forces. Among a sample that agreed to the proposition that social classes exist in the United States, cooperative shareholders were significantly more likely than workers in the conventional plant to conceive of themselves as middle-class, a tendency that is most pronounced among the newest members (Gamma = .35 for those working in their present firms for four years or less).

There is also some indication that the people who choose to join the plywood cooperatives are a more generally satisfied lot than others. On an open-ended question asking respondents what changes they would like to see made in the United States, among the newest members of each of the types of enterprises, shareholders were almost twice as likely to give totally supportive responses (over 20 percent of the sample) than were workers in conventional firms. Furthermore, among these newcomers, shareholders were significantly more likely to agree with the proposition (Gamma = .29) that "Our current economic system gives almost everyone a fair chance to get what they need to live a decent life."

The Effects of Industrial Self-Management

The cooperatives, relative to conventional firms, thus tend to attract members from what must be seen as a particular stratum of the blue-collar working class, that stratum characterized by a positive attraction to the market system and by a desire to leave the ranks of wage-earners and to enter the more privileged world of small property holders. Nevertheless, whatever workers' reasons for joining a producer cooperative, shared cooperative social relations at work might have unintended consequences not anticipated by those who join such enterprises. This is certainly the position taken by almost all of the theorists of industrial democracy, e.g., that the intentions of involved individuals are irrelevant. What matters for the development of particular attitudes among workers (whether they favor greater democratic participation or greater support for cooperative social relations) is the actual experience of cooperative labor.

In fact, the in-depth interviews with workers and observations at the plants give some indication that such unintended consequences might indeed result from working in the producer-cooperatives. It has already been pointed out, for instance, that shareholders are more likely, compared to workers in conventional companies, to enter into informal cooperative arrangements in activities that range from interacting in general shareholder meetings to mutually working out production problems that invariably arise during the course of building plywood panels. I have also discussed the impressive degree to which a tone of mutuality and group responsibility pervades these unique enterprises. Also worth noting is the existence of equal hourly wage rates within the cooperatives and the very strong, almost universal support for this form of compensation among shareholders. However, the overwhelming impression from the data that follows is that the experience of self-management within enterprises oriented to sales in a competitive marketplace is to enhance and nurture the small-property/petit-bourgeois orientations that shareholders bring with them upon joining. What this suggests is that the cooperative social relations that do exist are entered into and maintained for the pursuit of small-property ends. In this respect, those theorists of industrial democracy who see self-management as an important school for the development of orien-

Table 2. Percentage of Workers Having Owned a Business

Question: "Other than owning a share or a piece of stock in a company, have you ever owned a business of your own?"

	Shareholders in Cooperatives	Workers in Conventional Union Company
Owned a business previously but not now	17.6	7.0
Now own a business	6.8	2.3
Never owned a business	75.6	90.7
N =	250	129
Gamma =	.50	

Source: Collected and compiled by the author.

Table 3. Party Identification among Plywood Workers (Percent)

Question 1: "Generally speaking, do you usually think of yourself as a Republican, a Democrat, or an Independent?"

	Shareholders in Cooperatives	Workers in Conventional Union Company	
Republican	10.5	5.4	Gamma = .05
Democrat	47.8	54.3	
Independent	33.6	30.2	
Unsure/Other	8.1	10.1	
N =	247	129	

Question 2: "If you chose Independent, do you consider yourself as closer to the Republican or Democratic party?"

	Shareholders in Cooperatives	Workers in Conventional Union Company	
Republican	22.4	10.0	Gamma = .11
Democratic	38.8	52.5	
Neither	38.8	37.5	
N =	85	40	

Source: Collected and compiled by the author.

tations favorable to socialism would seem to be very wide of the mark, indeed.

Perhaps the most direct way to see this is to compare shareholders and workers in conventional companies on their support for classical liberal values, that set of values which arose with the appearance of market society. These values have been described as those of "possessive individualism," or "competitive individualism," and entail the familiar notions that each individual is responsible for his/her own fate in a perpetual competition for rewards against others in society, that success in the competition for rewards is justly arrived at through the marketplace, and that those who succeed owe nothing to those who fail in this competition, especially through the intervention of government. In order to tap this value dimension, I constructed an index of classical liberalism using responses to the following statements:[8]

Table 4. Class Identification among Plywood Workers (Percent)

Question: "If you agree that there are social classes in America, do you generally think of yourself as working-class, middle-class, or upper-class?"

	Shareholders in Cooperatives	Workers in Conventional Union Company
Working-class	54.5	66.7
Middle-class	45.5	33.3
Upper-class	0.0	0.0
N =	231	117
Gamma =	.25	

Source: Collected and compiled by the author.

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[8] A note on methodology is in order at this point about each of the indices which follows. Each is based on responses to items in the mail questionnaire. Each is a simple additive index based upon five response Likert-type items (from *strongly agree* through *strongly disagree*). Items were selected for each index by subjecting pools of potential items with evident surface similarity to factor analysis procedures (principal factor with iteration, varimax rotation). Once items falling along a unidimensional scale were identified, indices were constructed by summing the responses and recoding the

1. Many people are poor today because they are really not willing to work.
2. When people don't succeed financially, it's almost always their own fault and not society's.
3. A person should basically depend on himself and not ask or expect the help of others.
4. Most people wouldn't work if they didn't need the money for eating and living.
5. A person's first duty is to himself and his family.

Several things are immediately evident with regard to Table 5. First, both workers in co-ops and workers in the conventional firm strongly affirm classical liberal values, suggesting that support for competitive individualism may be so pervasive in American life that it even includes the great mass of the blue-collar working class. This finding tends to confirm those of a number of scholars (Lane, 1962; Sennett and Cobb, 1973; Terkel, 1974; Wills, 1970) and suggests the distance that must be traveled by those who perceive of the possibility of a political movement of the left in the United States in the near future. A second finding, most important for the immediate purposes of this article, is that shareholders in cooperatives tend to affirm classical liberal values more strongly than their counterparts in conventional work, though the relationship is weak.

This relationship is unaffected by controlling for time on the job. That is, the overall Gamma (.22) is not changed by introducing a control for job tenure (first-order Gamma = .22). The tendency for shareholders to hold more strongly to classical liberal values is evident throughout the entire range. Thus the overall (weak) relationship reported in Table 5 is given substance by its consistency throughout the entire range of work experience. That is to say, the experience of self-management (as it is practiced in the plywood cooperatives) does not, whether taken in isolation or as compared to work in conventional enterprises, serve to undermine or diminish the commitment to classical liberal values, as most theorists of industrial democracy have speculated. The persistence of the belief in a world characterized by self-interested struggles for survival in a group experiencing shared-cooperative productive relations must certainly give us pause.

These findings are given added emphasis by the results of an index designed to measure the belief in equality among workers. For many theorists of industrial democracy, one of the principal processes by which societal transformation is encouraged is through the generalization of beliefs in human equality gained through the cooperative-egalitarian setting of the self-managed enterprise to other spheres of life. The following measurement of egalitarian beliefs does not support the hypothesis, however. An index was created by summing the responses to the following questionnaire items:

1. A group of equals will work a lot better than a group with a strong boss.
2. Each person should get what he needs; . . . the things we produce as a society belong to all of us.

Table 5. Indices of the Sociopolitical Orientations of Plywood Workers (Percent)

	Shareholders in Cooperatives	Workers in Conventional Union Company	
1. Index of Classical Liberalism			
Lowest (5-12)	2.0	3.2	
Neutral (13-17)	30.6	40.0	Gamma = .22
Highest (18-22)	67.4	56.8	
2. Index of Egalitarianism			
Lowest (3-7)	26.3	20.2	
Neutral (8-10)	52.9	46.8	Gamma = .23
Highest (11-15	20.9	33.0	
3. Index of Support for the Welfare State			
Lowest (6-14)	38.5	15.2	
Neutral (15-21)	51.2	63.2	Gamma = .46
Highest (22-30)	10.3	21.6	

Source: Collected and compiled by the author.

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final scores into 5 categories corresponding to the Likert pattern. Comparisons were then run on each index contrasting respondents in the producer-cooperatives with those in the conventional plant.

3. For society to run better, the really talented people should be rewarded more than others.

The rather startling finding reported in Table 5 is that workers in conventional plants organized in traditional hierarchical ways are more likely to affirm general egalitarian values than shareholders in the plywood cooperatives where work is carried out under general conditions of equality. That is to say, the experience of egalitarian relationships in the cooperatives is not generalized by shareholders so as to incorporate the world beyond the walls of the enterprise. Again, while the strength of the relationship is not overwhelming, it is given added strength by its consistency with the results on the index of classical liberalism. The tendency to reject or to not affirm general egalitarian values is perfectly consistent with a belief in the reality and benefits of the competitive struggle. Furthermore, as with the previous index, the difference between the two groups holds throughout the entire range of job tenure (first-order Gamma = .25).

This finding that the effects of the self-management experience do not scale the walls of the enterprise to incorporate persons and institutions on the outside, that the general small-property outlooks that shareholders bring with them to the cooperative relative to the world around them are not diminished by virtue of their experience, is given further weight by responses to a question asking whether or not wages throughout American industry should be made as equal as possible. Recall that shareholders are themselves paid in an egalitarian fashion, and that to affirm this question would in some ways represent but a simple generalization of their own experience, one that has proved quite successful. Surprisingly, workers in a conventional firm proved somewhat more likely to affirm the more egalitarian position on wages than shareholders. (The relationship holds throughout the entire range of job tenure.) Again, while the relationship is not overwhelmingly powerful, its consistency with the previous indices on classical liberal and egalitarian values is noteworthy.

This general picture of a small-property/petit-bourgeois orientation among shareholders in the plywood cooperatives is given further weight by workers' responses to an index that seeks to measure reactions to the welfare state. While none of the particular items in the index measures reactions to public policies constitutive of socialism per se, the following questionnaire items all relate to public policies whereby government serves as a tool for egalitarian redistribution, for public control of the economy, and for the general health and welfare of the citizenry. It is a measure, if you will, of support for policies at the left wing of the modern liberal welfare state, and as such, for policies closely related to a more egalitarian/publicly controlled social order. The questions which made up the index are as follows:

1. Should the federal government own and operate key industries?
2. Should the federal government control how much profit a company can make in any one year?
3. Should the federal government pay for medical and hospital bills?
4. Should the federal government control the prices set by large corporations?
5. Should the federal government control big corporations so they would better serve the public interest?

The results, as reported in Table 5, are unambiguous: shareholders are much more likely to reject the public policies of the welfare state, to take the more conservative (classical-liberal), small-property position than are workers in the conventional industrial enterprise. Once again, these differences hold throughout the entire range of job tenure. The initial differences between worker-shareholders and workers in the conventional firm

Table 6. Workers' Opinions Regarding the Equalization of Industrial Wages and Salaries (Percent)

Question: "Some people have suggested that wages and salaries throughout American Industry be made as equal as possible. How do you feel about this suggestion?"

	Shareholders in Cooperatives	Workers in Conventional Union Company	
Agree and strongly agree	34.1	39.5	
Neither agree nor disagree	11.5	16.3	Gamma = .15
Disagree and strongly disagree	54.3	44.2	

Source: Collected and compiled by the author.

on their support for welfare state policies do not disappear over the years but are strongly maintained.

One of the hypotheses about the effects of industrial democracy advanced by both its friends and enemies is that the alteration of the traditional form of hierarchical work organization causes workers to question authority relationships in general. That is to say, according to the theory, under conditions of full self-management, workers not only come to see that hierarchical authority relationships at the point of production are superfluous, but come to generalize that view to the level of both the firm and society outside of the walls of the firm. To advocates of social change on the left, this possible social dynamic remains one of the most exciting possibilities of industrial democracy because it both dissolves the bonds to the status quo and forms the nucleus for a new kind of society. To defenders of the status quo, this dynamic forms the contours of a social nightmare (Crozier, 1975). We shall see that conservatives have little cause to worry.

In an attempt to tap the effects of the work experience on general orientations to authority, I constructed an index based on the following statements:

1. People in positions of authority are owed our highest respect.
2. Patriotism and loyalty are the most important requirements of a good citizen.
3. People should maintain their respect for the police even when the police are clearly in the wrong.

A comparison of shareholders in cooperatives and of workers in a conventional firm show almost no differences worth considering (Gamma = .03), a somewhat surprising finding in light of the nonhierarchical productive relationships found in the former. That is to say, the experience with authority relations is *not* transferred beyond the walls of the firm.[9]

Thus far, we have confined our attention to the possible effects of the experience of self-management on fairly abstract or general attitudes of the population of the producer-cooperatives. What we have found is that the cooperative work experience is either not sufficiently profound or relevant to certain domains (attitudes toward authority, government, the economy) to show any differences between worker-shareholders and workers in conventional settings, or tends to have effects opposite to those predicted by most advocates of industrial democracy (such as attitudes toward class identification, classical liberalism, egalitarianism, the welfare state). That is to say, with respect to general politico-social orientations, the experience of self-management in the producer-cooperatives has no effect in some cases on the general orientations typical of the blue-collar working class, and in others, seems to produce orientations even less conducive to societal change in an egalitarian/communitarian direction.

What is even more startling, however, are the findings that show no significant effects in relationship to social processes more directly derived from everyday experiences in the producer-cooperatives. According to many advocates of industrial democracy and social change, one of the most likely results of worker self-management is that workers should gain increased confidence in their fellows. One is supposed to find, in a kind of ascending dynamic, an initial confidence in those immediately around one spreading outward to eventually include the entire working class. According to this view, having seen that ordinary working people are capable of running all of the affairs of their own enterprises, workers gradually come to believe in the ability of ordinary working people to run the entire society.

The data in this study cast grave doubts on this theoretical dynamic. In order to test the effect of self-management on orientations to the blue-collar working class in general, I asked respondents to assess the general managerial capabilities of other working people. Despite the fact that worker-shareholders (who themselves are no better trained or more skilled than other working people) have been competently, skillfully, and profitably running their own industrial enterprises, this experience does not seem to increase their confidence (the relationship holds throughout the entire range of job tenure), relative to people in conventional companies, in the abilities of other working people to do the same thing in their own firms, although a slight plurality of shareholders is willing to affirm this proposition. That is, their own direct experience is not generalized to the blue-collar class taken in its entirety.

When we turn to a question that asks about confidence in the ability of working people to run

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[9] Given this finding that workers' direct experience with a different form of authority at work is not transferred to their general orientations toward authority outside of the factory walls, one should not be too surprised to learn that the experience of self-management in the producer-cooperative does not seem to affect orientations toward business (on indices of corporate power and between worker-shareholders and workers in the conventional firm shows no differences in orientations toward government (on indices of corporate power and corporate role in society) and the economy (indices on the efficiency and fairness of the economy). Limitations of space preclude detailed reports of these findings.

Table 7. Plywood Workers' Attitudes about the Role of Workers in Society (Percent)

	Shareholders in Cooperatives	Workers in Conventional Union Company	
1. Even without the help of managerial personnel, working people have enough skill and intelligence to run everyday production in their plants.			
Strongly agree	10.3	12.3	
Agree	34.5	33.8	
Neither agree nor disagree	13.9	16.2	Gamma = .07
Disagree	29.8	30.8	
Strongly disagree	11.5	6.9	
2. Even without the help of managerial personnel, working people have enough skill and intelligence to run the business and financial affairs of their plants.			
Strongly agree	2.0	2.4	
Agree	8.7	10.9	
Neither agree nor disagree	9.1	21.1	Gamma = .26
Disagree	55.7	50.0	
Strongly disagree	24.5	15.6	
3. Except for issues like wages and benefits, workers in conventional, privately owned plants should have little to say about running their plants. Those who own companies should run them.			
Strongly agree	13.5	10.9	
Agree	40.9	37.5	
Neither agree nor disagree	14.7	18.0	Gamma = .08
Disagree	25.8	28.1	
Strongly disagree	5.2	5.5	
N =	252	128	

Source: Collected and compiled by the author.

not merely the technical, production side of enterprises but also the business affiars of enterprises, activities even more distant from the direct experience of most working people, we find the above findings further strengthened (see Table 7). Not surprisingly, of course, most respondents tend to discount the abilities of their fellows. What is surprising, however, is the degree to which workers in conventional companies are slightly *more confident* in other members of the working class than are worker-shareholders who actually participate in the business affairs of their enterprises, enterprises which have proved quite successful in a business sense.

Finally, all of these findings about workers' orientations toward other blue-collar workers are further confirmed by a question that asks directly about the possibility of the dissemination of forms of industrial democracy in the American economy (see Table 7). Besides the interesting finding that neither group in our sample seems strongly supportive of industrial democracy, what is even more to the point is that experience in a self-managed enterprise does not enhance such support relative to other work experiences.

Conclusion

The expectation held by many theorists of industrial democracy that self-managed work environments might serve to nurture feelings of cooperation, equality, generosity, and self-confidence in one's fellows is only partly met within the plywood producer cooperatives. The expectation that such feelings would spill over the walls of the workplace so as to incorporate society, economy, and government is decidedly *not* met in the cases here under review. Indeed, the findings point to the opposite results. With respect to the outside world, the producer cooperatives seem to nurture outlooks characterized not by community, mutuality, equality and confidence in others, but outlooks more congruent with the tenets of classical liberalism: those of individualism, competition, limited government, equality of opportunity and inequality of condition, and so on. The data indicate that those entering the cooperatives bring with them a small-property/petit-bourgeois experience and outlook, and that tenure in the cooperatives serves both to maintain some elements of this orientation and to enhance others.[10] The cooperatives thus seem to take people with attitudes appropriate to a market economy and nurture them further in this regard. It would seem, then, at least with respect to the cases reviewed here, that self-managed enterprises operating within a system of market capitalism seem unlikely settings not only for the practice of socialist relationships but especially for the promulgation of general attitudes appropriate to a more egalitarian/cooperative society.

The producer cooperatives in plywood represent the most fully developed self-managed enterprises existing in the United States today. As such, they represent perhaps the best setting in which to observe the effects on workers' political and social outlooks of the experience of industrial democracy in enterprises operating within an overall market system. What the findings seem to suggest is that the *market* is a more powerful educative tool than is the cooperative experience itself. Or to put it differently, so long as the economic well-being of enterprise members depends upon success in the marketplace, whether it be based on ownership of shares, or on profit sharing, bonuses, or other such devices, behavior appropriate to that success will be encouraged even within self-managed enterprises.[11] This is especially true in settings where a political organization devoted to general coordination and socialist education (in the Gramscian sense) does not exist. One gets the strange result, then, of a collective/cooperative/egalitarian spirit that is somewhat encouraged within the enterprise walls running up against the harsh rocks and shoals of the marketplace. What this should further suggest to theorists of the left is that while industrial self-management is surely the model by which a decent and humane society will organize its productive life in the future, the worker-managed workplace may not be, isolated as it is, an appropriate educative setting in the present context for nurturing a larger political movement for change.

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[10] The argument that the outlook of shareholders is best characterized as a small-property/petit-bourgeois orientation is further affirmed by findings that indicate the existence of a strongly anti-corporation, anti-big government outlook among worker-shareholders. On an index designed to tap attitudes about large corporations among shareholders, less than 10 percent responded positively while well over 50 percent responded negatively. Other indices demonstrate strong distaste for government intervention and initiatives in economic and social affairs. The sample almost universally opposes current tax and spending levels. This complex combination of attitudes about individualism, competition, government, and concentrated economic power is the essence of classical liberalism, what we would today call conservatism.

[11] It might be argued that producer-cooperatives, because they are enterprises owned by workers, represent a special case and are therefore inappropriate for the examination of the theoretical issues raised here. To be sure, producer-cooperatives are by no stretch of the imagination worker-councils in the Gramsci, Mandel, and Gorz sense. Nevertheless, producer-cooperatives can give us an opening wedge in the understanding of some of the effects of cooperative labor in a distinctly hostile setting. Indeed, except for cases in which full-scale revolution is in progress, it is difficult to imagine enduring cases of self-management in industry which can in actuality or in theory escape the inherent logic of producer-cooperatives, e.g., collective control of enterprise productive apparatus with an eye towards profit making in the marketplace. What we have found to be the case in terms of attitude development in the producer-cooperatives, then, must surely hold to some degree in any self-management arrangement situated in capitalist market societies.

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Political Efficacy at Work: The Connection between More Autonomous Forms of Workplace Organization and a More Participatory Politics

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Carole Pateman (1970) argued that democratized industrial authority structures would require workers to develop precisely the skills and resources necessary for participation in political life beyond the workplace. She based her argument on a close analysis of classical democratic theory and the indirect empirical evidence available at the time concerning workplace-related political participation. Although a great deal of research in industrial psychology and sociology and organization behavior is available, none treats expressly political correlates of workplace democracy. Analysis of data from a new factory designed on the basis of semi-autonomous, self-managing work groups provides empirical evidence directly supporting Pateman. The analysis also results in empirically grounded scales that differentiate between work humanization *and* work democratization. *The latter correlates significantly with a sense of political efficacy. Further research to support democratizing industrial authority structures seems warranted for a political science that would contribute to a more participatory politics.*

Prior to Carole Pateman's *Participation and Democratic Theory* (1970), political scientists paid scant attention to workplace experience as a potential source of political learning (Almond and Verba, 1963; Lipsitz, 1964, being notable exceptions). Pateman carefully reviews classical democratic theorists (including J. S. Mill and G. D. H. Cole) and the substantial empirical literature on participation in organizational decision making (relying, in part, on Blumberg, 1968) to support her argument that industrial work resting on an undemocratic authority structure socializes people into passivity and political apathy. She suggests in her concluding chapter that the redesign of workplace authority structures following democratic principles would spill over beyond the workplace to reinforce greater participatory democracy instead of democratic elitism.

One of Pateman's major contributions is her definition of work organization as a possible determinant of political behavior. Hers is the first political science assessment of the quality of work life (QWL) in the context of democratic theory. However, despite her comprehensive assessment of the empirical evidence on participation in workplace decision making and evidence on the sense of political efficacy, she could not produce any direct empirical evidence linking the two. As Ambrecht notes (1975, p. 19), the "problem with Pateman's analysis is that she assumes away precisely that which needs to be investigated empirically: the link between participation in the workplace and participation in political life." The main purpose of this article is to report empirical research on this link.

Pateman's problem, of course, was that at the time of her analysis (the late 1960s) there was almost no empirical evidence concerning such a possible linkage, as she herself points out (1970, pp. 106-07):

> The major difficulty in a discussion of the empirical possibilities of democratizing industrial authority structures is that we do not have sufficient information on a participatory system . . . to test some of the arguments of the participatory theory of democracy satisfactorily.

This difficulty may be a result of the interdisciplinary nature of the problem. Political scientists and political sociologists interested in participation tend to define it in relation to the electoral system. They use occupational titles rather than looking inside organizations at the innards of QWL—concrete tasks and workplace relations—even when their subject is the relation between work and politics (e.g., Allardt, 1976). Conversely, psychologists, sociologists, and organizational researchers have produced an enormous body of research and theory about the inner dynamics of work organizations without paying the least attention to possible political ramifications outside the organization. Hence, another purpose here is to deepen and continue Pateman's definition of workplace political socialization as a political science problem.

Pateman's argument rests on a process of politically relevant adult social learning in the workplace. Participation is both the structure and the glue that holds the structure together. She sees the workplace as a potential training ground for acquiring participatory skills and resources, provided that it is organized in a participatory way. Work so organized would induce workers to develop the same skills, resources, and values that research on political efficacy has shown to be necessary for political participation in the larger society. Unfortunately, what Pateman means by a "participatory" work organization is not completely clear.

Pateman's analysis of classical democratic theorists shows "that we learn to participate by participating and that feelings of political efficacy are more likely to be developed in a participatory environment" (1970, p. 105). However, Pateman does not exactly define a "participatory environment" in the workplace. Students of organization and the quality of working life (QWL) have identified, studied, and manipulated work organization variables galore. Which of these contribute to a "participatory work environment"?

We cannot simply pull the necessary scales out of the hat of contemporary political science research. Nor can we uncritically take over scales and theories from the multitudinous literature in industrial organization, organizational psychology and sociology, etc., which is more concerned with productivity and job satisfaction than self-management and work democracy. Thus a third purpose of this article is to identify, both theoretically and empirically, indices that define a "participatory work environment" (what we call work democracy) and its politically relevant correlates.

We can summarize the argument in a simple model (Table 1). The critical issue is whether workplace experiences connect with political life. If empirical evidence linking Steps 2 and 3 is not forthcoming, Pateman's argument is seriously weakened. The link between Steps 1 and 2 is essential to establish that, in fact, a democratic work structure exists (Step 1) and has workplace consequences (Step 2).

Organization Design Concepts Necessary for Work Democracy

The first prerequisite of the model is obviously the design of a democratic form of work organization. Pateman's analysis of the participatory management literature failed to produce much empirical data based on work situations where workers actually made the final decisions concerning the management of their own work. But she found reason for optimism concerning future developments (1970, p. 105):

> The analysis of participation in the industrial context had made it clear that only a relatively minor modification of existing authority structures may be necessary for the development of a sense of political efficacy. It is quite conceivable, given recent theories of management, that partial participation at the lower level may become widespread in well-run enterprises in the future because of the multiplicity of advantages it appears to bring for efficiency and the capacity of the enterprise to adapt to changing circumstances.

Pateman's optimism seems to have been more than justified. The 1970s saw a virtual explosion of new theories of work organization under the generic name of QWL, which led to literally thousands of experiments in increased worker self-management. A comprehensive review is beyond my scope here, and several such reviews are already available. (In addition to the literature reviewed in Elden, 1975, see, for example, Cummings and Malloy, 1977; Cooper and Mumford, 1979; International Labor Office [ILO], 1979; and International Council for the Quality of Working Life [ICQWL], 1979.)

We will therefore only summarize some of the important concepts of organizational design as they have evolved over the last two decades. We limit our description primarily to work redesign experiments that aim at something more than merely a more efficient and flexible work organization. Pateman's democratized workplace clearly calls for something more than job enlargement

Table 1. A Simplified Model of Work Democracy and Its Organizational and Political Correlates

Step 1. Work Democracy	→	*Step 2. Positive Quality of Work Life (QWL) Consequences*	→	*Step 3. Political Consequences beyond the Workplace*
(production is organized around self-managing autonomous groups)		(self-managed workplace system results in increased levels of satisfaction, social appreciation, learning, etc., and becomes self-maintaining)		(politically relevant QWL factors connected with increased sense of political efficacy, politically relevant activity and better citizenship)

Source: Formulated by the author.

or participatory (i.e., "friendly") management. The key is a total redesign of the workplace so that workers control directly (instead of merely being consulted about) immediate workplace decision making.

In a democratic work organization, workers instead of managers make decisions about daily work schedules (who works when and where, doing what), personnel administration (hiring, training, rewarding, and disciplining) and those aspects of coordination most immediately affecting the flow of work for which the group is responsible (production management). This is something quite different from human relations training for supervisors. There is no place in a self-managed work group, for example, for the traditional role of a supervisor as "boss."

Organizational design principles to create precisely these democratic conditions in the workplace were first developed by researchers at the Tavistock Institute in London in the 1950s (see Herbst, 1962, 1976; Trist et al., 1973; and Emery, 1959, 1977; a more complete description is presented in Elden, 1976). They developed "sociotechnical" design principles that build on aggregated, "unit" tasks which include management and supervision functions and are performed by a self-managing or "autonomous" work group. The findings of the Tavistock researchers, originally derived from studies in coal mines, have been proven feasible under a wide variety of technological and organizational conditions. Even assembly lines in automobile manufacturing plants no longer seem inevitable as experiments at Volvo (Aguren et al., 1976), Volkswagen (Granel, 1979), and now General Motors (Guest, 1979) have demonstrated. These design principles have also been applied in the service of expressly democratizing change of the type postulated by Pateman. They were first applied in collaboration with Tavistock researchers in a series of government-sponsored shop-floor industrial democracy experiments in Norway in the mid-1960s (see Emery and Thorsrud, 1976, and Elden, 1979, for recent summaries). In the late 1960s and early 1970s they were applied in literally hundreds of experiments in Sweden alone, as well as in Europe and the United States.[1] Not all of this now-extensive literature reports on experiments that, like the Norwegian and a number of Swedish cases, were expressly designed to *democratize* working life. We return to the question of workplace democratization in constructing the scales necessary for an empirical assessment of Pateman's argument (operationalizing Steps 1 and 2 in Table 1).

We should make clear, however, that democracy at the workplace level—i.e., "work democracy"—is but a part of organizational or "industrial democracy," what Pateman terms "lower"—rather than "higher"—level participation. This distinction is quite common in the literature concerning the democratization of enterprises where direct (i.e., "participatory") democracy is distinguished from indirect (i.e., "representative") democracy. The former requires a completely new way of thinking about the design of work. The latter requires restructuring an organization's policy-making processes (e.g., worker councils, workers serving on boards of directors, worker ownership, etc.—see Elden, 1976, for a complete discussion). These two approaches are aimed at quite different problems, but, as Pateman argues (pp. 106-07), they should ideally be developed at the same time in the same company so as to reinforce each other. She also makes clear that the political socialization resulting from participation requires the redesign of work organization that affects everyone's everyday working life. Policy-level participation by a few elected representatives would not be sufficient.

In Table 1 we identified critical linkages in a simple three-step model. On the basis of our brief review of the QWL organizational design literature concerning self-managing work groups, we can now extend the model. The key, following Pateman's argument, is to move from hierarchical/elitist to self-managing/democratic authority structures so that social learning results in an increased sense of political efficacy rather than feelings of apathy and alienation. In Table 2 we elaborate the basic elements of our model and illustrate what kind of political learning we would hypothesize under the two different organization designs.

Even if we follow Pateman and build on the recent literature on work democracy experiments, we soon discover that virtually none of these explores the possible social learning effects of potential political relevance beyond the workplace. The study reported here is an exception. Our data were acquired in a new plant expressly designed according to QWL organization design principles (autonomous work groups). The express intention was to create a non-hierarchical authority structure where workers would make important decisions in managing their own work.

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[1] The Norwegians, followed by the Swedes, are generally considered to be pioneers in the theory and practice of work democracy. See Elden (1979) for a summary and analysis of the Norwegian experience. The Swedish Employers Confederation (1975) summarizes experiences from some 500 "experiments" while Stymne (1980) provides a more current and in-depth analysis of long-term government-sponsored work-democratization experiments in 5 companies. For collections of cases and studies of the field in Europe, see for example, Cooper and Mumford (1979); International Labor Office (ILO, 1979); ICQWL (1979); and Jenkins (1973, 1975). For America see, for example, Cummings and Malloy (1977); Davis and Cherns (1975); Davis and Taylor (1979); Hackman and Suttle (1977); and O'Toole (1973).

Data Sources and Methodology

As more fully described in Elden (1976), the plant in which our data were collected, located several hours' drive from a major metropolitan area on the west coast of the United States, was built around a highly automated (i.e., relatively capital-intensive) process for producing a paper-based consumer product. The automated machinery was arranged in 5 parallel lines, each managed by a group of approximately 15 workers on a three-shift basis 5 days a week. Thus at the time of this particular study, approximately 6 months after the factory started production, there were 225 employees.

This plant was one of several owned by the same parent company in different parts of the country. It was new and designed quite differently from the others. In the other plants there was a traditional work organization with supervisors, inspectors, and relatively highly skilled maintenance technicians. In the experimental plant, workers formed self-managing groups which took over managerial and specialist roles. Each group, in addition to controlling a set of production lines, contributed staff to the warehouse and to maintenance. The group met 30 minutes before

Table 2. Illustrations of the Three-Step Model under Two Types of Workplace Authority Structures

Elements in the Basic Steps of the Model	Authority Hierarchical/Elitist	Structure Self-Managed/Democratic
Step 1: Design of Work Organization		
1.1 Basic building block	A single individual limited to a single job (i.e., "One-man, one job")	A group of individuals responsible for a variety of jobs (i.e., "group work")
1.2 Flow of control-relevant information	Vertical, formal, and external to workplace	Highly horizontal, informal and within the work group
1.3 Locus of control of work flow variances	"Higher authority"	The work group as a whole
1.4 Job design emphasis	–fragmentation –repetition –social isolation –low choice –short-term training	–integration –variety –team work –decision making –continual learning
1.5 Supervisor's role	Full time, "boss"-policeman	Part-time, "consultant"-teacher
Step 2: QWL Consequences		
2.1 Motivation	Calculative, instrumental (money, security, status)	Integrative, expressive (competence, learning, craftsmanship)
2.2 Orientation to work	Extrinsic, individual	Intrinsic, collective
2.3 Most likely social learning	Deference to and dependence on organizational supervisors, fatalism, passivity	Dependence on self and team mates, self-reliance, initiative
2.4 Support for self-management	Low and inconsistent	High and stable (self-maintaining)
Step 3: Political Consequences		
3.1 Sense of political efficacy	Low	High
3.2 Feeling of personal potency	High fate control	Low fate control
3.3 Social participation	Little and passive	Much and active
3.4 Political system implications	Democratic elitism	Participatory democracy

Source: Formulated by the author.

going on shift to decide issues like who would work where. Workers with extra experience and training in one area trained fellow workers through a system of job rotation managed by the team. In addition, teams set production goals and participated in plant-wide annual budget forecasting.

They did not, however, control *what* was produced, as marketing and finances were controlled by corporate headquarters. These was no union. Nevertheless, the plant management was clearly attempting to develop a more democratic form of work organization explicitly following concepts from socio-technical theory. (McWhinney, 1972, describes the application of these concepts in the plant.) During the first year of operation these concepts were partially realized. Because of technical difficulties, each team started with a temporary manager. However, by the time survey data were acquired, midway through the first year, a majority of work groups had only part-time managers who functioned as consultants, not bosses. In addition, four groups were operating entirely on their own. We have, in short, at least a partial test of work democracy and an opportunity to explore its possible politically relevant consequences.

As a member of an organization development team that helped to implement the design of the plant, I conducted several studies during the plant's first year, including action research, participant observation, semi-structured interviewing, and a questionnaire survey (see Elden, 1976). The data analyzed here come from the questionnaire which was completed on company time by 205 of approximately 225 workers (93 percent).

Since one of my purposes was to develop empirically based indices in the workplace, the bulk of the items in the questionnaire were drawn from standardized scales which seemed to have a certain "face value" relevance. Questions concerning such issues as power, control, authority, autonomy, participation and influence were obviously relevant. Since the design of this organization and Pateman's theory both called for collaborative and participatory modes of work organization, variables concerning peer leadership and group process also seemed relevant. Even within these categories, one finds numerous scales and countless items that could be relevant. Ultimately, of course, the choice of a particular scale was not crucial as the scale items were to be the basis of new, more practically relevant indices.

For the sake of simplicity, the questionnaire was based on pre-tested and standardized scale items developed by the University of Michigan Institute for Social Research in its inter-company longitudinal study. These items, building on the theoretical work of Likert, have undergone extensive validation testing and were used in assessing some 37 industrial organizations with more than 30,000 respondents (Bowers, 1973; Taylor and Bowers, 1972). This theoretical framework also appeared relevant becuase it included both group process and interpersonal relationships on the one hand, plus peer leadership and team functioning on the other. Examples of the items from these scales that were used to operationalize the workplace dimensions of the model include the following (see Elden, 1976, for the complete questionnaire):

1. Concerning peer leadership, to what extent do persons in your team
   —help you find ways to do your job better?
   —provide the help you need so that you can plan, organize and schedule work ahead of time?
   —offer each other new ideas for solving job-related problems?
2. Concerning team functioning and effectiveness, to what extent does your team
   —plan together and coordinate its efforts?
   —make good decisions and solve problems well?
   —share information about important events and situations?
   —really want to meet its objective successfully?
3. Concerning supportive group processes, to what extent are the persons in your team
   —friendly and easy to approach?
   —willing to listen to your problems?
   —encourage each other to give their best effort?

Step 2 in our simple model is based on correlates of a more democratized workplace that includes support for workplace self-management and personal growth, job satisfaction and participation beyond immediate workplace. Data on these issues were captured by questions directly about the support of worker self-management and various dimensions of job satisfaction based on the Michigan questionnaire (satisfaction with the team manager, with progress of the organization, with the opportunity for furthering one's career within the organization, with the opportunity for performing the actual day-to-day activities in the workplace, and the extent to which workers looked forward to coming to work each day). Specifically, two questions were asked about participation and influence:

> When decisions are being made in this plant, to what extent are the persons affected asked for their ideas?

When the persons affected are asked for their ideas, to what extent are these ideas paid attention to?

Finally, the amount of influence over team decision making at different levels of the organization was assessed according to the four questions developed by Tannenbaum (1968) in his work on control graphs.

In the actual company where data were acquired, it turned out that other plants with a similar technology and product in the same corporation had used another questionnaire to assess QWL dimensions similar to these. Selected items from this questionnaire were also used, as they could possibly provide a basis of comparison to other plants with the same technology, but a more traditional form of organization. This comparison later proved problematic, but the items are included in the data analysis to provide a more differential basis for the indices we develop there. All of these items were fixed choice along a Likert 5-point continuum.

In general, operationalizing Steps 1 and 2 in our model presented the problem of having to select from among a wide variety of very sophisticated conceptual schemes. The problem was the reverse concerning the outcome variables in Step 3 of the model. This again reflects the relatively large amount of empirical research focused on behavior within organizations. Political scientists, and others, of course, have developed widely used measures of explicitly political behavior outside of organizations.

The measure most used by Pateman (see esp. 1970, Ch. 3) is the sense of political efficacy. This scale was also developed by political scientists at Michigan, but they were not concerned about work organization. It consists of four items (see Campbell et al., 1960, 1964) measuring attitudes toward government which have been found to be empirically associated with political participation (see, for example, Milbrath, 1965). An important part of the belief in one's political efficacy, of course, is a minimum degree of self-confidence or belief in one's ability to perform adequately (Milbrath, 1965, esp. pp. 56-60). It therefore also seemed reasonable to explore this dimension of personal potency which was operationalized by selected items from Rotter's fate-control scale (see Rotter, 1966; Rotter et al., 1972). These items are of particular interest in that they focus on the degree to which one feels powerless and controlled by fate or luck rather than by one's own will. Seeman (1967) uses this scale to measure that dimension of alienation expressed as a sense of powerlessness. Finally, a third way of empirically exploring the extra-workplace consequences of work democracy was developed in behavioral terms by asking the extent to which an individual allocates free time to various kinds of community service and leisure organizations. The hypothesis here, of course, is that personal potency and a sense of efficacy in the workplace should correlate with a more active social life and higher degree of participation outside the workplace.

It should be noted that more extensive data collection was planned on politically relevant behavior off the job, but because of objections from management, these questions could not be asked. At one point management's sensitivity concerning questions about behavior outside the workplace was reflected in the workers' responses. The final item in the questionnaire was an open-ended question concerning membership in voluntary organizations and was completed by just under half the respondents. The response rate on most other items was approximately 100 percent. Later studies within the plant, including both participant observation and action research, did provide data relevant to these off-the-job issues, but not in a form which can be analyzed here (see Elden, 1976).

In summary, the questionnaire was constructed around the three steps in our model, with the bulk of the items concerning the workplace. Of the total of 72 items, over half (39 items) concerned those QWL conditions theoretically necessary for work democracy (i.e., structural conditions for step 1 such as group autonomy, peer leadership, supportive group processes, opportunity to participate in plant decision making, etc.). Approximately a third (25 items) concerned hypothesized politically relevant consequences either within the plant (i.e., step 2—which included 12 QWL items on work orientation, job satisfaction, support for self-management, and personal growth) or beyond (i.e., Step 3—which included 13 items on political efficacy, personel potency and social participation). The remaining items covered social background (age, education, income, previous work experience, etc.). The complete questionnaire is available in Elden (1976).

Data Analysis

Data analysis occurred in two phases, both relying on the linkage analysis technique developed by McQuitty (1957). This technique gives results similar to factor analysis, but without the necessity of extra-statistical assumptions. It is particularly well suited to reveal the interconnectedness of variables in a correlation matrix. All correlations are simple product-moment coefficients. Those on which the analysis rests are all significant at least to the .05 level unless otherwise indicated.

The first task was to identify the extent to which questionnaire items concerning Steps 1 and 2 (i.e., all QWL items) in the model (Table 1) clustered together into meaningful indices. A correlation matrix was generated and subject to a linkage analysis according to the procedures developed by McQuitty. This analysis produced 11 QWL indices (numbered 1 through 11 in the appendix) which in several important respects departed from the original scales in which these items had been grouped (see Elden, 1976).

The next task was to examine how these 11 QWL indices related to the political variables from the model's Step 3. This task was approached by generating a second correlation matrix containing the inter-correlations of each QWL index with each of the other 10 QWL indices and the 3 political variables (sense of political efficacy, personal potency, and social participation). A McQuitty linkage of this matrix revealed two clusterings or dimensions which I have called Networks A and B (see Figure 1). These appear to represent two quite different dimensions of working life. Significantly, only those QWL indices in Network A are systematically and strongly related to political variables. Let us examine Network A more closely.

Network A QWL indices emphasize variables relating to power, autonomy, decision making, and egalitarian norms (recognition, fairness, participation) plus all work-related outcome variables: self-management support and work orientation. Significantly, the pattern of relationships closely corresponds to that predicted: we hypothesized that the experience of autonomy in work would support personal development and further involvement in the system of self-management. In effect, Network A operationalizes the three elements of the democratization proposition and demonstrates that work democracy links empirically with self-management at work and a sense of political efficacy, personal potency, and social participation beyond work. This network identifies *a political dimension of everyday work life.* The remaining QWL factors (Network B) concern primarily group process and interpersonal aspects of work. These apparently do not have first-order politically relevant linkages.

Network A is built around key concepts in the socio-technical design of work organization and the democratization of authority: the core of this network is the strong connection ($r = .65$) between work democracy (Index 1: "Equity and Recognition in Decision Making and Influence") and intrinsically rewarding work (Index 8: "Satisfaction with Self-Management Development Opportunities"). Clustering around the former, as predicted, are all aspects of power, control, and authority in work: group autonomy, plantwide influence, and democratic, egalitarian norms (fairness, recognition, and openness). That these factors correlate negatively with hierarchical control over the work group (Index 2) is a further indication that the indices measure a relatively self-managing form of work organization.

Around Index 8, as emphasized in socio-technical theory, are all indices of satisfaction, personal growth, and support for the system of self-management indicating an involvement in developmental opportunities. Democratization, in short, is connected with autonomy and learning.

Finally, and most significantly, the developmental cluster (Index 8) connects to participatory behavior beyond the workplace, apparently on the basis that day-to-day work relations under self-management relate to the sense of political efficacy ($r = .28$) which in turn connects with personal potency ($r = .24$). In short, a democratic authority structure covaries with support for self-management, which in turn covaries with more control over one's life in general. Work democracy links with "political potency."

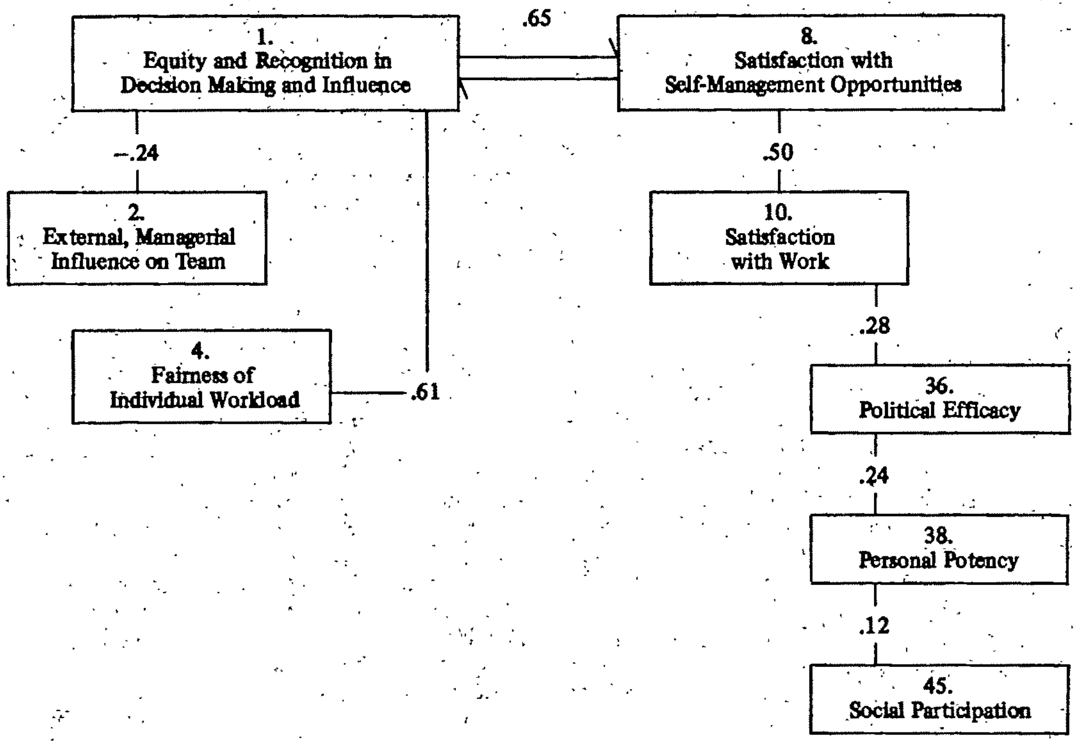
While Network A identifies the normative, power and authority features of work democracy and its politically relevant consequences, Network B focuses on its internal dynamics. Network B, identifying conditions for autonomous group competency, helps to operationalize another important dimension of the self-management design: its group basis. Consistent with socio-technical theory, Network B illustrates that team effectiveness and achievement (Indices 3 and 9) are a function of supportive interpersonal relations together with task mastery. Moreover, an effective autonomous group must possess management skills like planning, coordination, and cooperation.

In this plant, from the worker's viewpoint, effectiveness and achievement are not a function of an external, managerial structure but of the capacity of teams to manage themselves. Managerial influence (Index 2) is consistently and negatively related to all of the conditions for team competency, which reinforces the earlier argument that decreases in managerial authority have an impact on both the internal and external aspects of team autonomy.

Controls for age, education, income, and other background variables did not eliminate these correlations. Significantly, controlling for type of technology in the plant seemed to have more influence on the relation between work and the sense of political efficacy than did social attributes (see Elden, 1976).

The most significant finding is that the empirical data (i.e., Network A), are ordered precisely as hypothesized in our model. In other words, *having some power over one's work co-*

NETWORK A: Dynamics of the Self-Management System (Political Dimension of QWL).


1.
Equity and Recognition in Decision Making and Influence
.65
8.
Satisfaction with Self-Management Opportunities
−.24
2.
External, Managerial Influence on Team
.50
10.
Satisfaction with Work
.28
4.
Fairness of Individual Workload
.61
36.
Political Efficacy
.24
38.
Personal Potency
.12
45.
Social Participation

NETWORK B: Conditions for Team Competency (Non-Political Dimensions of QWL).

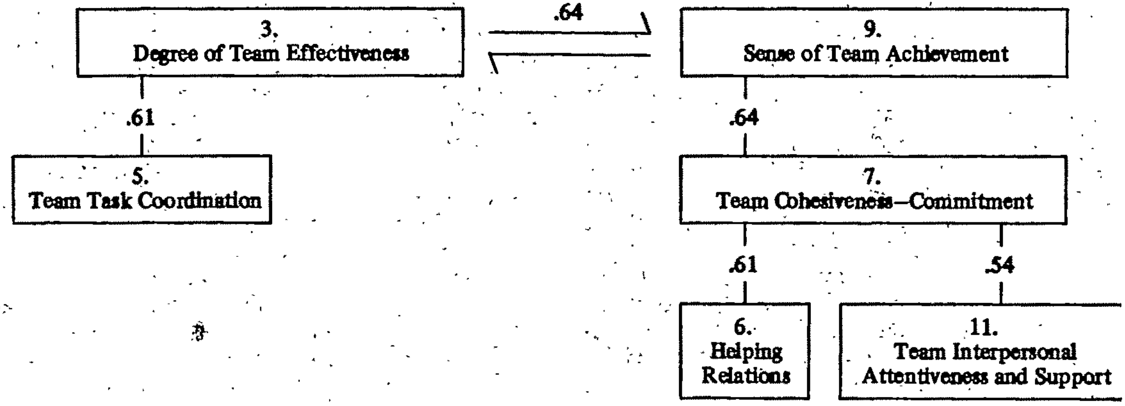

3.
Degree of Team Effectiveness
.64
9.
Sense of Team Achievement
.61
5.
Team Task Coordination
.64
7.
Team Cohesiveness–Commitment
.61
.54
6.
Helping Relations
11.
Team Interpersonal Attentiveness and Support

Figure 1. Dimensions of Politically Linked and Non-Politically Linked Quality of Working Life (QWL) Factors

Source: Formulated by the author.

varies with one's attitude toward taking up participatory opportunities. If there is no chance to have an impact, one does not attempt to participate: this is quite different from saying that personality, income, education or other individual psycho-social characteristics determine the degree of participation. These findings empirically support Pateman's contention that a democratic authority structure in a workplace is likely to enhance participation in and beyond it. Democratization at work is clearly linked to politically relevant variables beyond the organization. Empirically, there appears to be a *political dimension to QWL.*

A second significant finding, also in response to one of the main purposes of this research, is the identification of those QWL dimensions that have political relevance. Not surprisingly, these included all the items concerning control by workers over their own work and opportunities to influence other organizational decisions (Network A). Network B did not contain any items concerning power, control and authority. This conceptual clarification helps to make Pateman's "participatory environment" more operational. It also provides indices that can be used to guide organization redesign efforts more unambiguously in the direction of workplace democracy. At least conceptual tools for distinguishing between *work humanization* and *work democratization* are now available and it seems clear that a QWL strategy based exclusively on humanizing work is not as likely to foster democratic citizenship as one based on *democratizing* work.

Hence this research supports the importance of distinguishing between real and pseudo-participation that has been made on theoretical grounds (Pateman, 1970; Bråten, 1972) as well as on experimental grounds (Mulder, 1971). Simply enhancing the affective experience of workers—through sensitivity training, for example—would not be so likely to lead to an increased sense of political efficacy as would changes in task structures (especially power and authority relations), competencies and social norms which increased workers' relatively autonomous control over their work and influence in company affairs. Affective enhancement alone would imply a strategy of unobtrusively reinforcing hierarchical authority while developing a somewhat more satisfied, committed, and flexible work force.

It seems quite feasible to improve QWL by simply pleasing rather than empowering. Without empowering structural change, a QWL organizational development strategy like job enrichment or flexitime might lead to the reverse of work democracy. In the present case, analysis of participant-observation data (Elden, 1976) gathered after the survey and informal reports from the plant during the following several years indicate that management's long-run strategy may have been humanizing rather than democratizing. More contemporary efforts in other plants that are unionized suggest that union participation in work improvement programs may be a prerequisite for democratizing change (as well perhaps as authentically humanizing change)!

The findings of the present research are suggestive and possibly useful, but hardly conclusive. In the first place, the most critical correlation, between Steps 2 and 3, is only .28. While this is both theoretically and statistically significant, it is not impressive. Furthermore, it appears modest in comparison to those correlations between the intra-organizational QWL indices. There are several possible explanations for this. First, the scales from which the items composing our QWL indices were drawn have been tested and developed by many researchers over a long period of time. In contrast, politically relevant consequences might not be adequately tapped by the sense of political efficacy measure which was not designed for the purposes for which it is being used here. In retrospect, it seems likely that some of the most interesting and immediately relevant political consequenes were not captured by the two standardized scales allowed in the questionnaire. It might also be argued that 6 months was too brief a period in which to observe any connection between work-related political attitudes and political attitudes outside the workplace. A great deal more than 6 months were used to develop these attitudes in the first place. In short, this social learning process and the connection between work democracy and

Table 3. Evidence of a Political Dimension of QWL

1. Work Democracy →	*2. Positive QWL Consequences* →	*3. Participatory Politics*
Indices of autonomy, influence, and democratic norms (Index 1)	Indices of satisfaction, personal growth, and self-management: QWL (Indices 8 and 10)	Indices of a more participatory political orientation (sense of political efficacy–Index 36)
	$r = .65$	$r = .28$

Source: Formulated by the author.

political learning constitute a more subtle relationship that is tapped by correlational data from standardized attitudinal questionnaires. Indeed, a later participant-observation study indicated many other connections (Elden, 1976). We suggest this issue as a subject warranting further research.

In summary, the basic pattern in the empirical data is quite clear: all correlations are in the hypothesized direction. All save one (social participation for which the data are inadequate) are statistically significant. More important, all are theoretically significant. The empirical evidence is patterned exactly as one would expect if participatory workplace structures did *in fact* facilitate politically relevant learning.

Discussion

Correlational data, of course, do not permit one to draw such a conclusion. Logically, causation can go either way. Workers with a high sense of political efficacy prior to beginning work in this factory may have responded more favorably to the opportunity for self-management provided by the new organization's design than workers without such a sense of efficacy. Causation is difficult to assess in data correlating attitudes with attitudes. Little of the relevant research that has become available since this study was undertaken (shortly after the publication of Pateman's thesis in 1970 [see Table 4]) avoids any limitations of the present study. All of these studies support my findings without extending them much. The research presented in Table 4 is limited to empirical studies in the 1970s grounded in some form of democratic workplace experience and its politically relevant correlates. Other bodies of literature such as those dealing with work and family, work and mental orientations (instrumental-intrinsic and alienative versus self-realizing), and work and society are excluded here because of space limitations and because they are less directly rooted in a democratic work organization. These are taken up in Elden (1976).

Unfortunately, there are few studies directly relevant to my subject and those that have been identified are limited to a particular democratic aspect of work such as autonomy, discretion, or self-direction. Nevertheless, the findings reported are consistent with the findings of this study.

The most striking supportive evidence comes from the first report of a series of studies by Karasek (1978), which explicitly focused on the social and political effects of work design ("job socialization") in a nationally representative sample of employed males in Sweden between the ages of 18 and 60, interviewed in 1968 and again in 1974 (N = 1638). Interestingly enough, as we have noted earlier, since 1969 Sweden has perhaps experimented more than any other country with democratic job reforms. While it is not yet realistic to evaluate discrete factory-level changes in Sweden on the basis of national aggregate data, Karasek shows that increases in *"job decision freedom" and "job demands" between 1968 and 1974 are associated with behavioral increases in political participation,* both among those who are highly active (e.g., those speaking at meetings, writing articles, attempting to influence others, airing complaints) and among those who are more than passive members of organizations (e.g., those participating in political or labor union activity, demonstrations). Karasek's measures of work democracy and its consequences are more demanding and comprehensive than those used in the present study. He has therefore a more solid basis from which to generalize and is able to make more useful distinctions.

Although we cannot fully deal with these here, Karasek's summary of the empirical finding makes a strong case for the political effects of work experience (1978, p. i):

> A test of the model with a longitudinal random sample of the Swedish work force (including both expert and self-reports of job characteristics) shows that workers whose jobs become more "passive" also become "passive" in their political and leisure activity. Workers with more "active" jobs become more "active" outside of work. Cross-sectional analysis yields consistent results. The alternative explanation that personality background characteristics account for the findings is undermined by the fact that the above associations change little when controlled for education and father's education. Further controls for family income, and kinship ties define boundaries to the job socialization effect, but do not substantially weaken it.

As is evident in Table 4, similar findings from quite independent studies in different countries about the expressly political effects of work democracy are paralleled by findings that work democracy also correlates with more active leisure time. Karasek, for example, consistent with our findings that work democracy covaries with personal autonomy and social participation, also finds that greater challenge and self-determination in the workplace closely correlates with a more active social life. This finding is not new (see, for example, Kornhauser, 1965; Blauner, 1964), but has been extended and supported by over a dozen studies since 1970, each using different methods with different kinds of workers in different countries (see Elden, 1976; Gardell, 1976; Emery and Phillips, 1976; Karasek, 1978, for comprehensive analysis). In short, the more

Table 4. Studies of Workplace Political Correlates (1970-1980)

Reference	Data Sources and Methods	Indices of a Democratic Workplace	Political Correlates				
			Political Efficacy, Participation, Information, etc.	Democratic Character and Citizenship	Trade Union and Other "Higher-Level" Participation Related to Work Interests	Active Leisure —Participation in Voluntary Organizations	Psychological Functioning, Mental Health, and Quality of Life
Martin Meissner (1971). "The Long Arm of the Job: A Study of Work and Leisure." *Industrial Relations* 10: 239-60.	Observation and interviews with 206 industrial workers in one company	Task discretion and social interaction			X	X	
Stanley Parker (1972). *The Future of Work and Leisure.* London: Paladin.	200 interviews and 425 questionnaires from respondents in selected occupations	Use of abilities in work and job autonomy				X	
Harold Sheppard and Neal Herrick (1972). *Where Have All the Robots Gone?* New York: Free Press.	Interviews with 371 industrial workers in 5 companies	Job anatomy variety and responsibility	X	X			
William C. Torbet and Malcolm Rogers (1972). *Being for the Most Part Puppets.* Cambridge, Mass.: Schenkman.	Observation and interviews with 209 industrial workers in 5 companies	Control over work and demand on skill	X	X		X	
Melvin Kohn and Carmi Schooler (1973). "Occupational Experience and Psychological Functioning: An Assessment of Reciprocal Effects." *American Sociological Review* 48: 951-61.	Interviews with sample of 3,101 male, civilian workers	Occupational self-direction		(X)		X	X

(continued)

Table 4. (continued)

Reference	Data Sources and Methods	Indices of a Democratic Workplace	Political Correlates: Political Efficacy, Participation, Information, etc.	Political Correlates: Democratic Character and Citizenship	Political Correlates: Trade Union and Other "Higher-Level" Participation Related to Work Interests	Political Correlates: Active Leisure—Participation in Voluntary Organizations	Political Correlates: Psychological Functioning, Mental Health, and Quality of Life
Gardell, Bertil (1976). *Arbetsinnehåll och Livskvalitet [The Content of Work and the Quality of Life]*. Stockholm: Prisma.	Interviews with 1500 workers in selected industries	Degree of competence and freedom in the work			X	X	X
Gunnela Westlander (1976). *Arbete och Livssituation [Work and Life Situation]*. Stockholm: The Rationalization Council, LO-SAF.	3 studies, various methods, data from 2,387 industrial workers	Self-determination over one's work			(X)	X	X
Fred E. Emery and Chris Phillips (1976). *Living at Work*. Canberra; Australian Government Publishing Service.	Interviews with representative sample of 2000 urban workers	Autonomy, learning, and challenge				X	X
Winfried Hacker (1978). "Psychological Analysis and Design of Work Activities." *Nordisk Psykologi* 2: 114-26.	Reviews selected studies from Eastern Europe	Responsibility and freedom to decide tasks			X	X	
Robert Karasek (1978). "Job Socialization: A Longitudinal Study of Work, Political and Leisure Activity." Revised Working Paper No. 59. Stockholm: Institute for Social Research.	Interviews with a panel (longitudinal from 1968 and 1974) of 1638 workers representative of Sweden's total work force	Job discretion and engagement	X			X	

the political dimension of work develops toward democracy, the more productive use is made of free time and the more the characteristics of democratic citizenship are developed.

In short, the results of the research study reported here and all other empirical evidence which has become available since Pateman (1970) validate her contention that a democratically designed work environment induces the development of the type of political resources necessary for participation in and beyond the workplace. As Karasek observes:

> A telling finding in the Swedish data is that for workers with little education, the job is the primary classroom for transmitting patterns of leisure and political participation (Pateman, 1970). This job-centered educational process may only be successful to the extent that the work environment deemphasizes "passive" work that is emotionally undemanding and intellectually restrictive, and encourages "active" tasks involving psychological challenge and use of judgemental capacity (Karasek, 1978, p. 26; for a similar analysis, see the work of Ambrecht, 1975, 1976; and Inkeles, 1969).

With the exception of Karasek's research, the studies presented in Table 4 do not contribute much solid evidence on the issues raised by Pateman. These studies are difficult to compare, especially since none uses exactly the same measures of work democracy or of its politically relevant consequences. In addition, only 3 studies take up expressly *political* behavior. Furthermore, they are based on data from selected factories at one point in time and these data are only in some cases subject to correlational analysis. In short, research on this topic generally does not seem to have advanced much beyond the assessment by Seeman (1975, p. 108):

> What is at issue is the extent of generalization of the worker's experience, from the jobs proper to the extra-job milieu. Given the tendency to assume that work is of primary importance in defining the self and in connecting the individual with the larger society, and given the centrality of work in the Marxian sources of alienation, it seems surprising how limited the demonstration has actually been concerning this purported generalization.

This is not to say that research findings are in conflict. On the contrary, the findings of what little research is available exhibit a degree of agreement uncommon in the social sciences (Gardell, 1976). The main problem is that there has been too little research. One of the rare assessments of work organization from a political science point of view reaches exactly this conclusion and pinpoints the need for further research on adult political socialization in the context of work organization. As Ambrecht writes:

> Our review of the literature dealing with different cases of industrial democracy reveals that by and large, the political socialization effects accruing to increased workers participation within the work sphere have not so far been sufficiently explored. However, in those few cases where extraorganizational effects have been the object of inquiry, findings have unanimously pointed to politicization consequences ensuing from the democratization of the work organization. The redesigned work environment in which worker participation and control in decision-making is increased, may thus represent a major means whereby lower status individuals may overcome socialized limitations and become full participants.
>
> Efforts at democratizing the world of work—which no doubt will become more numerous in the future—thus represent a fertile and largely uncharted research territory where adult political socialization processes can fruitfully be explored (1975, pp. 22-23).

Ambrecht focuses on the central research issue in Pateman's proposition: the "educative effects" of participation. A number of investigators have reported anecdotal evidence illustrating politically relevant social learning as part of QWL improvement programs. This needs to be more systematically explored.

Research here would help overcome the limitations of correlational data from a single point in time. It would naturally require a longitudinal panel or field study type of research design that focused on the process of politically relevant social learning. Data on structural and attitudinal change would be necessary not only before and after the experience of democratization but also *during*. The main variables could be drawn from those identified in Table 2 and the indices resulting from the present study (see appendix). An important task in such a research project would be to develop more sophisticated indices of politically relevant consequences outside the workplace.

Regardless of whether this particular design is used, the work democracy indices developed in this study could possibly be used by students of political efficacy and political participation with only a relatively minor extension of the scope of the research. Use of these indices, of course, is not as important as political science analysis of work organization and job design from the point of view of adult political socialization. If the design of democratic organizational structure provides an alternative route to full democratic citizenship, alternative to those of income and formal education, then political scientists need to begin looking inside organizations at how jobs are designed and how more democratic work experiences can be developed.

Work democracy seems to be one way of creating an enduring propensity to participate in political affairs. Learning to participate can be intrinsically rewarding so that political participation, once initially experienced, would be self-reinforcing. The key seems to be developing structures, such as work democracy or community councils or free schools, where people have participation, rather than apathy, reinforced by the organization of day-to-day life. Learning to participate in such structures appears to be highly likely. Research facilitating their development is essential for a political science that would contribute to a more participatory politics.

Appendix: Contents of the Quality of Working Life (QWL) Indices Used in Figure 1

I relied on a variety of standardized scales to gather data on QWL aspects of work democracy, but since one purpose of this research was to develop indices more expressly and empirically related to work democracy, the data analysis was based on the actual items in the original scales. As I have described more fully elsewhere (Elden, 1976), items are grouped together to form an index because linkage analysis revealed them to be more closely correlated with each other than with other items. The items composing each index are presented here to provide a more complete understanding of the different indices composing Networks A and B (Figure 1). The numbers of the indices below correspond to the numbers of the indices in Figure 1. Network A contains indices 1, 2, 4, 8, and 10. Network B contains indices 3, 5, 6, 7, 9, and 11.

Index 1: Equity and recognition in decision making and influence.

—We feel we are treated fairly.
—We feel our efforts are recognized.
—We feel we are rewarded fairly for what we do.
—When the persons affected are asked for their ideas, to what extent are these ideas paid attention to?
—We have good ways of knowing how good our results are.
—People are open and honest in talking with each other.
—When decisions are made in this plant, to what extent are the persons affected asked for their ideas?
—How much influence do team members have on what goes on in your team?
—We help new people fit into the group.

Index 2: External, managerial influence on team activities.

—The degree of top management's influence on what's going on in your team.
—The degree of middle management's influence on what's going on in your team.
—The degree of team manager's influence on what's going on in your team.

These items all derive from the Tannenbaum control graph scale (1968). Interestingly enough, the fourth item on this scale, concerning the degree of influence that team members have over their own team, is more closely correlated with Index 1. All three of these items correlate negatively with Index 1, indicating some empirical verification of a degree of team self-management or autonomy.

Index 3: Degree of team problem-solving effectiveness.

—People in my group work well as a team.
—We work effectively together to solve problems.
—We are appropriately involved in decisions affecting our own work.
—Team mates offer each other new ideas for solving job-related problems.

Index 4: Fairness of individual workload.

—The amount of work I am asked to do is about right.
—The pressure I work under is reasonable.
—We get a fair shake when we do something wrong.
—The things I need to be able to do my work right are available.

Index 5: Team-task coordination.

—My team plans together and coordinates its efforts.
—My team makes good decisions and solves problems well.

Index 6: Helping relations.

—Team mates help me find ways to do my job better.
—Team mates help me plan, organize and schedule work ahead of time.

Index 7: Team cohesiveness, commitment.

- —People on my team emphasize a team goal.
- —People on my team encourage each other to work as a team.
- —People on my team exchange opinions and ideas.
- —My team is able to respond to unusual work demands placed on it.
- —I have trust and confidence in the people on my team.
- —People on my team encourage each other to give their best effort.
- —My team really wants to meet its objectives successfully.
- —Information about important events and situations is shared within my team.

Index 8: Satisfaction with self-management development opportunities.

- —I support team self-management in this plant.
- —I am satisfied with the progress that I have made in this organization up to now.
- —I am satisfied with what I feel are my chances for getting ahead in this organization in the future.
- —All in all, I am satisfied with my team manager.
- —I feel that my pay is related to helping the company be successful.
- —There is an opportunity here for people to grow.

Index 9: Sense of achievement.

- —Morale in our team is high.
- —We really feel we are getting things done.
- —I know how well I am measuring up to my work.
- —People on my team maintain high standards of performance.
- —People on my team are friendly and easy to approach.

Index 10: Work satisfaction.

- —I enjoy performing the actual day to day activities that make up my functions.
- —I look forward to coming to work each day.

Index 11: Team interpersonal attentiveness and support.

- —To what extent are persons on your team willing to listen to your problems?
- —When you talk with persons on your team, to what extent do they pay attention to what you are saying?

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Language and Its Abuses in Hobbes' Political Philosophy

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As in Hobbes' view it is principally the capacity for speech that distinguishes men from even the social animals, so it is in verbal and doctrinal controversies that he usually finds the sources of conflict and sedition. Hobbes analyzes—in the hope of doing away with them—a variety of what he regards as abuses of language, such as metaphor, equivocation, eloquence, and absurdity, which are especially productive of political disorder. He also offers models and, in his own political philosophy, examples of the proper uses of language as science and counsel, which he believes are necessary to the establishment and governance of well-ordered commonwealths in the modern world, characterized as it is by widespread learning and disputatious habits. In the pursuance of his project, however, Hobbes himself is paradoxically forced to resort to the eloquence which he otherwise condemns, and his own observations on language provide grounds for doubts about the success of his enterprise.

Language and Hobbes' Analysis of Conflict

In all three of his political treatises (*Elements* I.19.5; *De Cive* V.5; *Leviathan*, Ch. 17, p. 111) Thomas Hobbes takes issue with Aristotle's comparison of political man to other gregarious animals, advancing six arguments to refute the suggestion that man is by nature a political animal like the bees or ants. His arguments are as follows: (1) Men, unlike bees, compete for precedence and honor, and for the "acknowledgment of wisdom" which can be a source of honor, and thus fall into hatred and disputes. (2) Man's private good, which lies in being eminent in "dominion, superiority, and private wealth," is not identical with the common good. (3) Men, having high opinions of their own wisdom, claim to perceive defects in governments and devise schemes for altering them. (4) Men, unlike bees, possess the "art of words" by which things can be made to appear better or worse than they actually are, and hence are able to "instigate one another to faction." (5) Men alone can quarrel over differing conceptions of right and wrong, and distinguish between "injury" and mere "damage." (6) And finally, the union of men is artificial, while that of bees is natural—an assertion whose demonstration constitutes the project of Hobbes' political theory as a whole.[1]

Four of the six specifically human attributes that Hobbes regards as conducive of political discord have to do with the faculty of language, a fact indicative of Hobbes' pervasive concern with words and doctrines as sources of conflict and sedition. "The tongue of man is a trumpet of war and sedition," he says; and the "elegant speeches" of Pericles confounded all Greece (*De Cive* V.5). Nor is it only in the spoken word, nor only in antiquity, that Hobbes perceives disturbing consequences in unrestrained speech. Speeches of republican Romans such as Cato, to the effect that "*all kings are to be reckoned amongst ravenous beasts*" (*De Cive*, Ep. Ded.), preserved and studied by young men at English universities, were in his opinion a principal cause of the Civil War in which he found his political teachings confirmed.

I shall argue, contrary to a common view,[2] that it is in speech in general, and in the misuse of words, that Hobbes usually seeks and finds the proximate causes of political discord. This is true in the state of nature, which, it has aptly been pointed out, is characterized by an "anarchy of meanings" as well as the absence of sovereign power (Wolin, 1961, p. 257), and it is likewise true in the imperfectly constituted commonwealths whose experience comprises most of history. This argument may be related to a larger theme which I shall not elaborate, to the effect that, in default of natural order and ends such as were posited by traditional natural law, Hobbes conceives the political problem as one of reconstituting the state as artifice, by human will (Oakeshott, 1975, p. 7).

I am grateful to Professor Michael L. Walzer, in whose Hobbes seminar at Harvard the ideas in this paper were first developed.

[1] Hobbes (deliberately?) misreads Aristotle, who argues (*Politics* 1253a1-18) that it is precisely speech that differentiates men from bees. Politics for Aristotle is to a large degree contestation in words over doctrines of justice. Cf. Weinberger (1975, p. 1339): "For Hobbes . . . opinion is inherently controversial."

[2] For example, McNeilly (1968, pp. 159-68) and Gauthier (1969, pp. 14-18) offer expositions of Hobbes' analysis of conflict without mentioning language abuse or verbal and doctrinal controversy.

It may simply be noted that this project depends in large degree on the correct application of language, from the form of words in the founding covenant to the sovereign's authority to promulgate right doctrine. It is also a precarious one, threatened at every stage by failure to reach agreement or abide by the rules established. And while Hobbes points to the persistence of private opinion (grounded in private passions) as the ultimate source of conflict, verbal disputes are its most common immediate and manifest cause.[3]

Despite his extreme individualism and materialistic motivation, Hobbesian man is not so much acquisitive as he is avid for power, honor, and preeminence.[4] Less widely recognized is the degree to which Hobbes presents the manipulation of words as an important, perhaps even the primary, means to both of these ends. Men achieve power by persuading others to follow them or their cause; men achieve eminence through the reputation of cleverness or wisdom. In either case, disputes arise over differences of opinion, which in the last analysis may often be reduced to differences over the meaning of words. Hobbes goes so far as to say that "all controversies" stem from contradictory use of such appellations as *meum/tuum,* just/unjust, good/evil (*De Cive* VI.9; cf. Watkins, 1968, p. 151). It is perhaps not always so simple; but Hobbes has no doubt, in his own age and country, of the danger not only of pernicious doctrines, but also of the emotive overtones and political impact of even a single word:

> From the reading, I say, of such books, men have undertaken to kill their kings, because the Greek and Latin writers, in their books, and discourses of policy, make it lawful, and laudable for any man to do so; provided, before he do it, he call him tyrant. For they say not *regicide,* that is killing a king, but *tyrannicide,* that is killing a tyrant is lawful (*Leviathan,* Ch. 29, p. 214).

Semantic disputes such as these in politics may have figured as prominently in motivating Hobbes' elaborate theory of language as did his campaign against the Aristotelianism of the universities in favor of the new science of motion (cf. Peters, 1967, p. 112). These two motives, however, are by no means unrelated. Absurdity, or the uttering of meaningless propositions, is a charge Hobbes frequently makes against both his political and his philosophical opponents; and absurdity he regards as a common pitfall of the use of words, which leads men to madness if not to wisdom (*Leviathan,* Ch. 4, p. 22), or indeed of thought, which for Hobbes is nothing more than the manipulation of words (*De Homine* X.1). But the political implications of language and its abuses go far beyond the dangers of absurdity, and Hobbes' theory of language advances from the unmasking of tautologies in scholastic Aristotelianism to the exploration of a variety of types of misuses of words for political purposes. Writing in an age of severe ideological conflict, Hobbes was the first modern political philosopher to become aware of the force of words and doctrines as political weapons, both exacerbating conflicts of interest and generating new conflicts of principle with a momentum of their own.

Differences of opinion, which prevent cooperation or a natural union of men, seem inevitable: men's passions and inclinations are diverse, stemming from their different "constitutions," and "good and evil are names given to things to signify the inclination or aversion of them, by whom they were given" (*De Cive* III.31). Differences of opinion, moreover, quite apart from competition over material goods, produce conflicts as a consequence of certain psychological mechanisms connected with the pleasure attached to honor:

> Again, when every man follows his own opinion, it is necessary that the controversies which rise among them, will become innumerable and indeterminable; whence there will breed among men, who by their own natural inclinations do account all dissension an affront, first hatred, then brawls and wars. . . (*De Cive* XVII.27).

In these circumstances Hobbes concludes that unrestrained liberty of opinion is incompatible with civil peace, and that "in the well-governing of opinions consisteth the well-governing of men's actions" (*Leviathan,* Ch. 18, p. 116). The third function of the sovereign, after wielding the "sword of justice" and the "sword of war," is "to make some common rules for all men, and to declare them publicly, by which every man may know what may be called his, what another's, what just, what unjust, what honest, what dishonest, what good, what evil. . ." (*De Cive* VI.9).[5] The authoritative definition of all such politically sensitive or disputed terms is thus claimed as a prerogative of the sovereign in the state (cf. Wolin, 1961, pp. 254-56, 259-60). Indeed, before the establishment of sovereign authority there cannot even be said to have existed any common sense of such words, whose *only* public meaning, Hobbes says, "proceeds from the right of the magistrate" (*De Cive* XII.1). It is not, furthermore, only words usually regarded as having political significance which must receive authoritative definition, but any term—even, ultimately, a term in a scientific dispute—that may breed controversy detrimental to peace: the city even defines what is a *man,* if "a woman bring forth a child of unwonted shape, and the law forbid to kill a man. . ." (*De Cive* XVII.12).[6]

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[3] Cf. Mansfield (1971, p. 100) on the Hobbesian covenant as a "bypass of [private] political opinion." Krook (1956, p. 20) says that "wrong linguistic usage is the *defining mark* of sedition."

[4] This view persists in opposition to Macpherson (1962). A historical version is presented by Thomas (1965), but the case rests on Hobbes' theory apart from his or its social affinities. The place of "vanity" in the motivation of Hobbesian men is stressed most notably by Strauss (1952, pp. 10-13).

[5] Parry (1967), Bell (1969), and Watkins (1968, p. 153) discuss ways in which Hobbes anticipates modern linguistic philosophy, particularly with reference to Austin's account of "performatives."

But while all public meanings are conventional, there is clearly a class of words of special political importance, confusion or disagreement about which is particularly likely to generate disputes. Hobbes points out that people make an invidious distinction, for example, between "aristocracy" and "oligarchy"—"which two words signify the same thing, together with the divers passions of those which use them" (*Elements* II.1.3; cf. *De Cive* VII.2). The common lawyers persist in Aristotle's obscure and question-begging definition of "justice" as "giving to every man his own" (*Dialogue,* p. 8). Sir Edward Coke, citing Bracton, insists that "law" is a "just statute, commanding those things which are honest, and forbidding the contrary," failing to understand the meaning of the word "command" and maintaining an illusory separate standard of justice (*Dialogue,* p. 25).

If the sovereign must in the end define such troublesome terms as these, political philosophy also has a role to play, counseling the sovereign when one exists, teaching true principles on its own account in times of disturbance, unmasking fallacies and abuses of words and reasonings at all times. "In the right definition of names lies the first use of speech; which is the acquisition of science" (*Leviathan,* Ch. 8, p. 22); and from correct definitions proceeds correct political science, and thence, peace in the commonwealth. I shall turn later to Hobbes' conception of science as one of the proper uses of language; first, however, I shall examine his criticisms of its various improper uses and the dangers thereby arising in political life.

Abuses of Language and Speech

Hobbes enumerates various abuses of language, among which the following may be mentioned. People can "devise errors and pass them on," errors that may arise from deceptions of sense, from words of inconstant or equivocal signification, from "words of things inconceivable," or from fallacious reasoning. They can speak passionately, or lie, or use language metaphorically, wresting words from their true signification "either to adorn or deceive," and using words as weapons "to grieve one another." And people may speak merely from habit, without thinking, or through reliance on unexamined authority (*De Homine* X.3; *Elements* I.5.14; *De Cive* XVIII.4; *Leviathan,* Ch. 4, p. 19).

For the most part these abuses appear to be perversions of legitimate functions of language. The principal such function is rational thought itself, including both science, or the formation and communication of true or hypothetical propositions, and the prudential or probable knowledge that is derived from reflection on experience. Although Hobbes claims that all reasoning is in the end at the service of the passions (*Leviathan,* Ch. 8, p. 46), he nevertheless maintains the possibility of exact and demonstrable science (geometry being the prototype) and of dispassionate, precise reasoning on any subject given prior agreement on first principles. Corresponding to this use of language is a class of abuses that may be called errors or fallacies: these are more characteristic of the written than the spoken word, and the pernicious influence of dubious philosophical doctrines in religion and politics stands out as a major example of this kind of perversion.

Hobbes also recognizes that speech is legitimately used to express certain passions, such as appetites (as in interrogation, requesting, prayer) and will (as in promising, threatening, and commanding). Speech functions such as these, however, may easily be transformed into such perverted forms as instigation and appeasement (serving to increase or diminish passion in another) or persuasion and exhortation beyond the limits of rational discourse (*Elements* I.13.6-7; cf. also *Leviathan,* Ch. 6, p. 38). Here we encounter a second class of abuses which in Hobbes' terms may be called passionate speech, perhaps qualified as speech immediately in the service of passions, and especially of passions other than fear or prudence.[7] For the political scientist the principal danger of this sort of abuse lies in the spoken word, with eloquence appearing as a frequent villain, although Hobbes has much to say, particularly with respect to the Civil War, about how written doctrines too can stimulate passions that find an outlet in violent speeches and sedition.

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[6] Compare the current controversy regarding abortion.

[7] Strauss (1953, p. 201; 1952, p. 150) argues that, although all reason is in the service of passion, Hobbes can maintain a notion of "right reason" as reason in the service of the greatest passion, fear.

Besides these two general classes of language abuses I shall consider two Hobbesian accounts of the motivation of such abuses, one a set of psychological observations about people's propensities to be ensnared by nonrational language, and the other a rudimentary theory of ideology, or the use of elaborate doctrine as a political weapon in the service of partisan interests or a will to power.

Turning first to Hobbes' theory of the passions and language abuse, we must recall that for Hobbes every word is a name that signifies a conception in the mind of the speaker. Conceptions, however, depend on objects as construed by the individual and are thus subject to the influence of his particular constitution and set of passions, which, because they may obstruct right reasoning, Hobbes calls "perturbations of the mind" (*De Homine* XII.1):

> And therefore in reasoning a man must take heed of words; which besides the signification of what we imagine of their nature, have a signification also of the nature, disposition, and interest of the speaker; such as are the names of the virtues and vices; for one man calleth *wisdom*, what another calleth *fear*. . . . Such names can never be true grounds of any ratiocination (*Leviathan*, Ch. 4, pp. 24-25).

The first problem of passionate speech thus pertains to names of "inconstant signification," whose referents are inescapably private and affective, at least in part. What kinds of words fall into this category? Hobbes asserts that all words (besides "is") fall into three classes: names of things, names of accidents, and names of names (common names for classes of things sharing certain accidents).[8] It is difficult to imagine passion intruding into the naming of bodies or of physical accidents, although errors of sense may create disagreements. Problems mainly arise with what may be called moral accidents, and it is generally moral epithets such as "good/bad" and "right/wrong" that Hobbes suggests are in need of authoritative definition. This list may be extended to include the names of the passions themselves, each of which according to Hobbes has two names, as "glory," depending on one's feelings, is equivalent to "pride" (*Elements* I.9.1); and to those names of names that depend on judgments about moral accidents, such as the contraries "democracy/anarchy," "aristocracy/oligarchy," and "monarchy/tyranny," which, Aristotle notwithstanding, signify not different things but conceptions differing according to the speaker's passions (*De Cive* VII.2). Without explicitly stipulated definitions no such words can be used for ratiocination, and even then their lack of tangible referents renders them always liable to inconstancy, or shifts of meaning under the influence of passions.

The problem of inconstant signification concerns a generally unconscious phenomenon. More easily identifiable, but perhaps more sinister, is the problem of equivocation—and of metaphor, since "every *metaphor* is by profession equivocal" (*De Corpore* II.12). Hobbes argues that names can be univocal or equivocal, that is, they may have precise or variable meanings, depending on how they are used, "for some use them properly and accurately for the finding out of truth; others draw them from their proper sense, for ornament or deceit" (*De Corpore* II.12). This suggests that by equivocation Hobbes refers to variability of signification that is normally conscious, even intentional. Any word can be made equivocal (*Elements* I.5.7), a fact that Hobbes attributes to the human faculty of quickness of mind, a peculiar verbal ability producing "those grateful similes, metaphors, and other tropes, by which both poets and orators have it in their power to make things please or displease. . ." (*Elements* I.10.4). Metaphor can of course be employed simply for ornamentation, although even in this case it can be dangerous, as in the Bible, which Hobbes insists must receive authoritative interpretation precisely because it contains so many metaphors: it is important for Hobbes' theology to realize, for example, that "spirit" and "inspiration" are used metaphorically, while "kingdom of God" is not (*Leviathan*, Chs. 34, 35, pp. 257, 264, 269). But equivocation also has its deliberate political uses, and in this regard also Hobbes has his suspicions about the Bible: he finds, for instance, that the word "justice" occurs frequently in the Greek and Latin versions, but that in English this is often subversively rendered "righteousness," "which few understand to signify the same, but take it rather for rightness of opinion" (*Behemoth*, pp. 242-43).

Beyond such particular difficulties lies the more general problem of elaborate speech, which is usually based on equivocation, and its manifestations as eloquence. Again, Hobbes is concerned with a certain type of fine speech, that in which "a metaphorical use of words [is] fitted to the passions," and of which the end is not truth, but victory (*De Cive* XII.12). Hobbes treats the problem of eloquence primarily in connection with the spoken word (rhetoric),[9] which in assemblies of men invariably becomes a weapon in the omnipresent competition for eminence (*De Cive* X.11). His criticisms of collective counsel as susceptible of kinds of oratorical imprudence to which the councillors separately would not be subject (*Leviathan*, Ch. 5, p. 30), and of democracy as a contentious "aristocracy of orators" (*Elements* II.2.5), are based on his observations of the readiness with which public speaking departs from the proper rational function of words:

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[8] On Hobbes' nominalism and its stark implications for ethics and politics, see Krook (1956), Goldsmith (1966, pp. 63-64), and Watkins (1965, pp. 255-57; and 1968, pp. 104-07, 144-50).

> Impudence in democratical assemblies does almost all that is done; it is the goddess of rhetoric, and carries proof with it. For what ordinary man will not, from so great boldness of affirmation, conclude there is great probability in the thing affirmed? (*Behemoth*, p. 250).

The lesson that Hobbes drew from Thucydides, too, was that reckless and dangerous policies are likely to result from the eloquence of demagogues who compete to flatter the vanity of their popular audiences:

> For a man that reasoneth with himself, will not be ashamed to admit of timorous suggestions in his business, that he may the stronglier provide; but in public deliberations before a multitude, fear (which for the most part adviseth well, though it execute not so) seldom or never showeth itself or is admitted ("Thucydides," p. xvi).

In his analysis of the civil disorder in England, Hobbes points to several areas where eloquence of the spoken word was a contributory cause. His attack on the common law and his preference for clearly defined statute law, rests in part on the prevalence of equivocation in pleadings; this arises, in turn, both from the vagueness of common law terminology and from its admission of the legitimacy of the lawyer's interested position in suits, which induces him to try to "wrest the sense of words from their true meaning," and to cultivate a "faculty of *rhetoric* to seduce the jury, and sometimes the judge also, . . ." (*Dialogue*, pp. 6-7).

Political controversies feed on the disputatious habits of lawyers, whose profession teaches them "the art of cavilling against the words of a statute" (*Dialogue*, p. 45). They also arise, generally, from the liberty of public speaking on the part of private men, without license from the state, an invitation to sedition that Hobbes believes is unique to Christendom: "The heathen kings foresaw, that a few such orators would be able to make a great sedition" (*Behemoth*, p. 183). Hobbes deplores the disorders generated by the preaching of the itinerant friars of an earlier age, and he compares their role to that of the Puritan preachers of his own day, whom he describes in terms of their histrionics, their ability to feign inspiration, and their knowledge of how, in speaking, to play on the fears and interests of their audience (*Behemoth*, pp. 193-96). The proceedings of Parliament, finally, stand out as a case in point of the dangers of semantic abuse in a deliberative assembly, whether it be a council (as Hobbes regards Parliament) or a democratic assembly (as he accused it of regarding itself). Hobbes points to the meaninglessness of the terms used in the attainder of Strafford: he is accused of acting "traitoriously" (without reference to any statutory definition of treason), of subverting the "fundamental laws" (which are undefined), and of attempting to introduce "arbitrary government" (when in fact *all* government is necesssarily arbitrary)—and, in general,

> This [the Long] Parliament, in the use of their words, when they accused any man, never regarded the signification of them but the weight they had to aggravate the accusation to the ignorant multitude, which think all faults heinous that are expressed in heinous terms, if they hate the person accused (*Behemoth*, p. 249).

But eloquence is a hazard not only of spoken language, nor are Parliament and the law courts the only institutions that Hobbes finds guilty of uncritical speech. Written doctrine presents its dangers as well, and the universities with their Aristotelianism most often loom as Hobbes' favorite objects of attack. If public speech presents obvious dangers to peace, Hobbes also acknowledges that circumstances (such as "time, place, countenance, gesture," and so forth) may serve to clarify the meaning and intentions of the speaker (*De Cive* XVII.18). But written doctrine lacks these aids, with the result that, unless it is as precise as Euclid, it is perhaps even more susceptible of abuse than speech—though perhaps here it is often error rather than, or along with, the passions, which is to blame: "For there is scarce anything so clearly written, that when the cause thereof is forgotten, may not be wrested by an ignorant grammarian, or a cavilling logician, to the injury, oppression, or perhaps destruction of an honest man" (*Dialogue*, p. 64). Although it is possible to identify two types of such abuse, the disinterested errors of the "ignorant" and the directed equivocations of the "caviller," it is in practice not always easy to distinguish them, and the damage they do is similar. Academic disputings, moreover, are self-propelling: unclear writings elicit commentaries; the "commentaries will need explications; and in process of time, those explications expositions; those expositions new commentaries, without any end" (*De Cive* XVII.18). An end can be reached only through authoritative decision: Hobbes concludes that the sovereign must act as the final interpreter of Scripture and, by extension, of any scholastic dispute of consequence.

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[9] Strauss (1952) emphasizes Hobbes' debt to Aristotle's *Rhetoric*; the argument pertains, however, to Hobbes' doctrine of the passions rather than to his analysis of verbal conflict.

The cavillers and commentators to whom Hobbes refers are the philosophers of the universities, those whom he calls *dogmatici* in contrast to the *mathematici* of sounder method. The former put forward their opinions with passion (*Elements* I.13.4), like the adherents of the various Greek philosophical schools, of whom Hobbes says,

> Their moral philosophy is but a description of their own passions; . . . they make the rules of *good* and *bad*, by their own *liking* and *disliking:* by which means, in so great diversity of taste, there is nothing generally agreed on; but everyone doth, as far as he dares, whatsoever seemeth good in his own eyes, to the subversion of the Commonwealth (*Leviathan,* Ch. 46, p. 438).

More moderately, however, he also says that the *dogmatici* err in starting from unexamined custom or literary authority, "take the habitual discourse of the tongue for ratiocination," and proceed without "any evident demonstration." Both of these charges run through Hobbes' polemic against philosophers, although it is the latter, that of bad reasoning and absurdity, that is the more devastating. There is, in addition, the philosophers' characteristic urge towards novelty for its own sake: "In a multitude of men there are many who, supposing themselves wiser than others, endeavor to innovate, and divers innovators innovate divers ways; which is mere distraction and civil war" (*De Cive* V.5).

In good faith or bad, philosophy as traditionally practiced is in Hobbes' eyes a vast breeding ground of controversies, and in particular of the two major kinds of controversy from which England was suffering. In the first place, he charges that the hopeless incomprehensibility and contentiousness of Roman Catholic doctrine is due to its assimilation of Aristotelian teachings of the sort perpetuated at the universities; philosophy serves as a salve for "a great many absurd articles . . . which articles they thought fit to have believed, because they bring, some of them profit, and others reverence to the clergy, even to the meanest of them" (*Behemoth,* p. 215). Three of the four causes of "spiritual darkness" are doctrines maintained through scholasticism: erroneous interpretation of Scripture, the admixture with religion of erroneous philosophy, and "false, feigned, or uncertain" history (*Leviathan,* Ch. 44, p. 398). Cardinal Bellarmine's subtle distinction between temporal and spiritual authority "is but words" (*Leviathan,* Ch. 42, p. 378); and in general all theological controversies involve "words some of them without meaning, and nothing but the canting of Grecian sophisters" (*Elements* II.6.9).

In the second place it is the universities which, through the same philosophy, promote subversive political doctrines: "Studying Greek and Latin, [men] became acquainted with the democratical principles of Aristotle and Cicero, and from the love of their eloquence fell in love with their politics, and that more and more, till it grew into the rebellion (*Behemoth,* p. 218). It is these philosophers—"Plato, Aristotle, Cicero, Seneca, Plutarch, and the rest of the maintainers of the Greek and Roman anarchies" (*De Cive* XII.3)—who are guilty of making the meaningless verbal distinction between "kings" and "tyrants" which Hobbes is so at pains to expose (*Leviathan,* Chs. 29, 46, pp. 214, 447). Nor is it merely erroneous doctrine, but equally insidiously an overrated sense of their own wisdom, that the universities instill into their students, conducing to the dangerous habit of political criticism:

> For it is a hard matter for men, who do all think highly of their own wits, when they have also acquired the learning of the university, to be persuaded that they want any ability requisite for the government of a commonwealth, especially having read the glorious histories and the sententious politics of the ancient popular governments of the Greeks and the Romans. . . (*Behemoth,* pp. 192-93).

These attacks on erroneous or absurd philosophical doctrines bring us to the final category of language abuses, that pertaining to the use of "words insignificant" and words for "feigned things"—words which, although uttered, do not signify any coherent conception in the mind, and words which, though significant, do not refer to any "something" in the world (*De Corpore* II.6). In the second category are words which signify fictional things, clear mental conceptions arrived at through the faculty of "compound imagination," but which do not correspond to any real object: we have a meaningful name "centaur," or we can compound an image of ourself with that of Hercules or Alexander, "which happeneth often to them that are much taken with reading of romances" (*Leviathan,* Ch. 2, p. 10). Perhaps such political words as "democracy," under whose influence Hobbes saw his seditious contemporaries acting, are significant only of conceptions imaginatively generated through the association of a variety of images.

In contrast to these are words that are strictly insignificant or nonsensical, lacking even a coherent mental conception, but that are nevertheless used to the detriment of civil peace. A contradiction such as "round quadrangle" falls into this category of "senseless speech" that Hobbes calls absurdity (*Leviathan,* Chs. 4, 5, pp. 24, 27); so does the logical error of treating an abstraction (that is, a class of accidents) as if it were a body, as is done when one says (as Roman Catholics do) that "faith" is "infused."[10] Hobbes' favorite example of a senseless word that has had momentous consequences is "ghost"—which "signifieth nothing, neither in heaven nor earth, but the imaginary inhabitants of men's brain" (*Leviathan,* Ch. 34, p. 259). Ghosts, like the gods and demons of the Greeks, are the externalization of certain kinds of "phantasms" in the mind, of the sort that may be caused by a blow to the eye, refracted light, visual error, or day-dreaming; the conceptions behind them, expressed by the absurd phrase "incorporeal substance," are contradictory and empty.[11] But ignorance of how to distinguish dreams from reality on the part of the "simple people," who "are content to believe any absurdity, rather than to trouble themselves to examine it," is easily exploited by "crafty ambitious persons"; hence the belief in ghosts that produced gentile demonology was exploited by the Roman Catholic priesthood to justify their ritual practice (*Leviathan,* Chs. 2, 45, pp. 12, 418-19, 431-32). It is the scholastic philosophers, however, who, attempting to introduce the categories of a faulty physics into the rules for worshiping God, are responsible for most of the absurd propositions of Catholic doctrine (such as transubstantiation, free will, and "incorporeal substances"). These, in Hobbes' view, were responsible for all the disputes of the Reformation (*De Homine* XIV.13; cf. *Leviathan,* Ch. 12, p. 79):

> School-divines, who striving to make good many points of faith incomprehensible, and calling in the philosophy of Aristotle to their assistance, wrote great books of school-divinity, which no man else, nor they themselves, were able to understand. . . . For none of the ancient philosophers' writings are comparable to those of Aristotle, for their aptness to puzzle and entangle men with words, and to breed disputation, which must at last be ended in the determination of the Church of Rome (*Behemoth,* pp. 185, 215)

But even if incomprehensible even to themselves, the "distinctions which signify nothing" that are the stock-in-trade of the philosophers do serve "to astonish the multitude of ignorant men" (*Behemoth,* p. 213); even absurdities can be effective weapons of political controversy. Hobbes' other example of this is the case of the common lawyers, who, declining to accept the definition of law as command, seek an alternative definition in "natural reason," "universal reason," or especially "legal reason," defined as the "artificial perfection of reason, gotten [by lawyers] by long study, observation, and experience." But Hobbes finds unintelligible the concept "legal reason": "There is no reason in earthly creatures, but human reason" (*Dialogue,* pp. 122, 22, 4); to essentialize an accident which is properly used only of individuals is a logical error culminating in "insignificant speech," or absurdity, which is yet not without its attractions.

Passionate speech, inconstant signification, equivocation, metaphor, eloquence, absurdity: these then are the main types of abuse of language analyzed and illustrated by Hobbes. But why are these abuses so frequently encountered? In the first place Hobbes presents us with a variety of observations about motivation with respect to speech as an autonomous human activity. Habit, for example, can detract from the rational use of speech: a man can speak partly or entirely without thinking—

> as it is with beggars, when they say their *paternoster,* putting together such words, and in such manner, as in their education they have learned from their nurses, from their companies, or from their teachers, having *no images or conceptions* in their mind, answering to the words they speak (*Elements* I.5.14).

Hobbes also mentions such speech-acts as "cursing, swearing, reviling, and the like" which "do not signify as speech; but as the actions of a tongue accustomed" (*Leviathan,* Ch. 6, p. 39). Nor is it only beggars and swearers who speak from mere habit; learned men also "take up maxims from their education, and from the authority of men, or of custom, and take the habitual discourse of the tongue for ratiocination" (*Elements* I.13.4). In none of these examples of habitual speech does Hobbes examine political implications, yet one can without difficulty understand how the mechanism of a "tongue accustomed" may perpetuate the use, in political controversies, of the various insignificant, equivocal, and otherwise passion-laden terms invented by philosophers and converted to partisan use.

It is also easy to grasp, in the light of Hobbes' onslaught on professional philosophers, the significance of his second psychological observation, which concerns the intrinsic appeal of words to certain kinds of people. Knowledge is said to be "good," not only because it is useful, but because it is pleasing to men's natural curiosity (*De Homine* XI.9), and because "the constitution of man's nature, is itself subject to desire novelty" (*Leviathan,* Ch. 29, p. 213). Language, furthermore, can be beautiful: "innocent" ornamentation is even said to be one of its legitimate uses (*Leviathan,* Ch. 4, p. 19); and beautiful language can be seductive: "The reading of some such books is very delightful; for they are most eloquently written, and contain many clear, wholesome, and choice sentences, which are yet not universally true" (*De Corpore* I.7). And the principal reason why ancient philosophy has for so long held its ground against the moderns is "not so much by giving any light to the understanding as entertainment to the affections" (*De Cive,* Ep. Ded.). It is, of course, primarily intellectuals—"such as love knowledge"—who are attracted to the mysteries of words and elaborate doctrines, and who "consequently take delight in finding out the meaning of the most hard texts . . . [and] praetermitting the easy places which teach them their duty, fall to scanning only the mysteries of religion" (*Behemoth,* p. 232).

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[10] On Hobbes' theory of absurdity, see Peters (1967, pp. 127-28).

[11] *Elements* I.11.5; *Leviathan,* Ch. 45, p. 418. Hobbes uses "phantasms" as generally equivalent to "images" in the mind, the source of which is always physical and external, and which usually but not always reflect an external body. Cf. Peters (1967, pp. 116-18) on private phantasms and public meanings.

Seldom has a philosopher had sharper words for fellow philosophers of differing views than Hobbes has for the scholastics, yet in this antagonism he is merely illustrating his final major point on the psychological motivation of disputes with words:

> Furthermore, since the combat of wits is the fiercest, *the greatest discords* which are, must necessarily arise from this contention. . . . For not to approve of what a man saith, is no less than tacitly to accuse him of an error in that thing which he speaketh: as in very many things to dissent, is as much as if you counted him a fool whom you dissent from. Which may appear hence, that there are no wars so sharply waged as between sects of the same religion, and factions of the same commonweal, where the contestation is either concerning doctrines or politic prudence (*De Cive* I. 5, emphasis added).

Man's nature is to seek power; but glory is the sense of power, and fine speech is a source of glory (*De Cive* I.2); while honor is the recognition of one's power by others, and so men quarrel "for trifles, as a word, a smile, a different opinion, or any other sign of undervalue" (*Leviathan,* Ch. 13, pp. 81-82). Words, as slander, can serve as direct weapons of attack on another's honor, provoking "brawling and fighting"; and from the fact (contrary to Hobbes' usual estimation of the fear of violent death) that "most men would rather lose their lives, . . . than suffer slander" (*De Cive* III.12; *Leviathan,* Ch. 15, p. 100) follows a law of nature against contumely. But words are weapons indirectly as well, as men fight with doctrines as a mode of competition for honor. Hobbes says that men do not quarrel over geometry because no interest is involved (*Leviathan,* Ch. 11, p. 68), yet he himself was for years engaged in a bitter dispute over a mathematical demonstration, thus confirming his observation: "There is scarce any principle, . . . from whence there may not spring dissensions, discords, reproaches, and by degrees war itself," a fact attributable to "the disposition of men, who, seeming wise to themselves, will needs appear such to all others."[11] Hobbesian men (like Hobbes himself) are attracted to words and demonstrations; they devise doctrines in which they come to feel a vested interest, and they defend them with all the passion elicited by their instinct for honor and eminence. When Hobbes claims that knowledge is power, his meaning goes beyond a Baconian orientation to utility to a recognition of a separate plane of human intercourse and conflict on which verbal confrontations are primary.

If Hobbes' analysis of language provides much insight into the dynamics of doctrinaire disputes, in which people are divided more over words than over facts and material interests, he is also interested in those cases where words—still employed as political weapons—serve and sometimes mask group interests of various kinds. Hobbes thus on occasion articulates, although not in any systematic fashion, a concept of ideology as a second general way of accounting for language abuses (cf. Wolin, 1961, p. 475, n. 45). One can find a few suggestions that a crude acquisitive instinct normally underlies ideological conflicts: Hobbes concludes generally about the Church-state clashes, for example, that "the Kings had a more considerable part on their sides, than the Pope had on his; and shall always have so, if they have more money" (*Behemoth,* p. 186). Doctrinaire differences are likely to occasion violent conflict only when they coincide with the "ambition, profit, or lust" that Hobbes elsewhere asserts underlie all disputes (*Leviathan,* Ch. 11, p. 68). Likewise, Hobbes alleges that men who invent clever doctrines do so mainly because it is lucrative: "Astrology . . . is the strategem of a man to steal money from stupid people in order to escape poverty. Those who pretend that they are prophets, when they have performed no miracles, have the same purpose" (*De Homine* XIV.12). And his history of the Civil War is replete with allusions to the financial interests lying behind the proceedings of the Commons, as when he points out that the Parliamentarians, after having willfully misinterpreted Magna Carta to support their disinclination to pay Ship Money, proceeded when in power to tax the country, in their own interests, even more heavily than the king had done (*Behemoth*, pp. 209-12).

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[11] *De Cive* VI.11 (note). Hobbes' dispute with Wallis on squaring the circle is discussed in Peters (1967, pp. 36-38). Cf. Wolin (1970, pp. 15-16) on Hobbes' own "epical impulse" to intellectual combat; and Oakeshott (1975, p. 12).

More often, however, Hobbes identifies a concern for honor or a drive after power itself, rather than material interest, as the motivating force. He doubts, for example, that the aristocratic Scots Presbyterians could have had such tender consciences or fine theological convictions, but rather finds that their religious allegiance was a suitable vehicle for asserting their particular Scottish and aristocratic claims to honor (*Behemoth*, pp. 200-01). Ambition for office and power is imputed to democrats—"seditious teachers, and other prating men"—whose verbal abilities serve them as weapons to the end of "popular government, where the most ignorant and boldest talkers do commonly obtain the best preferments" (*Dialogue*, p. 18). It is to the corporate interests of "unpleasing priests" of all sects that "all the changes of religion in the world" may be attributed (*Leviathan*, Ch.. 12, pp. 79-80)—priests who "wish to win people over not so much to faith in God (in whom all believe already) as to faith in themselves" (*De Homine* XIV.4). And to this end the mere appearance of knowledge can be an effective weapon; for while some religious men go so far as to feign inspiration and prophecy, "the rest pretend only, for their advancement to benefices and charge of souls, a greater skill in the Scriptures than other men have, by reason of their breeding in the universities" (*Behemoth*, p. 276). Philosophers, finally, do not escape the charge of rationalizing, in their doctrines, a desire for power and influence:

> Aristotle in the first book of his *Politics*, for a foundation of his doctrine, maketh men by nature, some more worthy to command, meaning the wiser sort, such as he thought himself to be for his philosophy; others to serve, meaning those that had strong bodies, but were not philosophers as he (*Leviathan*, Ch. 15, p. 100).

The love of novelty and eloquence, the attraction of mysteries, the inertia of received opinions institutionally entrenched—if all of these are insufficient to account for so startling an array of abuses, Hobbes is never slow to suspect and to unmask the "restless desire of power after power" which is so often its most fundamental motivation.

The Right Uses of Language and Hobbes' Objectives

The extent of the possibilities of the misuse of speech, and the universality of the factors that encourage it, would appear not to augur well for Hobbes' objectives of political science and civil peace. Can Hobbes entertain any serious hopes in these respects when he is generally so pessimistic —and so incisive in his documentation of the grounds for pessimism—in others? His case rests on the conviction that language abuses and fallacies are of such a nature that, once they are uncovered, they can be abolished; and philosophy is charged with this task. The feasibility of this project, however, can be challenged on grounds provided by Hobbes himself; and in the final analysis it seems that his case is too strong, that he himself provides, on numerous occasions, insights which implicitly belie his ostensible purposes. First, however, I shall consider two Hobbesian prototypes of the correct use of language, which stand as counterparts to the perversions already discussed: counsel in the realm of speech, and science in that of doctrine. Hobbes discusses these two enterprises as examples of the legitimate and proper use of words, and they provide models of the rationality whose implementation he seeks. Even as we examine them, however, it is difficult to escape the sense of precariousness with which they are presented; and they may thus serve as an introduction to the criticisms of Hobbes' position with which I shall close.

Hobbes points out that confusion arises from the fact that three different modes of speech make use of the "imperative manner of speaking," and that the same words, "do this," depending on the context, the speaker, and his motives, may constitute either command, counsel, or exhortation. The nature of command is clear: it depends on the presence of obligation in the addressee. As between the other two, Hobbes seeks clearly to distinguish (legitimate) counsel from exhortation, or "counsel vehemently pressed." Counsel is dispassionate speech whereby the experience of the counselor is made known to another—in a polity, to the ruler—in "as significant and proper language, and as briefly, as the evidence will permit" (*Leviathan*, Ch. 25, pp. 166-69). Since experience is identical with memory of past consequences, which is vital to the formation and confirmation of probable propositions and predictions, a good counselor is said to be analogous to the memory and reason of the ruler he serves; in fact, Hobbes regards counsel as an exemplar of legitimate speech because it is simply an extension of the rational faculties, enlarging the ruler's experience and assisting in his reasoning from cause to effect, and from end to means.[13]

Counsel thus understood presupposes the absence of passion or interest—indeed of will—on the part of the counselor; the end of counsel is the benefit of the ruler of whose reason the counselor is an extension. That this is an unattainable ideal, however, is suggested when Hobbes says that in practice no counsel can ever be entirely free of passion (*Elements* II.5.4). What purports to be counsel readily shades over into its perverted, "vehement" counterpart, exhortation or dehortation. These, while attempting to maintain the appearance and forms of ratiocination, are varieties of illicit "passionate speech":

> And, therefore, they have in their speeches, a regard to the common passions and opinions of men, in deducing their reasons; and make use of similitudes, metaphors, examples, and other tools of oratory, to persuade their hearers of the utility, honor, or justice of their advice (*Leviathan*, Ch.. 25, p. 167).

Besides the "long and vehement urging" that is invariably its mark, Hobbes argues that proffering unwanted advice (such as Parliament directed at an unwilling king) amounts to exhortation, and in this form it is prohibited by a law of nature (*Elements* I.17.8). Giving false "counsel" may of course be motivated by the counselor's own selfish interests; but it may also be pressed on an unwilling ruler, because, as with public speaking in general, it can be a source of honor: "To hearken to a man's counsel, or discourse of what kind soever, is to honour; as a sign we think him wise, or eloquent, or witty. To sleep, or go forth, or talk the while, is to dishonour" (*Leviathan*, Ch. 10, p. 58). The omnipresent urge to eminence which operates when a man is among other men, and the suitability of speech to this end, leads Hobbes to deny the possibility of genuine counsel being given to a multitude and to suggest that a sovereign listen to counselors separately, never in an assembly, where all the dangers of passion and eloquence converge:

> There cannot be an assembly of many, called together for advice, where there be not some, that have the ambition to be thought eloquent, and also learned in the politics; and give not their advice of care of the business propounded, but of the applause of their motley orations, made of the divers colored threads, or shreds of authors. . . . For the passions of men, which asunder are moderate, as the heat of one brand; in an assembly are like many brands, that inflame one another, especially when they blow one another with orations, to the setting of the commonwealth on fire (*Leviathan*, Ch. 25, pp. 170-71; cf. also *Elements* II.5.4).

If Hobbes offers counsel as a possible type of precise, rational speech, he nevertheless impresses upon us in the same breath its tendency to degenerate into its opposite, leaving us with a vivid picture of the dangers of exhortation. And perhaps even Hobbes fails to perceive the full extent of the potentialities for discord inherent in the offering of advice on policy. Hobbes is able to characterize counsel both as dispassionate reasoning and as advice oriented towards the ends of the ruler, since the counselor is conceived as an extension of the ruler's reason. Conversely, Hobbes describes exhortation both as passionately offered advice and as "corrupt" counsel, covertly in the interest of him who exhorts, since he regards the passions as directly reflecting the appetites and aversions of the individual (*Leviathan*, Ch. 25, p. 167). But this scheme overlooks a third case, which in times of political upheaval and disagreement can be the most discordant of all—the case of the intransigent idealist who passionately advances an opinion which he believes to be directed towards the common (or the sovereign's) good, and which he insists on advancing whether or not the sovereign willingly hears him. For Hobbes, such selfless political activism would count as a special case of exhortation—albeit one that presents peculiar difficulties for Hobbesian psychology.

Hobbes presents his political works as an attempt to construct a comprehensive and exact political science, deduced on the one hand from his materialistic psychology and ultimately from his physics, and verified in many respects on the other hand by reference to experience. Such a science has more than a theoretical interest: for anyone who, like Hobbes, desires civil peace it results in a set of recommendations constituting counsel on a large scale for sovereigns and people alike. The thrust of this project is twofold. The first task of Hobbes' science, since it is a novel discovery, is to combat the false philosophy which has in the past been conducive of erroneous political science and hence sedition: "It is to this purpose, that men may no longer suffer themselves to be abused by them, that by this doctrine of *separated essences*, built on the vain philosophy of Aristotle, would fright them from obeying the laws of their country, with empty names" (*Leviathan*, Ch. 46, p. 442). Since all of the causes of the dissolution of commonwealths are false opinions, or structural weaknesses supported by false doctrines (*Leviathan*, Ch. 24, pp. 211 f.), it is clear that the critical role of the new science must be extensive. But beyond this lies the revelation that states themselves are created by men through the use of words: the pacts and covenants in which they originate "resemble that *fiat*, or the *let us make man*, pronounced by God in the creation."[14] If this is true, then clearly the stability and orderliness of any commonwealth depends on the content of those words spoken by its institutors and subsequently acquiesced in by them and by their successors. Any state rests upon a contract, however this may manifest itself in particular instances; and civil peace in the state depends both on common agreement about the meaning of the terms used in that contract and on the validity of the doctrines presupposed by its articles. Hobbesian political science establishes these propositions and thus the importance of its own role as a source of accurate definitions and valid doctrine; and it then proceeds to enunciate those truths of human nature and of politics on the basis of which the desired kind of commonwealth may be founded. Hence, from the potential contributions of accurate science to the establishment of the conditions of civil harmony, there arises Hobbes' sense of purpose and his confident hopes when setting forth the methods of a new political science. Whether such confidence is justifiable, however, is a matter on which some doubts may be cast.

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[13] *Leviathan*, Ch. 25, deals with the nature of counsel; in chapter 30 Hobbes evidently is offering counsel to the sovereign.

If counsel is that legitimate kind of speech which reflects the functions of observation and memory in reasoning, so science is that which reflects the deductive faculty, embracing those forms of reasoning that result in logical truths (cf. *De Cive* XVIII.4). This means that the only kinds of statements proper to science are universal propositions: particular, empirical claims, and the conclusions resulting from syllogisms in which particular propositions serve as the minor premises, are contingent or hypothetical and thus lack the status of universality proper to science. It also means that scientific truth is grounded in the conventional meanings of the words which constitute the subject and predicate of the proposition. Science proceeds by deductive (or "synthetic") "demonstrations," and it thus depends on the clarity and precision of definition of its initial terms from which the body of propositions are derived (*De Corpore* III.2-11). Besides the avoidance of logical fallacies, however, the truth of any science also depends on the truth of its initial definitions, or first principles. While Hobbes believes that certain statements about bodies and motion must constitute the fundamental definitions of any science of the physical world, he recognizes that the first principles of any science cannot be demonstrated, but only claimed as self-evident (*De Corpore* III.9, VI.5, 12); and their truth in the end relies on the agreement of the propositions derived from them with the evidence of experience: "Now when a man reasoneth from principles that are found indubitable by experience, all deceptions of sense and equivocation of words avoided, the conclusion he maketh is said to be according to right reason" (*Elements* I.5.12).

In Hobbes' system political science belongs to the class of sciences of physical bodies, since man, its subject matter, is nothing but a special sort of body; and thus his political propositions are derived from the principles of his psychology and ultimately his physics. Hobbes' hopes for the acceptance of his political science, therefore, depend on agreement about the self-evidence of his first physical principles; different theories of physics or psychology would result in disagreements about politics, and Hobbes cannot escape the difficulties presented by this fact except through his confidence in the eventual victory of the new physics and his conviction that common experience would serve to confirm the validity of his deductions.[15] But Hobbes must also face the criticisms of opponents who deny that political science is or can be a branch of the physical and psychological sciences and who construct arguments that involve deductions from first principles claiming self-evidence of a different sort. Hobbes points out that all agree to the syllogism, "Socrates is a man, and therefore a living creature," because all agree on the respective meanings of "man" and "living creature." But there are those who also argue that, "Sophroniscus is Socrates' father, and therefore his lord," is equally evident. Hobbes denies that it can be considered evident until agreement has been reached on the definitions of "father" and "lord," and in fact he disputes the definitions implied in this deduction (*De Cive* IX.1). Yet Hobbes cannot escape the fact that there were those in his day who argued this way and drew momentous conclusions from such arguments; nor can he deny that their logic is as precise as his. The difficulty lies in the definitions of words and in particular in the first principles or initial definitions from which one constructs one's demonstrations; and Hobbes' own political science seems unlikely to oust its competitors from the field unless its particular definitions should somehow prove more persuasive than all others.

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[14] *Leviathan*, Introduction, p. 5. Wolin (1961, p. 248) interprets Hobbes' covenant as a "highest expression of political creativity." It should be emphasized that covenanting is (formally speaking) simply a matter of pronouncing the right words, as taught by political philosophy. The creativity rests on verbal acuity.

[15] Cf. Watkins (1968, pp. 144-50). Hobbes seems to have believed in the possibility of objective scientific knowledge, so that the meanings of the words used in physics have a different status of conventionality than moral epithets. But disagreements over scientific, no less than over moral, beliefs can lead to political controversy. For a recent critical discussion of Hobbes' science and the place of language in it, see Danford (1980).

Hobbes' project of having his political science accepted as a whole would appear to be a long and arduous one. There are, however, certain subsidiary objectives that he can pursue, which may have the effects of reducing controversy in certain areas and of conducing in a limited fashion to the strengthening of civil authority. He can, for instance, effect a clarification in political argumentation through the example he provides of rigorous logic and consistency in the use of words. Furthermore, even if he fails to demonstrate the logical necessity of his political science through its deduction from physics, he can still have significant sections of it accepted by winning acquiescence in certain of his key definitions—whether such acquiescence comes about through his claims of self-evidence or through authoritative imposition. At the very least he can wage particular battles against logical fallacies and specific misuses of words, ranging over the various disputes that trouble his countrymen.

It is in these lesser campaigns that Hobbes appeals to common usage and to etymology as means of demonstrating the superiority of his definitions. This represents a retreat from his scientific deductions and a gesture towards the claims of opinion, yet it is a tactic which Hobbes pursues with the same concern for unambiguous meanings that characterizes his scientific ideal. On occasion Hobbes, not unlike a modern language philosopher, seeks the true or original definition of a troublesome term by examining the various ways in which it is customarily used, hoping that the revelation of the inadequacies of the received meanings might result in a willingness to accept his version of the true meaning. Hobbes says, for example, that to know whether or not a promise or covenant has been entered into, "is nothing else but to demand whether such words, spoken in such a manner, be by common use and consent of the subjects *a promise* or *covenant*" (*De Cive* XVII.28). Hobbes here assumes that there exists common agreement on the use of certain terms, which may therefore be accepted as satisfactory by the political scientist. When unanimity is not forthcoming, however, as in the case of the terms "word of God" and "church," Hobbes finds it useful to run through a large number of different usages in the process of evolving definitions which he believes to be the most accurate and the most appropriate for political science (*De Cive* XVII.15-21). Likewise, in offering his definition of law as command, Hobbes seeks to refute its erroneous definitions as counsel, or common consent, or right, by a close examination of how these latter words are used in other contexts—an examination which uncovers inconsistencies when they are applied to law (*De Cive* XVI.1-3). Finally, Hobbes enjoys indulging in speculative etymology when it can aid him in buttressing his definitions, presumably hoping that the argument from origins is one that would appeal to his contemporaries: "Etymologies are no definitions, and yet when they are true, they give us much light towards the finding out of a definition."[16] Some of Hobbes' etymologies, such as those of "ratiocination" and "deliberation" (*Leviathan*, Chs. 4, 6, pp. 22, 37), simply enable him to enlarge on his psychology; others, however, are of direct political significance. The original meaning of "tyranny" suggests that its differentiation from "monarchy" is a merely partisan one (*Leviathan*, Ch. 46, p. 447); that of "heresy" serves to diminish the significance of the offense and to unmask the historical interest of the Roman Church in altering the meaning of the word (*Behemoth*, p. 174; *Dialogue*, pp. 97-102). A correct understanding of the original denotation of the term "chancellor," finally, suggests that this office was at one time a merely subordinate, clerical one and serves as ammunition against Sir Edward Coke's claims to the contrary (*Dialogue*, pp. 55-57).

Hobbes' political science, then, appears to hold promise for the advancement of knowledge and for peace. The obvious difficulty is, as I have suggested, that it is merely one system of definitions and deductions—and rather startling ones at that—offered to a public already divided by doctrinal and terminological disputes. Hobbes' definitions have the advantages of precision, consistency, and systematic coherence; yet for all that they cannot be fully demonstrable, nor are they universally self-evident, nor can they avoid head-on clashes with deeply held popular opinions. In these circumstances Hobbes is faced with the task not only of expounding his system but of convincing others to accept it, persuading them of its superiority. Is this an enterprise in which Hobbes can engage without contradiction, without incurring the charge of using language in ways which he himself regards as improper?

Hobbes must *teach* his principles to others, and he recognizes that he must assume the role of educator:

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[16] *Dialogue*, p. 80. According to Hobbes' conventionalist view of meaning, origins should not be important; his use of etymologies thus appears to be pursued as a rhetorical or persuasive device.

> Why may not men be taught their duty, that is, the science of just and unjust, as divers other sciences have been taught, from true principles and evident demonstration; and much more easily than any of those preachers and democratical gentlemen could teach rebellion and treason? (*Behemoth*, p. 212).

Teaching is defined as the "begetting in one another the same conceptions that we have in ourselves," through the marshaling of evidence and right reasoning; if evidence is lacking, and one conveys unfounded opinions, one is no longer teaching but persuading (*Elements* I.13.2). Hobbes thus considers teaching to be a third case of the legitimate use of speech, with affinities to both counsel and science; persuasion is a perversion analogous to exhortation. Yet is not such teaching, like counsel, an unrealizable ideal? Hobbes goes on to say that "the infallible sign of teaching exactly and without error, is this, that no man hath ever taught the contrary"—be the contrary view held by ever so few, "for commonly truth is on the side of a few, rather than of the multitude" (*Elements* I.13.3). "Teaching" in the strict sense is thus reserved for the communication of conceptions of absolute clarity and self-evidence, just as counsel and science are held up as pure types of rational discourse. Yet by this criterion Hobbes is certainly not teaching, since his doctrine is new and controversial. Must he then resort to persuasion in order to promulgate his definitions, and will he not be obliged to utilize passionate speech in this effort?

In his major philosophical work Hobbes asserts: "Philosophy professedly rejects not only the paint and false colors of language, but even the very ornaments and graces of the same" (*De Corpore* I.1); it is partly for this reason that true philosophy finds it difficult to dislodge the false opinions of its opponents, especially if they are eloquent. Later, however, Hobbes tempers his confidence in the capacities of pure science to abolish all errors and bring peace; and in the end he acknowledges that reason must reach some compromise with eloquence in the establishment of a commonwealth:

> So also reason, and eloquence, though not perhaps in the natural sciences, yet, in the moral, may stand very well together. For where soever there is place for adorning and preferring of error, there is much more place for adorning and preferring of truth, if they have it to adorn (*Leviathan*, "A Review, and Conclusion," p. 461).

This admission explains the paradox that Hobbes, despite his vociferous polemics against metaphors, eloquence, and other forms of language abuse, is himself one of the most eloquent, and probably the most metaphorical, of political philosophers (Wolin, 1970, p. 38; cf. Oakeshott, 1975, p. 14). "Metaphors, and senseless and ambiguous words, are like *ignes fatui*; and reasoning upon them is wandering amongst innumerable absurdities" (*Leviathan*, Ch. 5, p. 30);

> A man that seeketh precise truth had need to remember what every name he uses stands for, and to place it accordingly, or else he will find himself entangled in words, as a bird in lime twigs, the more he struggles the more belimed (*Leviathan*, Ch. 4, p. 21).

Figures of speech accentuate the dangers of figures of speech; and an extended simile draws us into an awareness of the ensnaring potentiality of beautiful language: "For speech has something in it like to a spider's web, (as it was said of old of *Solon's* laws) for by contexture of words tender and delicate wits are ensnared and stopped; but strong wits break easily through them" (*De Corpore* III.8). As counsel is in practice not always distinguishable from exhortation, so neither is teaching from persuasion; and Hobbes the teacher—the teacher of a science ostensibly characterized above all by its precision of language—finds that he cannot avoid the tactical use of eloquence as a means of access to the mind of his fellow man.

Hobbes' work is plagued throughout by the contradiction between the alleged self-evidence of its principles and the prevalence of the kinds of errors and discords against which those principles militate. If men are capable of governing themselves according to a rational science, then why do Hobbes' prescriptions encounter so many obstacles and antagonists? Hobbes' mood oscillates between confidence in the efficacy of his demonstrations and outraged wonder at the tenaciousness of errors and absurdities; but in the end the astuteness of his observations of irrational features in political behavior overshadows the assumption of rationality required by his system. Science not only acknowledges that it must use eloquence in its battle for recognition; it also discovers, while engaged in the exposure of errors, that there are propensities in human nature which render questionable the entire project of a scientific politics. The attractiveness of myth and custom, the belief in ghosts, and the tenacity of private meanings are three further phenomena that Hobbes acknowledges but cannot finally reconcile with his hopes for a science.

Hobbes believes that ignorance of the correct principles of politics leds to sedition, but he also recognizes that this is a modern phenomenon, true in an age of disputation but not true in all times and places. All that is necessary to be known for civil peace, he says,

> the most ancient sages did judge fittest to be delivered to posterity, either curiously adorned with verse, or clouded with allegories, as a most beautiful and hallowed mystery of royal authority; lest by the disputations of private men it might be defiled (*De Cive*, Author's Preface).

In those pre-Socratic days, when "the science of justice [was] wrapped up in fables," kings did not have to justify the authority which they exercised, nor did the citizens, who "reverenced the supreme power," measure justice by the "judgments of private men," but by the laws (*De Cive*, Author's Preface). Even Hobbes (of all people) can idealize the ancient city and the unreflecting solidarity, grounded in myth, that it enjoyed. He argues, however, that the time of such harmony is irrevocably past. We live in an age of reasoning and disputation in which the state must be founded anew on doctrine so convincing that it can put an end to all erroneous reasonings forever. Science, in other words, purged of every equivocal and emotive element, must accomplish the same end once achieved in opposite fashion, through fable and myth.[17]

Hobbes knows this will be difficult: the requisite knowledge is not easily come by, and in ancient times, he says, it was more feasible to persuade people of absurd beliefs in gods than to know or teach a rational basis of civil obedience (*De Corpore* I.7). Is it likely to be more feasible in modern times—when people in general are so ignorant that "not one perhaps in ten thousand" understands the principles of government? (*Behemoth*, p. 169). Hobbes at any rate understands the appeal of myth and custom to many people, although in the end one wonders whether he underrates such factors. Hobbes even has his favorite ancient myths which he retells in the interests of his political principles—those of Ixion (*De Cive*, Author's Preface), of Prometheus (*De Cive* I.3 n.; *Leviathan*, Ch. 12, p. 70), and of the daughters of Pelias (*Elements* II.8.15; *De Cive* XII.13); these, we may assume, fall into the category of innocent adornment for his arguments.

More noteworthy is Hobbes' awareness of the ways in which religion has been used throughout time to support political institutions. He mentions a variety of appealing arguments for monarchy, for example, which rely on an analogy between a king and God; and he interpolates into *De Cive* an entire chapter of biblical quotations and parables supporting monarchy (*De Cive* X.3, XI). And he ascribes the motive of civil peace to the ancient founders of religions: "They have had a care, to make it believed that the same things were displeasing to the gods, which were forbidden by the laws" (*Leviathan*, Ch. 12, p. 76); in the past, in any case, there is no doubt that such nonrational supports were necessary to the state.

Closely related to myth as a potential source of stability in a commonwealth are the nonrational factors of custom and habit, which, Hobbes says, can render pleasing things that offend when new (*De Homine* XIII.3). Hobbes explains why, after the execution of the king, the abolition of the House of Lords, and the exclusion of those members who opposed the Ordinance of Non-Address, the then-sovereign Rump continued to refer to itself as "Parliament":

> Men may give to their assembly what name they please, what signification soever such name might formerly have had; and the Rump took the name of Parliament, as most suitable to their purpose, and such a name, as being venerable amongst the people for many hundred years, had countenanced and sweetened subsidies and other levies of money, otherwise very unpleasant to the subject (*Behemoth*, p. 360).

Hobbes thus recognizes that customary and emotive attachments, even to mere names, can be a force in politics. In marked contrast to later conservative theorists, however, he invariably sees them as disruptive forces which must be exposed and done away with by science, and never as sources of stability and peace. Hobbes points out that it was necessary to institute a sign (circumcision) so that Abraham and his descendants would remember their covenant with God (*De Cive* XVI.3), but he does not discuss a possible role for signs in perpetuating among citizens a memory of their civil contract. With obvious curiosity he devotes several pages to coats of arms and archaic titles as signs of honor (*Leviathan*, Ch. 10, pp.. 61-63), yet in general he makes little of the functions of such symbols in political life, except to say that they must fall under the authority of the sovereign. A disposition to rely on tradition and custom is said, like sedition, to follow from ignorance of causes (*Leviathan*, Ch. 11, p. 67). And when the common lawyer proposes that there ought to be in a polity a "reverend respect to customs not unreasonable," Hobbes' retort is that the claims of custom must stand or fall according to reason alone (*Dialogue*, pp. 58-63).

Hobbesian science then is directed with full force against all the mythical, customary, and symbolic elements which he realizes were once necessary foundations for states but which he regards in the present as hindrances to peace; but although Hobbes can expose the greater part of such elements as resting on various kinds of language abuse, he cannot offer any reasons to believe that their force among people is subsiding. The same can be said of another characteristic of human nature, the belief in "ghosts" and the inferences that people commonly draw from spiritual experiences for the conduct of their lives. Ghosts for Hobbes, as we have seen, are mere verbal absurdities, and alleged experience of them is ascribed to various freaks of sensation coupled with ignorance. And yet people, as Hobbes knows, seem to have a strong inclination to absurdities of this sort. Their natural curiosity, aided by imagination, leads them to seek causes, but their ability to account for everything materially is limited; meanwhile phantasms occurring in dreams or in "hallowed places" at night (though actually nothing but motions in the brain) are taken for subsisting things—especially by guilty, fearful, or superstitious men—and identified as ghosts (*De Corpore* XXV.9). And from the belief in ghosts there arises to challenge the civil sovereign a ghostly authority, "working on men's minds, with words and distinctions, that of themselves signify nothing, but bewray by their obscurity" (*Leviathan,* Ch. 29, p. 215). Hobbes' system depends on a "disenchantment" of the world, because otherwise the fear of violent death would remain secondary to the fear of ghosts, and the sovereign edifice would crumble; and Hobbes says that fear of other men is commonly greater than that of "spirits invisible" (*Leviathan,* Ch. 14, p. 92). Yet elsewhere Hobbes contradicts himself, conceding that the civil authority will always be troubled by the claims of a spiritual party, "because the fear of darkness and ghosts, is greater than other fears" (*Leviathan,* Ch. 29, p. 215).

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[17] But see Wolin (1970, pp. 4-8) and Oakeshott (1975, pp. 14, 150-54), who argue that Hobbes' political philosophy, despite its scientific pretensions, should be understood as an epical "thought-deed" or as a new myth, compelling in part because of Hobbes' artistry and skill with words.

Hobbes' ambition to ground the principles of civil authority in science is partially undermined, not only by the persistence of superstition and fear, but by his own admission that there are other means than science by which men can apprehend truth. Christians, he grants, must "acknowledge" things (such as angels, and the soul) which they cannot know (*Elements* I.11.5)—although he does not explore the political implications of this faculty of acknowledgement. And he even makes a place for mystery: there are some things in God's word, he says, which, being above reason, we are not meant to understand, but rather "we are bidden to captivate our understanding to the words" (*Leviathan,* Ch. 32, p. 242). Mystery, then, founded in words not expressive of conceptions, cannot be entirely abolished; and if people have and must have this capacity to be captivated by mystery in religion, there seems to be little reason to suppose that captivating and non-scientific language will cease to be efficacious in other areas of life. The belief in ghosts and the sense of mystery, then, are two further phenomena of human nature which diminish our confidence that Hobbes can successfully clear the field of political discourse of such abuses as elaborate and insignificant language.

It is, finally, possible to criticize the assumption, underlying Hobbes' advocacy of a public language whose key words would be authoritatively defined, that public meanings can ever significantly replace private ones. Hobbes insists, as mentioned above, that the sovereign must set down rules for the use of certain words, and in particular moral epithets, over which controversies arise. But even if the sovereign could enforce in public certain kinds of word usage, it seems unlikely that it could command unanimity of private understanding. Individuals' estimation of their own intelligence, and the diversity of their constitutions, are too great: "After the Bible was translated into English, every man, nay, every boy and wench, that could read English, thought they spoke with God almighty, and understood what he said. . ." (*Behemoth,* p. 190). And moreover, the state has no way of entering a person's mind and examining the meanings which he ascribes to words in private; Hobbes recognizes that the attempt to enforce uniformity of thought, apart from expression, would be futile: "A state can constrain obedience, but convince no error, nor alter the mind of them that believe they have the better reason" (*Behemoth,* p. 242). So it would seem that Hobbes must grant that private meanings will continue to exist, even after public meanings have been laid down, and this will remain a potential source of sedition and conflict. More important, however, is the fact that the sovereign cannot simply create a public vocabulary *de novo,* but must rather attempt to redefine words previously in use; and extant words, while they may be ambiguous and productive of quarrels, nevertheless have meanings. Hobbes may have a point in arguing that justice cannot be said to exist before there is a sovereign authority to constitute what is "just" by its actions, but this does not mean that the word "just" was not used prior to the creation of the sovereign. Nor does it seem correct to suppose that every individual used it in a different sense: more likely the private meanings of the word fell (and would continue to fall) into several categories, perhaps corresponding to factional divisions in society.[11] Thus, not only would private meanings persist after the institution of a public language, but so also would a non-official, and fragmentary, social vocabulary, the availability of which would constitute a continuing source of potential criticisms of authority.

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[11] If meanings were entirely private, of course, they would not be a political problem, since sedition requires the collective efforts of groups of people who understand one another's words.

Hobbes unwittingly provides examples of this possibility, again revealing insights into human nature that serve to weaken his case for political science. It may be established, for example, that "tyrant" is a meaningless term, and that any ruler by right is to be called "king"; otherwise, an "enemy" (*De Cive* XII.3). But how does one identify an "enemy"? It seems that only the terminology, and not the problem, has been changed. And will not equally emotive alternatives for "enemy" be available, if the need arise? Similarly, "just" and "unjust" have no private meaning: "Legitimate kings make things just by commanding them. . ." (*De Cive* XII.1). But will private judgment not be invoked to decide on the meaning of "legitimacy," or if not this word, then some term used in its definition? Even more strikingly, Hobbes allows that anyone may rightfully refuse to obey a "shameful" command of his sovereign, such as the command to kill one's parents, because a parricide would be "hated of all the world" (*De Cive* VI.13). Here Hobbes clearly implies that there exist private criteria for such a quality as "shamefulness," and that it is one on which there is common, though not public, agreement. Finally, there is the problem that, while a Hobbesian sovereign can do no injury or injustice, he can sin (by breaking the law of nature);[19] and while a subject cannot rightfully speak evil of a king, even if he sin (*Dialogue*, p. 139), it would seem that he can think evil of him, and that a standard of sin remains to society as a criterion of judgment over and above that of justice, even if it may not be publicly invoked.

Thus, notwithstanding his critical analysis of verbal disputes and their contribution to political controversy, and despite his hopes of replacing language abuses with the precision of the new political science, Hobbes gives us at the same time indications of the areas in which his enterprise is most vulnerable. If human nature were so tractable, or people so (potentially) rational, that Hobbes' project could succeed, it would probably not be necessary. Instead, however, politics and people's use of language in political life—together with the related pheomena that Hobbes deplores—show every sign of continuing to reflect the contentious features of human nature that Hobbes' writings capture so well.

Although he feared and attacked the proliferation of private opinions on political matters, Hobbes nevertheless, though a private man, sought to teach his fellow subjects "the grounds of their duty, and the reasons why calamities ever follow disobedience to their lawful sovereign" (*Behemoth*, p. 343). Hobbes did this because he believed he was living in a period of unusual disorder, when "reverence for antiquity" was newly declining (*Leviathan*, "A Review, and Conclusion," p. 466) and when common meanings had been lost in a storm of doctrinal conflicts. For us, however, there is nothing extraordinary about this state of affairs, which appears rather to be a permanent condition of modern political life. While Hobbes remains an illuminating guide to this aspect of politics, we must live with the fact, contrary to his hopes, that political philosophy's task of clarification in speech and reasoning is an endless one.

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[19] *De Cive* VII.14, XIII.16, XV.18. Less is made of this possibility in *Leviathan*, where the equation of sin and crime is dwelt upon.

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Black Consciousness and Political Participation: The Missing Link

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Recent research has demonstrated that black Americans are far more politically active than whites of similar socioeconomic status. The difference has been related to black consciousness. Yet the reasons for this relationship have not been adequately explained. Starting with the work of Gurin and Gamson, this article theorizes that black consciousness contributes to political mistrust and a sense of internal political efficacy which in turn encourages policy-related participation. The relationship between the two attitudes and policy-related behavior is demonstrated to be conditional. The conditions favor blacks more than whites. What I shall call the Gamson-Gurin thesis is supported by data from Verba's and Nie's 1967 survey of the American public. The thesis, and its derivations, prove useful in clarifying the scope and nature of black participation in the American political process as well as helping us to understand how individuals in general select one mode of participation over another and how the choice varies by race and social class.

Low-income groups are generally politically inactive. This is particularly true of more demanding forms of political participation, such as campaign work and organized community activities which require scarce human resources and high levels of individual initiative (Verba and Nie, 1972, pp. 133-37; Milbrath and Goel, 1977, pp. 43-74). The poor generally lack the ability and the motivation to participate in politics. Yet, despite their over-representation among the poor, black Americans largely overcome these disadvantages. A large number of studies, using a variety of indicators and samples, report black participation at a level significantly above that which would be expected, given blacks' average level of education, income and occupational prestige, and at a higher level that that of whites of similar status.[1] The evidence suggests a level of black political participation since the 1960s which is historically unprecedented in black history and which rivals, if it does not surpass, that of other major groups in American society.

The incidence of greater black political participation is not uniform. It varies by type of activity and region and, in some studies, it is more prominent among women than men. The traditionally low black voter registration and voter turnout in the South, which continues to be lower than the national average, and the steady decline in black voting outside the South since its zenith in 1964 are major exceptions (Cavanagh, 1979, pp. 11-14). So too is the reluctance of blacks to engage in one-on-one personal contacts with public officials (Verba and Nie, 1972, pp. 164-71). Nevertheless, blacks are more politically active than whites of comparable socioeconomic status in a wide range of activities which have a considerably more demanding and more significant impact on the participants' lives and the local political process.

The most visible forms of black activism are the unconventional confrontation tactics, both nonviolent and violent, which jarred the national consciousness in the 1960s. However, the attention such tactics have received has concealed a much more prevalent involvement among blacks in conventional politics at the local level. To a degree disproportionate to their numbers, blacks have been involved in numerous community activities —including Community Action Agencies, Model City and Head Start programs, civil rights organizations, block associations, neighborhood development projects, tenant associations, PTA and related neighborhood school groups—as well as more overtly partisan activities such as belonging to political clubs, attending political meetings and rallies, donating money and time to partisan campaigns, and attempting to influence the opinions

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The data used in this article were made available in part by the Inter-University Consortium for Political and Social Research. The data were originally collected by The National Opinion Research Center. Neither the original source or collectors of the data nor the Consortium bears any responsibility for the analyses or interpretations presented here.

[1] Much of the research on black Americans is based on case studies covering a wide variety of people, places and times. Sources reporting a higher level of participation among blacks include: Babchuk and Thompson, 1962; Matthews and Prothro, 1966, pp. 49-50; Orum, 1966; Olsen, 1970; Zurcher, 1970, p. 103; Verba and Nie, 1972, pp. 151-60; Aberbach and Walker, 1972, pp. 30-34; Williams et al., 1973, p. 13; Greenberg, 1974, p.. 42 f.; Antunes and Gaitz, 1975; Steggert, 1975: Clemente and Sauer, 1975; London, 1975; Lamb, 1975, p. 109; McPherson, 1977: Cohen and Kapis, 1978; Klobus-Edwards et al., 1978.

of others. Some of this participation involves electoral politics. More of it centers around civic projects at the neighborhood level and administrative politics involving dealings with agencies which serve the poor. Much of it has come about as the result of efforts toward political decentralization and community self-government in the 1960s (Lamb, 1975, pp. 4-5).

Verba and Nie (among others) explain the overrepresentation of black Americans among political activists in terms of "black consciousness," the awareness among blacks of their shared status as an unjustly deprived and oppressed group (Verba and Nie, 1972, pp. 160-61; Verba, Ashmed and Bhatt, 1971, pp. 25-26). They demonstrate that blacks who are aware of their oppression are much more active than blacks who are not, and more active than whites of similar socioeconomic status. Even more impressive is the Verba-Nie finding that blacks with a sense of black consciousness participate in campaign and cooperative activities at a rate higher than that of the whites in their sample, despite the clear socioeconomic advantage of the latter. Verba and Nie also identify an increase in black political participation during the 1960s (p. 252 f.), a period in which blacks become increasingly more aware of the illegality of their oppression.

Overall, the empirical case made by Verba and Nie establishing a relationship between black consciousness and political participation is impressive. Yet their theoretical explanation of the relationship, which relies primarily on Olsen (1970), is deficient. It leaves three fundamental questions unanswered.

First, Verba and Nie do not adequately explain why blacks specialize in certain types of activities and not others. Their argument that black consciousness contributes only to collective political action is not supported by their data (Verba and Nie, 1972, p. 164; Olsen, 1970, pp. 692-94). The fact that blacks continue to experience intimidation and a sense of futility when voting and when trying to contact public officials (Verba and Nie, 1972, pp. 164-71) also does not provide a complete answer because it only accounts for why blacks participate in some political activities at a level below that of whites. It does not tell us why they are considerably more active than comparable whites in other areas such as campaign work and community projects. Campaign work and community activities are not necessarily segregated, so neither can the answer lie in the explanation that blacks are compensating for exclusion from white-dominated activities by overparticipating in all-black activities. Second, why do blacks emphasize *political* solutions to their problems? Olsen's explanation, that the black community stresses "participatory norms," group pressure to participate in politics to achieve collective goals, simply puts the question back one step. Why do blacks, but not poor whites, stress participatory norms? What has politicized them? Third, what about poor whites who are politically active? Do they require a different set of motivations to participate in politics? Or does black consciousness give blacks a greater impetus to participate because it contributes to a common set of attitudes which motivate all poor people, regardless of race?

Answers to these questions require that we specify the psychological state linking black consciousness with political involvement. It is the purpose of this article, in drawing upon the work of Patricia and Gerald Gurin and William Gamson, and in extending the analysis of Verba's and Nie's data, to develop such a link.

The Missing Link: Political Mistrust and a Sense of Political Efficacy

The thesis of this article is that the primary reason black consciousness has such a dramatic effect on political participation is that it contributes to the combination of a sense of political efficacy and political mistrust which in turn induces political involvement. The development of this line of reasoning requires that a relationship be established between black consciousness and the combination mistrust and sense of efficacy, on the one hand, and between the combination mistrust and sense of efficacy and political involvement on the other.

It has been established that various mixes of the sense of political efficacy and political trust contribute to different types of political activity (Gamson, 1968, pp. 46-48; Muller, 1977; Milbrath and Goel, 1977, pp. 57-74). The consensus is that a high sense of political efficacy encourages active political involvement: *allegiant activity* through conventional modes of access to government when political trust is also high; critical *protest activity* of an unconventional, nonviolent or violent nature when trust is low. Low levels of the sense of political efficacy discourage political participation, leading to *complacent inactivity* when trust is high and *estranged inactivity* when trust is low.

Gamson (1968, p. 48) contends that the optimal combination for political mobilization is low political trust and a high sense of political efficacy. What underlies his argument is the model of rational actors who decide to act, in part, on the basis of whether they perceive a possible benefit for their effort and what they believe to be the likelihood of success (Riker and Ordeshook, 1973, pp. 45-52). Gamson describes the absence of political trust as providing the "need to act" and the sense of political efficacy as the perceived "ability to act." He states that mistrusting or cynical individuals who nevertheless have a strong sense of political efficacy have both the motivation (the belief that public officials will not be responsive if left to their own devices) and the confidence (the belief that action taken to make public officials responsive will be effective) which are necessary for active political participation. As we shall see, the logic is oversimplified; but, with several refinements, it can be extended to cover a wide range of political activities. Henceforth I shall refer to the principal assertion of Gamson's thesis as the "mistrust-sense of efficacy" hypothesis.

The link between black consciousness and the combination of mistrust and the sense of efficacy is addressed by Patricia Gurin and associates (1969, 1975). We live in a society in which most people accept the Protestant work ethic, the idea that each individual is responsible for his or her own successes and failures in life. This ethic weighs particularly heavily on the poor and racial minorities at the bottom of the socioeconomic hierarchy who traditionally have been shackled with guilt for their poverty. The essence of Gurin's work is the generalization that, if deprived individuals can successfully externalize this guilt, transferring responsibility for their lot from the self to "the system," they will acquire a healthier, improved self-image along with a more critical, cynical view of the system. By drawing attention to their status as a deprived and oppressed group, black consciousness helps blacks to replace the more negative overtones of the Protestant work ethic with a "system-blame" ethic and complete such a transfer. The result is a heightened sense of personal efficacy and an increased mistrust of the political system. This logic is empirically supported by concomitant increases during the 1960s in black self-esteem, self-confidence and political cynicism (Hulbury, 1975: Yancey et al., 1972; ISR, 1973).

The Conditional Relationship between Mistrust-Sense of Efficacy and Participation

Though the work of Gamson and Gurin provides a sound basis for establishing a theoretical link between black consciousness and black political participation, a problem remains. An impressive accumulation of research supports the original mistrust-sense of efficacy hypothesis for acts of political protest; but, so far as I am aware, only for unconventional acts of protest of a violent or nonviolent nature.[1] Rarely do these studies test the hypothesis for purely conventional forms of political participation of the type examined by Verba and Nie, activities such as voting in elections, working in political campaigns or more routine involvement in community politics. More perplexing, those that do, have not substantiated the hypothesized relationship (Muller, 1977, p. 460; Hawkins et al., 1971; Frazer, 1970).

The hypothesis is supported by limited evidence because the three variables—sense of efficacy, trust and participation—have not been recognized as multifaceted, global concepts having different relationships with one another depending upon which facets or components are involved. We should thus identify key components and clarify these relationships before specifying the conditions under which the sense of political efficacy and political mistrust jointly effect conventional participation. Several key assumptions require close attention. The assumptions will be discussed under five topic headings, each heading specifying the points in contention.

1. *Allegiant Behavior Versus Policy Behavior.* There is no reason to believe that political mistrust and the sense of political efficacy have the same relationship with all modes of political activity. A classification of activities is needed. Two criteria are important. The first is the amount of *individual initiative* required for the activity. It is generally agreed that actions like flying the flag, voting and discussing politics with friends require

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[1] Some typical works are: Paige, 1971; Millbrath and Goel, 1977, pp. 68-74; Caplan, 1970; Crawford and Naditch, 1970; Forward and Williams, 1970; and Sanger and Alker, 1972. There are, however, what I interpret to be confirmations of the hypothesis for conventional participation from a related body of literature dealing specifically with system blame and the sense of personal efficacy. In his analysis of the Detroit area study, Aberbach (1977, p. 558) reports that individuals who blame their lack of political efficacy on the political system are more active in *both* conventional and unconventional political activities than those who blame it on their own personal inadequacies. Miller et al. (1978, pp. 13-14) find an interactive effect on political participation between "group identification" (related to the group consciousness predicted here to contribute to internal political efficacy) and blaming the system. Klobus-Edwards et al. (1978) find blacks who blame the system are more active in four out of five modes of activity, one of which is an index of "organized involvement" which includes political and civil rights organizations.

Though Klobus-Edwards et al. find dissimilar results for blacks on an index of "political involvement" (which includes voter registration and turnout) and for higher-status whites on all indices of participation, their results are consistent with the thesis to be developed herein.

considerably less initiative and effort than most forms of campaign work, community involvement and violent protest.

The second criterion is the *purpose* of the activity. The distinction made by Almond and Verba between participant and subject orientations is pertinent here (1963, pp. 13-19). People with only subject orientations make no effort to influence policy other than to support it by their compliance. For them, "good citizenship" typically means obeying the law and showing other displays of loyalty to their regime or country. Activities which fit this mode will be called *allegiant behavior.* The participant orientation entails an active and vigilant role in the policy-making and policy-implementation process. "Good citizenship" is interpreted in terms of popular sovereignty and entails the right and the obligation to exercise influence in public affairs. Any attempt to affect the election or appointment of public officials or the actions of public officials for the purpose of influencing the policy-making or policy-implementation process will be labeled *policy behavior.*

Table 1 presents a partial inventory of political activities cross classified by purpose and level of initiative. The proper classification of activities is sometimes ambiguous. This is owing in part to the occasionally thin line between respondents' loyalty to their regime or country and disagreements over government policy (for example, during the Vietnam War people tended to confuse disputes with a government policy with disloyalty to country). It is also owing to the multiple purposes served by some activities such as voting. Although Verba and Nie (1972, p. 52) and Milbrath and Goel (1977, p. 12) consider voting to be largely a low-initiative allegiant activity, it must be recognized that the meaning and difficulty of the act vary, depending upon the individual.[3] Because

Table 1. Classifying Political Activities by Their Purpose and by the Level of Initiative Required to Implement Them

	PURPOSE	
INITIATIVE	Allegiance to Regime and Country	Influence on Government Policy
HIGH	*a* 1. Active participation in patriotic rallies 2. Volunteering for combat duty in the armed forces 3. Individual acts of heroism by citizens, policemen, firemen, or soldiers in support of community or country 4. Nonviolent mass demonstrations in support of country or regime 5. Violent actions (lynching, riots, etc.) intended to display support for the nation and opposition to perceived enemies	*b* 1. Active efforts to educate oneself on the issues for the purpose of casting an informed vote or swaying the opinions of others 2. Involvement in political clubs 3. Active participation in political campaigns 4. Active lobbying of public officials 5. Running for party or public office 6. Active involvement in community organizations concerned with public issues 7. One-on-one contacts with public officials 8. Nonviolent direct action protest against specific government policies 9. Violent protest against specific government policies
LOW	*c* 1. Flying the flag or displaying other symbols of patriotism 2. Passive compliance with the law 3. Deference to government authorities responsible for implementing the law 4. Passive attendance at patriotic rallies, parades 5. Voluntary enrollment in the nation's armed forces 6. Verbal support of country or regime 7. Voting as a symbolic gesture of regime loyalty	*d* 1. Wearing a campaign button 2. Discussing political issues with friends 3. Voting as a choice among competing alternatives

Source: Compiled by the author.

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[3] Voting can be credibly assigned to any one of three cells in Table 2. For many Americans it is a low-initia-

voting cannot be decisively classified, it is impossible to predict which combination of trust and efficacy will optimize its occurrence. Studies which have failed to substantiate the mistrust–sense of efficacy hypothesis for conventional participation are largely based upon voting data and cannot be interpreted as a general refutation of the hypothesis.

Applying this classification to modify Gamson's thesis, we can expect that, first, political efficacy is conducive to high-initiative activities and, second, trust contributes to allegiant behavior, whereas mistrust encourages solely policy behavior. Therefore the relationship between mistrust–sense of efficacy and participation is expected to be limited to *high-initiative policy behavior.* This is exactly the kind of political behavior in which black Americans have been prominently involved.

2. *Internal Political Efficacy Versus External Political Efficacy.* The distinction between internal and external efficacy (Balch, 1974) has been ignored in past tests of the mistrust–sense of efficacy hypothesis. A sense of political efficacy is the expectation that one's political activity will be successful. A *sense of internal political efficacy* is that portion of a person's sense of political efficacy which is perceived to stem from one's own internal strengths or weaknesses. A *sense of external political efficacy* is the portion of the sense of political efficacy which is believed to stem from environmental forces external to the individual that influence the ease or difficulty of successful political action. *Political trust,* which is faith in the ability and will of government to respond to peoples' needs, is one component of the sense of external political efficacy, referring to the regime, institutions and public officials—in other words, to what is collectively referred to as "government." Empirical tests of the mistrust–sense of efficacy hypothesis which ignore the distinction between internal and external efficacy are confounded by the fact that political trust and external political efficacy are related by definition. Gamson's proposition will accordingly be renamed the *mistrust–sense of internal efficacy hypothesis.*

3. *Political Trust: Perceived Need to Act versus the Perceived Ability to Act.* Political trust influences not only the perceived "need" to act, pointed to by Gamson, but also the "perceived ability" to act. Persons who have a high sense of internal political efficacy will be even more confident if they also have a high sense of external political efficacy; that is, if they believe that the political environment is conducive to success (in the case of political trust), that those to whom they seek access are competent and responsive. Most forms of allegiant behavior comprise low-initiative, largely symbolic activity which can be sustained by political trust alone.

4. *The Role of Trust in Conventional Participation versus Its Role in Unconventional Participation.* Individuals may trust one segment of the political system (e.g., incumbents, institutions, regime principles, and the political community) while withholding support from others (Gamson, 1968, pp. 49-50; Muller and Jukam, 1977). As a rule, tests of the mistrust–sense of efficacy hypothesis have ignored this distinction. A global, undifferentiated mistrust is typically said to result in unconventional protest. This assumption does not square with the fact that as long as one has even minimal faith in existing political institutions, it is much less costly to protest against government policy through traditional, conventional modes of participation than it is to engage in unconventional demonstrations of a violent or nonviolent nature. Muller (1972) and Muller and Jukam (1977) have demonstrated that individuals are only likely to resort to violence when all conventional modes of access to the policy-making process are blocked or futile, when the entire system (as opposed to an incumbent administration) is perceived to be unresponsive and incapable of reform and when violence is perceived to be an effective alternative political tactic. Therefore we may expect political mistrust–sense of internal political efficacy to contribute to both conventional and unconventional policy behavior, but more typically to the former.

5. *Ceteris Paribus versus the Presence of Other Motivations.* Much of the relevant literature implicitly assumes that mistrust provides the only necessity to act, that "everything else is equal." At least for most conventional forms of policy behavior, this is clearly incorrect. Various motivations encourage political involvement—civic duty, partisanship, interest in politics, material self-interest, and ideological principles (Verba and Nie, 1972, pp. 82-88; Milbrath and Goel, 1977, pp. 43-85). Mistrust is neither necessary nor sufficient. Consequently, for citizens who are *already politically motivated,* the combination which maximizes an overall sense of political efficacy (high political trust and a strong sense of internal political efficacy) will also maximize high-initia-

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tive, symbolic act intended to display patriotism and good citizenship (cell *c*). For many others it is an attempt to influence policy by choosing among candidates who are perceived to offer viable alternatives. Voting is also one of the easiest ways to attempt to influence the policy process (cell *d*); but, for those who take it seriously, responsible voting takes a considerable amount of time and effort informing oneself about issues, party platforms and the candidates' relative merits (cell *b*).

tive political involvement of both an allegiant and a policy nature. It is only among those groups, such as the poor, who are normally *not otherwise politically motivated* that the combination of political mistrust and a high sense of internal political efficacy optimizes political behavior, and then only policy behavior. Among these people mistrust is a necessary condition for participation in the policy-making and policy-implementation process.

The following six generalizations summarize the discussion refining Gamson's original hypothesis by specifying the conditions under which political trust and the sense of internal political efficacy contribute to political participation. The relationships specified by the six generalizations are displayed in Table 2. The table presents two models: the first assumes no other motivation to participate; the second assumes the existence of other motivations. The symbols in parentheses accompanying each generalization refer to the corresponding model and cell in Table 2.

For our purposes, the key generalization is the last: *The combination of political mistrust and a sense of internal political efficacy optimizes high-initiative, conventional policy behavior for otherwise unmotivated citizens.* Low-income groups are poorly motivated. However, because of the prevalence of black consciousness, black Americans are more likely than other low-income groups to feel personally efficacious, but cynical about politics; therefore they are more likely to participate actively in conventional policy-making and policy-implementation processes.

Summary of Generalizations:

1. Ceteris paribus, a high sense of internal political efficacy encourages *high-initiative* political participation (I a, b; II a, b); the absence of a sense of internal political efficacy discourages such activity (I c, d; II c, d). One's sense of internal political efficacy is of less consequence for *low-initiative* political participation.
2. Ceteris paribus, high levels of political trust contribute to *allegiant behavior* (I a, c; II a, c); mistrust contributes to *policy behavior* (I b, d; II b, d).
3. When citizens limit their cynicism to a low opinion of public officials, they are likely to rely solely on *conventional* modes of political par-

Table 2. The Joint Effects of the Sense of Internal Political Efficacy and Political Trust on Different Modes of Political Participation, Controlling for Prior Motivations

Model I: Given no other motivation to participate

		Trust in Government	
		High	Low
Internal Political Efficacy	High	*a* Optimal High-initiative allegiant behavior	*b* Optimal high-initiative allegiant behavior
	Low	*c* Low-initiative allegiant behavior	*d* Total withdrawal from conventional politics

Model II: Given prior motivation to participate

		Trust in Government	
		High	Low
Internal Political Efficacy	High	*a* Optimal high-initiative allegiant and policy behavior	*b* High-initiative allegiant and policy behavior
	Low	*c* Low-initiative allegiant and policy behavior	*d* Total withdrawal from conventional politics

Source: Compiled by the author.

ticipation. When they extend their cynicism to the entire political regime, they are more likely to use *unconventional* tactics. Whether the unconventional tactics are violent or nonviolent depends upon the citizens' assessment of the relative efficacy of violence.

4. Regardless of motivation, individuals who have absolutely no sense of political efficacy, who have neither a sense of internal political efficacy nor political trust, withdraw from conventional politics altogether (I d, II d).
5. For otherwise *motivated individuals,* conventional participation of either a policy or allegiant nature occurs when either a sense of internal political efficacy or political trust is present (II, a, b, c). The optimal combination for high-initiative behavior of either type is a strong sense of internal political efficacy and high political trust (II a).
6. For otherwise *unmotivated individuals,* the optimal combination for high-initiative conventional policy behavior is a high sense of internal political efficacy and low political trust (I b). The optimal combination for high-initiative allegiant behavior is a high sense of internal political efficacy and high political trust (I a). Trusting citizens who have little sense of internal political efficacy have no basis for participation in other than low-initiative allegiant activities (I c).

An Empirical Test

If the foregoing discussion, merging the work of Gurin and Gamson, provides a successful explanation of the link between black consciousness and conventional policy behavior, then the following *working hypotheses* should be supported by Verba's and Nie's data: (1) poor blacks willl be more likely than poor whites to possess the combination of mistrust and sense of internal efficacy; (2) the advantage will be limited to blacks who display a sense of black consciousness; (3) among the poor (whether black or white), those who display the combination will participate at a higher rate in the high-initiative conventional policy activities studied by Verba and Nie—campaign and communal participation—than those who do not.

There is no reason to expect the Gurin-Gamson thesis to pertain to individuals of higher socioeconomic status. Generalization 5 indicates that Gamson's hypothesis has no relevance for higher-status, higher-motivated individuals. Gurin's hypothesis does not apply because the transition from blaming oneself to blaming the system for one's plight does not pertain to economically successful blacks. Those who have not failed have no need for blame and therefore nothing to transfer. Nevertheless, in the following analysis I will display the data for both poor and non-poor. Data on the non-poor will provide a frame of reference for understanding the poor and serve as a resource for those interested in a more general theory of participation.

Measurement. The operational definitions for black consciousness and political participation have been replicated from Verba's and Nie's *Participation in America.*[4] *Black Consciousness* is indicated by whether blacks voluntarily raise the issue of race in response to a series of open-ended questions inquiring as to the presence of any conflict within their communities or any problems they perceive in their personal lives, the community or the nation. The spontaneous mention of racial problems and racial conflict among black respondents is treated as an indicator of the potential saliency and importance of racial conflict as an explanation of personal failure. Activities included in the *campaign index* entail persuading others how to vote, working in a campaign, attending political meetings or rallies, contributing money to campaigns, and membership in political clubs. The *communal index* includes efforts to solve community problems by working with others, helping to form groups or organizations and the initiation of personal political contacts within the community. The two indices of participation have been combined into a single *high-initiative conventional policy behavior index* which has been standardized to have a mean equal to zero and standard deviation equal to one.[5]

Composite measures of the *sense of internal political efficacy* and *political trust* have been created by selecting relevant variables in the study and submitting them to several stages of factor analysis, using Thurston's most simple structure criterion. Several weakly correlated or ambiguous items have been discarded; other nearly identical items are combined. The final two-factor solution is presented in Table 3.[6] Items 1 and 5 are composite indices in which the responses to identically worded questions repeated for local, state and national politics are standardized and added together. The first factor is comprised of those questions which best measure a sense of internal political efficacy, each posing the question of political efficacy in terms of a personal pronoun, either referring to the individual or to people like the individual. The second factor contains questions which most distinctly measure political trust, addressing the ease of access to, and the responsibility of, government officials. Weighted composite indices have been reconstructed for each factor and then standardized. The factors are orthogonal, having a Pearson correlation of −.03.

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[4] The full weighted sample of 3095 and the scale construction for the political participation factors and race consciousness are described in the appendices of *Participation in America* (Verba and Nie, 1972). The analysis that follows was repeated for the unweighted sample and no important differences were identified.

[5] A previous report on this project (Shingles, 1978) indicates identical results when the campaign and communal indices are tested separately. For this analysis the 10 individual items comprising the two scales are weighted by their factor scores on the first unrotated factor, added together and then restandardized. Unlike Verba and Nie, I used all the items from the "communal" factor. In their analysis of black consciousness, they exclude the personal contact items because they wish to examine only "cooperative" or group-related activities. The emphasis in this analysis on high-initiative policy behavior requires their inclusion. Their inclusion does very little to alter the relationship with black consciousness reported in *Participation in America.*

Findings for High-Initiative Conventional Policy Behavior.

WORKING HYPOTHESIS 1: *Among the poor, blacks are more likely than whites to possess the combination of political mistrust and a sense of internal political efficacy.* Table 4 presents average standardized scores measuring the sense of internal political efficacy and political trust for each race, controlling for income.[7] As hypothesized, poor blacks are more likely than poor whites to possess the combination of mistrust and a sense of internal efficacy; in fact, they are much more likely to.

Average scores for whites do not deviate very

Table 3. Rotated Factor Loadings* for the Questions Measuring Sense of Internal Political Efficacy and Political Trust

Item	Internal Political Efficacy	Political Trust
1. Two individual efficacy questions combined: Suppose the most powerful people in all of the United States (this community) were at the top of this ladder and the least powerful were at the bottom. . . . Where would you place yourself in terms of power?	.78	−.18
2. Thinking about the important local issues in this community, how well do you feel you understand them?	.63	.18
3. How much influence do you think people like you can have over local government decisions?	.61	.38
4. If you had some complaint about a local government activity and took that complaint to a member of the local government council, how much attention would you expect him to pay to what you say?	.34	.60
5. Three government comprehension questions combined: In general, how well do you think the local (state, national) government understands the needs of the people in this community (state, country)?	.08	.49
6. Suppose you had some problem to take up with an important local official in local government . . . but you did not personally know this official—would it be necessary to go through some connection to approach the official or could you approach him directly? (If a connection is needed) could you easily find some such person who could help you make such a contact?	−.09	.73

Source: Compiled by the author from a 1967 survey of a representative sample of Americans 21 years or older conducted by the National Opinion Research Center.

*Principal component analysis using a varimax orthogonal rotation.

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[6] Exact wording of the questions and their frequency distributions may be found in the ICPSR codebook, *Political Participation in America* (ICPSR 7015). Variable numbers of the items included in the final factor analysis in order of their appearance are v102, v106, v155, v156, v160, v307, v308, v309, v303, v304.

[7] Writing in 1965, Gunnar Myrdal estimated that approximately two-fifths of the American population lived in poverty. The closest coded value to Myrdal's estimate in the 1967 Verba and Nie index of annual family income before taxes is $5,000.. Thirty-seven percent of the sample report incomes of $5,000 or below. This closely approximates the 1968 median family income of $5,200 for families of Target Neighborhoods reported in the OEO funded Local Change Study (Lamb, 1975, p. 59). Therefore "non-poor" are defined as those earning annual family incomes before taxes of over $5,000, "poor" as those earning incomes of $5,000 or less.

much from the population mean, since whites comprise 86 percent of the sample and largely define the mean. Nevertheless, whereas economically better-off whites as a group are slightly above average in their sense of internal efficacy (.09) and trust (.14), the combination which provides them with the greatest incentive to participate, poor whites fall below average on both (−.18, −.19), feeling mildly incompetent and mildly cynical. They consequently have little reason to engage in high-initiative policy behavior. In contrast, the differences for blacks are startling. Regardless of class, blacks have a much higher sense of internal political efficacy than whites, so much so that poor blacks score higher (.36) than moderate- to high-income whites (.09). Better-off blacks score even higher (.53), a half a standard deviation above the population mean. Equally evident is the fact that blacks, regardless of income, harbor extremely cynical views of government, scoring approximately two-thirds of a standard deviation below the national average (−.67, −.64).

The uniform distribution of black political cynicism across classes is not predicted by the Gurin-Gamson thesis, yet it requires little explanation. Black Americans are the victims of a long history of exploitation, discrimination and exclusion from power. In publicizing the black plight, political leaders and a free press have legitimized and intensified black discontent (Hraba and Siegman, 1974). No matter how the black sample is subdivided, mistrust appears to permeate black society (Abramson, 1972; Laurence, 1970).

Even more impressive is the magnitude of black internal political efficacy. How can blacks be so cynical about the responsiveness of government to black people and yet retain so much confidence about their own ability to exercise political influence? The answer lies in black consciousness.

WORKING HYPOTHESIS 2: *Black consciousness contributes to the combination of political mistrust and a sense of internal political efficacy.* Table 5 presents average standardized scores measuring sense of internal political efficacy and political trust for blacks who indicate a sense of black consciousness and those who do not, controlling for income. The hypothesis is confirmed. Among the poor, the combination mistrust–sense of internal efficacy is limited solely to blacks with a sense of their common plight and oppression as a race. Once again, cynicism is uniform across all black groups. However, poor blacks with black consciousness have a remarkably strong sense of internal political efficacy (two-thirds of a standard deviation above the population mean), higher than any other group in the analysis. In stark contrast, poor blacks without black consciousness average no higher in their sense of internal efficacy than the poor whites in Table 4 (−.13 versus −.18). In short, black consciousness accounts for *all* of the differences in the sense of internal political efficacy between poor blacks and poor whites.

WORKING HYPOTHESIS 3: *Political mistrust–sense of internal political efficacy contributes to high-initiative conventional policy behavior.* Part I of Table 6 presents average rates of high-initiative conventional policy behavior (HICPB) for low and moderate-to-high income respondents. Part II breaks the averages down further for different combinations of political trust and sense of internal political efficacy. A two-way analysis of variance (ANOVA) is presented in Part III of the table to provide a test of statistical significance. The multiple correlations in Part IV provide a convenient summary of the strength of the relationship between attitudes and participation.[8]

Table 4. A Comparison of Whites' and Blacks' Average Standardized Scores on the Indices of Sense of Internal Political Efficacy and Political Trust, Controlling for Annual Family Income

	Low Income		Moderate-to-High Income			
	Whites	Blacks	Whites	Blacks	*F*	Sig.
Internal Political Efficacy	−.18	.36	.09	.53	27.033	***
Political Trust	−.19	−.67	.14	−.64	60.290	***
(N)	631	183	1447	117		

Source: Compiled by the author from a 1967 survey of a representative sample of Americans 21 years or older conducted by the National Opinion Research Center.

***$p < .001$

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[8] There are any number of polynomial equations, representing different degrees of interaction which conform to the hypothesis, making the choice of any one model using multiple regression difficult and arbitrary.

Table 5. A Comparison of Average Standardized Scores on the Indices of the Sense of Internal Political Efficacy and Political Trust for Respondents Who Manifest Black Consciousness and Those Who Do Not, Controlling for Annual Family Income—Black Respondents Only

	Black Consciousness					
	Low Income		Moderate-to-High Income			
	Indicated	Not Indicated	Indicated	Not Indicated	*F*	Sig.
Internal Political Efficacy	.66	−.13	.59	.43	6.904	***
Political Trust	−.59	−.80	−.64	−.60	.485	NS
(N)	112	71	78	39		

Source: Compiled by the author from a 1967 survey of a representative sample of Americans 21 years or older conducted by the National Opinion Research Center.

***$p < .001$
NS = not significant

Table 6. A Comparison of Average Standardized Scores on the Index of High-Initiative Conventional Policy Behavior for Respondents of Varying Combinations of Sense of Internal Political Efficacy and Political Trust, Controlling for Annual Family Income

	Low Income[a]		Moderate-to-High Income[b]	
I. Average Standardized HICPB Scores:	−.22		.08	
	Political Trust		Political Trust	
	High	Low	High	Low
II. Average Standardized HICPB Scores by Trust and Internal Efficacy:				
Internal Political Efficacy				
High	−.19	.06	.29	.09
Low	−.26	−.45	−.07	−.22
	F Value	Sig.	*F* Value	Sig.
III. Test of Statistical Significance (ANOVA):				
Main Effects	9.500	***	36.760	***
Internal Political Efficacy	18.897	***	54.754	***
Political Trust	0.320	NS	13.255	***
Interactive Effects	14.326	***	0.217	NS
Total Effects	11.109	***	24.579	***
IV. Multiple Correlation, $R_{y.x_1x_2(x_1x_2)}$:[c]	.33		.39	

Source: Compiled by the author from a 1967 survey of a representative sample of Americans 21 years or older conducted by the National Opinion Research Center.

[a] Total number responding = 1097. Broken down by cell: $a = 246$; $b = 183$; $c = 232$; $d = 398$.

[b] Total number responding = 2016. Broken down by cell: $a = 320$; $b = 348$; $c = 497$; $d = 796$.

[c] y = HICPB; x_1 = sense of internal political efficacy; x_2 = political trust; and (x_1x_2) = their joint effect.

***$p < .001$
NS = not significant

Hypothesis 3 is confirmed. Though in both income groups the magnitude of the multiple correlation between respondents' attitudes and their reported activity are similar, the nature of the associations differ. Among the poor, it is those who mistrust government but feel they can do something about it who are the most involved in high-initiative, conventional policy activities (.06). They participate at a rate equal to that of the average citizen and significantly above that of the poor as a whole (-.22). In short, the combination of mistrust and a sense of efficacy completely overcomes the disadvantages of class. In contrast, among better-off, more motivated respondents, the optimal combination which produces the same behavior is high trust and a strong sense of efficacy (.29). Another way to look at the same relationship is to observe the results of ANOVA. The *F* value for interactive effects (low trust–high sense of internal efficacy) among the poor (F = 14.326) is statistically significant; the *F* value for the non-poor (F = 0.217) is not.

An Aside on the Black Middle Class. Although the Gurin-Gamson thesis does not pertain to moderate-to-high income groups, the small black middle class deserves attention. Their political orientations—in terms of the distribution of political trust and the sense of internal political efficacy (Table 4) and the relation of both to political participation (Table 7)—are very similar to those of

Table 7. A Comparison of Average Standardized Scores on the Index of High-Initiative Conventional Policy Behavior for Respondents of Varying Combinations of the Sense of Internal Political Efficacy and Political Trust, Controlling for Race–Moderate to High Income Respondents Only

	Moderate-to-High Income Blacks[a]		Moderate-to-High Income Whites[b]	
I. Average Standardized HICPB Scores:	.06		.09	
	Political Trust		Political Trust	
	High	Low	High	Low
II. Average Standardized HICPB Scores by Trust and Internal Efficacy:				
Internal Political Efficacy				
High	–.03	.46	.32	.03
Low	–.19	–.31	–.07	–.21
	F Value	Sig.	*F* Value	Sig.
III. Test of Statistical Significance (ANOVA):				
Main Effects	4.988	**	36.702	***
Internal Political Efficacy	7.480	**	48.011	***
Political Trust	2.644	NS	18.434	***
Interactive Effects	2.875	NS	1.694	NS
Total Effects	4.284	**	25.032	***
IV. Multiple Correlation, $R_{y.x_1x_2(x_1x_2)}$:[c]	.36		.40	

Source: Compiled by the author from a 1967 survey of a representative sample of Americans 21 years or older conducted by the National Opinion Research Center.

[a] Total number of respondents = 157. Broken down by cell: a = 48; b = 54; c = 24; d = 29.

[b] Total number of respondents = 1839. Broken down by cell: a = 740; b = 291; c = 469; d = 288.

[c] y = HICPB; x_1 = sense of internal political efficacy; x_2 = political trust; and (x_1x_2) = their joint effect.

**$p < .01$

***$p < .001$

NS = not significant

The results of the most parsimonious equation, $(y = B_0 + B_1X_1 + B_2X_2 + B_3(X_1X_2)$, are presented in Part IV of Tables 6, 7 and 8.

Whereas trust and efficacy are dichotomized for ANOVA, they are left uncollapsed in the calculation of the multiple correlation.

the black lower class. Like the black poor, but even more so, they have an impressively strong sense of internal efficacy, significantly stronger than that of whites of comparable status. There is no reason, however, on the basis of the Gurins' work, to expect black consciousness to explain the black middle-class' sense of internal efficacy, and it does not (Table 5). Middle-class blacks feel internally efficacious politically regardless of their level of consciousness. The very type of person who can "make it" as a black in a predominantly white society must have tremendous confidence in his or her own abilities in order to get there in the first place and then again as a result of having made it. Yet, despite their confidence in their individual abilities to succeed in politics, middle-class blacks are as skeptical about the willingness and ability of government to respond to black needs as the majority of their race who are poor.

Like the poor, middle-class blacks who mistrust government and who feel internally efficacious are most active politically (Table 7). Blacks in this category participate at a rate nearly half a standard deviation above the population mean (.46), higher than the most active white group (.32). Though the interactive effect is not statistically significant for a group of this size, the findings are intriguing.

The reason cannot lie in the Gamson-Gurin thesis, which does not apply to highly motivated individuals. Higher-status blacks and whites are considerably more motivated than the poor, although for different reasons (data not shown). Higher-status whites have a stronger sense of civic duty and can more easily identify the names of political incumbents than blacks of comparable status. Middle-class blacks have stronger partisan and ideological motivation. Both have comparable levels of psychological involvement.[9]

Two alternative explanations fit the data. The first explanation, though not derived from the Gurin-Gamson thesis, is compatible with it. The second offers an important rival interpretation. On the one hand, political cynicism may be a more important motivation for all black Americans regardless of status. Black activists of all levels of income and motivation may be particularly motivated by the democratic principle of representative and responsive government which they believe to be violated in their own case. The long historical struggle to gain equal access to governmental processes has certainly made the issue exceptionally salient to the black community. Though the weak sense of external efficacy, which is intrinsic to political cynicism, would normally discourage the otherwise highly motivated from participation in politics, it is more than compensated for by the extraordinary sense of internal efficacy displayed by black activists.

On the other hand, as is so often the case with data collected at one point in time, the fact that higher-status white activists are trusting, while black and lower-status white activists are cynical, could be an indicator of the relative success or failure of those groups in the political marketplace. Black and poor activists confront a political process that favors the well-to-do and the powerful and, even at its best, is designed to respond to political majorities, not minorities. Under these conditions, efforts to gain equal access or compete politically can lead to political cynicism. In short, the simple association between trust and participation can be interpreted to mean either that trust influences political activity, or that such activity influences trust.

The issue cannot be resolved here. The rigorous types of experimental or quasi-experimental data necessary to enable us to choose among the equally plausible, but rival interpretations of the association do not exist. What data are available (Zurcher, 1969, pp. 213-33; 1970, pp. 93-94, 99-102; Shingles, 1973, pp. 251-68; 1975, pp. 49-57), quasi-experiments involving the poor in Community Action Agencies, suggests that the causality goes both ways; that those most likely to volunteer to serve on CAA boards and neighborhood committees are self-confident and politically cynical, and that membership in community action agencies can contribute to greater cynicism, depending upon the participants' level and type of involvement and the level of their initial cynicism.

Black Voter Turnout. Black nonvoting is a distinct exception to black activism. Voter registration and turnout among blacks have traditionally lagged behind those of whites in all regions of the country. By 1976, federal efforts to end discrimination in the South had brought black turnout in that region up to the same level as blacks elsewhere—11 percent below that of whites (Cavanagh, 1979, pp. 11-14). Outside the South, black voting peaked temporarily in 1964, actually exceeding that of whites of similar socioeconomic status (Verba and Nie, 1972, p. 170). Since then it has declined by 20 percent, compared to a loss for whites of only 12 percent. The greater decline in black voting occurred among the lower classes. Turnout rates for professionals and managers of both races were nearly identical in 1976 (Erie and Brown, 1979, p. 34).

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[9] Operational definitions for each of the incentives were developed from the *Participation in America* dataset: *Civic Duty to Participate in Politics:* responses to openended questions v271 and v272 (Values 40 - 49 = 1, other values = 0); *Political Information:* v266 + v267 + v275 + v276 + v277; *Psychological Involvement:* v235 + v232 + v126; *Ideological Fervor:* absolute values of the following liberal-conservative index: v139 + v141 + v143 + v147 + v329 + v240 + v293 + v294 + v295; *Partisanship:* v414 recoded to measure degree rather than types of partisanship.

Four factors have traditionally discouraged black voting: (1) economic and physical intimidation (Verba and Nie, 1972, pp. 164-71; Salmon and Van Evera, 1973); (2) fraud and legal tactics such as gerrymandering districts and holding at-large elections (Walton, 1972, pp. 33-35); (3) the fact that many black citizens do not see the relationship between the social and economic problems they face and the political process (Morris, 1975, pp. 169-78); and (4) the fact that the most visible form of government for poor blacks is the welfare bureaucracy, with the result that the officials with whom they have the greatest contact are appointed rather than elected, thereby reducing the immediate relevance of the electoral process (Erie and Brown, 1979). These factors are more likely to discourage voting among lower-status blacks than among middle-class blacks. They also help explain why blacks have increasingly turned to other forms of policy participation such as working directly with local and federal administrators in addressing community problems.

The explanation presented here to explain the over-representation of poor blacks in high-initiative policy activities provides additional insight into their under-representation at the polls. The same attitude, political cynicism, which (when coupled with a sense of internal political efficacy) encourages policy behavior among the poor, discourages allegiant behavior. Black trust in government declined between 1964 and 1972 at a rate four times that of trust among whites (ISR, 1973). If Verba and Nie (1972, p. 52) and Milbrath and Goel (1977, p. 12) are correct in classifying voting as primarily an allegiant activity, the increase in black cynicism explains *both* the decline in black voting and the increased participation of blacks in

Table 8. A Comparison of Average Standardized Scores on the Index of Voting for Respondents of Varying Combinations of the Sense of Political Efficacy and Political Trust, Controlling for Income

	Low Income[a]		Moderate-to-High Income[b]	
I. Average Standardized Voting Scores:	–.20		.10	
	Political Trust		Political Trust	
II. Average Standardized Voting Scores by Trust and Internal Efficacy:	High	Low	High	Low
Internal Political Efficacy				
High	–.27	–.10	.18	.11
Low	.06	–.42	.06	–.07
III. Test of Statistical Significance (ANOVA):	*F* Value	Sig.	*F* Value	Sig.
Main Effects	2.750	NS	8.427	***
Internal Political Efficacy	.816	NS	10.185	***
Political Trust	5.293	*	5.256	*
Interactive Effects	20.441	***	.260	NS
Total Effects	8.647	***	5.705	***
IV. Multiple Correlation, $R_{y.x_1x_2(x_1x_2)}$:[c]	.24		.25	

Source: Compiled by the author from a 1967 survey of a representative sample of Americans 21 years or older conducted by the National Opinion Research Center.

[a] Total number responding = 1097. Broken down by cell: $a = 246; b = 183; c = 232; d = 796$.

[b] Total number responding = 2016. Broken down by cell: $a = 320; b = 348; c = 497; d = 796$.

[c] y = HICPB; x_1 = sense of internal political efficacy; x_2 = political trust; and (x_1x_2) = their joint effect.

*$p < .05$

***$p < .01$

NS = not significant

policy-related activities. The data reported in Table 8 support this interpretation. Relative to the sense of internal political efficacy, political trust is a better predictor of voting among the poor than among better-motivated individuals of higher socioeconomic status (see ANOVA). The poor most likely to vote are those who have a high sense of trust and little sense of internal efficacy. This is the combination specified in generalization 6 as providing the optimal incentive for low-initiative allegiant activity. Among the poor, blacks are least likely to meet this condition.

In light of this perspective, the high black voter turnout in 1964 must be seen as an anomaly. The most likely explanation consistent with our thesis is that it was due to the short-term forces—civil rights activity and the War on Poverty—that dramatically increased the policy relevance of voting for blacks and stimulated large numbers of low-income blacks to vote for the first time (Barker and McCorry, 1976, p. 119). These primarily black issues have had less saliency in recent elections with the consequence that voting as a policy activity has had less importance to poor blacks. Thus by the mid-1970s, voting had little appeal to the black poor as either an allegiant or a policy activity.

Conclusion

The increases during the 1960s in racial consciousness, political participation, political cynicism, and self-esteem of black Americans are related. Black consciousness contributed to active efforts to influence public policy by freeing a new generation of black Americans from the mental shackles of the Protestant work ethic which imputes guilt to economically unsuccessful individuals. We use the expression "blaming the victim" to describe the practice of loading guilt on those who do not succeed in our society, a society which, throughout most of its history, has alternated between oppressing and ignoring its largest racial minority. The realization that the reason for black deprivation does not lie squarely on them has allowed many poor blacks to transfer the responsibility for poverty from themselves to society and to the most visible symbol of society, government. The result is a mentally healthier and politically more active black citizenry. The strong sense of self-confidence and a deep suspicion regarding the willingness and ability of government to respond to their needs has proved to be an important catalyst, mobilizing blacks to seek to influence the policy process.

The federal government has had an important, if unintended, role in shaping the nature of black political participation. It is second only to the civil rights movement in spurring the development of black consciousness. By declaring war on both poverty and racial discrimination in the 1960s, the federal government legitimized and politicized black goals of social and economic equality. Needs changed to political demands as the idea set in that society has a responsibility for black deprivation and as the government assumed increasingly greater responsibility for black welfare. Both at the symbolic level of political rhetoric and the operational level of the local welfare office, it has been acknowledged that government deserves the credit or, more likely, the blame for the fortunes of black people.

It is paradoxical that both the government's apparent retreat from its initial commitment to black goals and the continued growth of a welfare bureaucracy have contributed to a deepening mistrust of government among blacks, the former because of the image of betrayal it created, the latter because it symbolizes the moral responsibility of government for the deprivation of the black poor. Equally paradoxical, the decline in trust among blacks has contributed to a further decline in black voting at the same time that it has encouraged high-initiative policy behavior.

An understanding of differing bases of blacks' and whites' participation helps to clarify their political relation. Blacks make a greater effort to influence public policy than poor whites who, having no equivalent ideology, are more inclined to accept the implications of the Protestant work ethic for their sense of self-worth and the utility of political action (Verba and Schlozman, 1977; Lamb, 1975, pp. 61-78). Blacks therefore more often speak for all of the poor even though the overwhelming majority of those living in poverty are white.

Among poor activists, blacks focus more than whites on the policy process and less on purely allegiant activities. In their efforts to influence policy, they are likely to convey a distrust for government officials with whom they interact. Therein lies the potential for conflict. However, the conflict does not necessitate nor does it usually involve confrontation politics. The black violence and the nonviolent direct action of the 1960s have received too much attention. In its fascination with the dramatic and the unusual, the scholarly community (like the press) has overlooked the fact that black Americans are heavily involved in conventional politics. The tactics they employ are, for the most part, no different from those used historically by other ethnic groups who worked through the political process and in their communities in seeking an equitable place in American society.

Although this discussion has been necessarily confined to black politics, it has broad relevance for other minorities. The ability of black consciousness to mobilize black Americans supports Nelson's position that ethnicity rivals socioeconomic status as the principal analytical tool for understanding political participation (1979). Comparable ideologies like "brown consciousness" and "red consciousness" may have a similar importance for Hispanic and Native Americans (Welch et al., 1975; Greenberg, 1974, pp. 131-72). The key to policy-relevant behavior, however, will not be ethnic identity per se, but the existence of a counterculture which politicizes the community by having its members transfer responsibility for their deprivation from themselves to the political system.

Finally, the discussion has relevance beyond specific groups. It contributes to our understanding of the complex relationship among sense of political efficacy, political trust and political participation. There are multiple relationships among them, depending upon which components of the variables are involved. The specification of these components, the relationships among them and the conditions under which these relationships hold should constitute a constructive step in the development of a more general theory of political participation.

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The Sources of Political Tolerance: A Multivariate Analysis

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Over the past 25 years a number of conclusions concerning the development of political tolerance have come to be well accepted in the literature on political behavior. There are, however, two persisting problems with the studies that have generated these findings: they have relied on a content-biased measure of tolerance, and have failed to examine well specified models of the factors leading to tolerance. In this article we report the results of an analysis of the determinants of political tolerance using a content-controlled measure of tolerance and a more fully specified multivariate model. The parameters of the model are estimated from a national sample of the U.S. The results indicate the explanatory power of two political variables, the level of perceived threat and the commitment to general norms, and psychological sources of political tolerance. Social and demographic factors are found to have no direct effect and little indirect influence on the development of political tolerance.

The study of political tolerance has been a major component of public opinion research in the 25 years since Stouffer's seminal contribution (1955). Over this time, a number of findings on the determinants of tolerance have become accepted. Yet there are two reasons to suspect the general conclusions of these studies. First, as we have attempted to show earlier (Sullivan, Piereson and Marcus, 1979), previous studies of political tolerance were flawed in their measurement of the dependent variable. The use of items developed by Stouffer, all with left-wing targets (communists, socialists, and atheists), creates problems that will affect the distribution of political tolerance and will bias estimates of relationships between tolerance and other variables. The content bias resulting from using only left-wing targets means that respondents who are intolerant of *other* groups may erroneously be classified as generally tolerant. Such measures confound the willingness to extend rights to objectionable groups (tolerance) with indifference or perhaps even support of the groups specified. Correlations involving these measures of tolerance are thus difficult to interpret since they invariably tap attitudes toward the group in question as well as the construct of tolerance. We have shown elsewhere the distortion that results when the selection of a target group is related to socioeconomic characteristics such as education and urban/rural residence (Sullivan, Marcus, Piereson and Feldman, 1979; and Marcus, Piereson, and Sullivan, 1980).

The second problem with previous studies of political tolerance has been their reliance on simple correlations to establish the relationships between tolerance and a number of independent variables. Bivariate correlations, however, can be misleading when other important factors influence both variables in question. This sort of specification error can affect the magnitude and even the direction of parameter estimates. Yet, so far as we have been able to find, no study presents or evaluates a well-specified model of political tolerance. As a result, many currently accepted findings on the determinants of political tolerance may be based on severely biased estimates of the relationships between key independent variables and tolerance.

We report here an analysis of the determinants of political tolerance that attempts to correct these problems. We use a content-controlled measure of tolerance to remedy the content-bias of previous measures. This measure is used in a multivariate analysis to evaluate a more fully specified model of political tolerance. In addition, we rely

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We wish to acknowledge the support of the National Science Foundation, grant SOC 77-17623, in conducting this study. Professor Michal Shamir provided invaluable assistance.

on maximum likelihood confirmatory factor analysis (LISREL) to fit the model to our data. The advantage of this approach to model fitting is that it incorporates a measurement model into the data analysis. We are thus able to examine the reliability and appropriateness of our indicators as one part of the overall evaluation of the model. This will further raise confidence in our results since parameter estimates will be free of the effects of measurement error and the major constructs will be more clearly defined.

Political Tolerance and Democratic Theory

Much of the theoretical significance of political tolerance derives from its role in varieties of democratic theory. Its importance depends on two sets of empirical questions. The first of these questions involves the distribution of levels of tolerance in society and the relationship of tolerance to political activism. Given the hierarchy of political roles available to citizens today, from those uninvolved in politics to those in formal positions of authority, the citizen's opportunity (and thus responsibility) to sustain the "rules of the game," including ensuring political access to potentially objectionable groups, may vary across these roles in a systematic way. Empirical findings suggesting that those with greatest responsibility have greater commitment to the norms and maintenance of political tolerance would support the continuing hierarchy of influence among political roles as necessary to sustain the tolerant character of the regime. Alternatively, finding the aggregate level of political tolerance to be fairly uniform across different political roles would support a more equitable redistribution of political influence. Thus how tolerant various classes of people are in various political roles has been one of the abiding areas of dispute in the contention between "elitist or revisionist" theories of democracy and "participatory" theories of democracy (see Walker, 1966; Pateman, 1970; Thompson, 1970).

A second important way in which the empirical study of political tolerance can affect views on democratic theory lies in the question of the relationship of political tolerance to other factors. As societies change, for example by becoming more educated, more industrialized and more urban, these changes may alter the extent and nature of tolerance in the political culture. We need, therefore, to explore the determinants of political tolerance and how changes in these determinants are likely to affect the level and distribution of political tolerance. For example, while finding low levels of political tolerance, Stouffer (1955) also found powerful links between tolerance and education which he expected would lead to increased tolerance in the future. The extent to which political tolerance is linked to determinants likely to go through progressive changes that will in turn ameliorate political intolerance is thus an important empirical question. The answer will add fuel to the controversies as to which type of democratic theory is best suited to our values and circumstances (see Pennock, 1979; Thompson, 1970). Some varieties of democratic theory, for example, have strong developmental expectations. According to such theorists as J. S. Mill, the political culture and the political judgment of the electorate can be expected to improve as formal education and practical political experience become more widespread throughout the public. A model of the development of political tolerance can help to assess the empirical status of such theories.

Development of the Model

We will consider variables taken from two sources. First, we will review the major independent variables that previous studies have shown to have significant relationships with political tolerance. Such variables fall into three broad categories: social (or demographic) characteristics such as education, occupation, age and religion; psychological (or personality) characteristics such as self-esteem, authoritarianism and dogmatism; and political characteristics such as political ideology, political involvement and commitment to democratic norms. Second, we will consider those factors that are theoretically important even if research has not yet demonstrated significant relationships.

The Dependent Variable: Political Tolerance

As we noted in our previous work (Sullivan, Piereson and Marcus, 1979) a measurement procedure is needed which allows respondents themselves to specify the groups they most strongly oppose. In an attempt to obtain such a measure, we developed and tested the following measurement approach on a national survey of 1509 respondents conducted for us by the National Opinion Research Center in the spring of 1978. First, we provided each interview respondent with a list of potentially unpopular groups that ranged from communists and socialists on the left, to fascists, John Birch Society members, and Ku Klux Klan members on the right. We also included a number of groups, such as atheists, pro-abortionists, and anti-abortionists, which we expected in some ways to represent positions that are independent of the left/right dimension. Respondents were then asked to identify the group they liked the least, and we made it very clear that they could also select a group not on the list. Respondents were then presented with a series of statements in an agree/disagree format which elicited their views about a range of activities in which members of that group might have participated. The following statements were among those included in the series:

1. Members of the ______ should be banned from being president of the United States.
2. Members of the ______ should be allowed to teach in the public schools.
3. The ______ should be outlawed.
4. Members of the ______ should be allowed to make a speech in this city.
5. The ______ should have their phones tapped by our government.
6. The ______ should be allowed to hold public rallies in our city.

The statements were read as they appear above with the blanks filled with the group selected by each respondent.

Our intention was to avoid contaminating the tolerance/intolerance dimension with the respondents' political beliefs. If we had merely asked all respondents whether communists should be allowed to hold public office, their responses would have depended not only on their levels of tolerance, but also on their feelings toward communists. The advantage of our procedure is that it creates a situation in which the evaluation of each respondent toward the group in question is held constant. Clearly, our measures are not "content-free" since there is a context and a specific group toward which each respondent must react. We thus call it a "content-controlled" measure, to emphasize that we have attempted to "control for" the content by allowing respondents to select functionally equivalent groups.

In the following analysis, we rely upon a six-item political tolerance scale, based on the six political tolerance items reviewed above. Since each item has five-point agree/disagree response categories, scores on the six-item scale range from 6 to 30. The mean score for the six-item scale is 16.1, well below the midpoint of 18; and the coefficient alpha, an estimate of the lower bound of reliability, is .78.

Social Determinants of Tolerance. Since Stouffer's in-depth empirical study of tolerance, a number of basic demographic variables have been thought to influence political tolerance. Foremost among these is education. Education is thought to increase familiarity with diverse ideas and people. Moreover, the citizen must *learn* that a free market of ideas is vital to American democracy and that nonconformists are not necessarily bad. This is basically a cognitive explanation; one learns the principle that free exchange of ideas is necessary and that to be different is not necessarily to be bad and dangerous. Thus education plays a dual role in increasing levels of information and increasing the willingness to accept hitherto threatening information.

While this argument makes no attempt to tie education into the more general aspects of the social structure, high social status is also likely to be linked to increased experience (hence acceptance of diversity of opinion) and increased security from threat (hence greater willingness to allow others to hold potentially threatening ideas and to display potentially threatening behavior). This reasoning would lead us to expect social status to be related to tolerance in somewhat the same way as education is (see Korman, 1975).

In our study, we examine the impact of both education and social status, the latter measured by income and occupational position. Social status is measured by the Hodge-Siegel Prestige Score, which is assigned to the reported occupation of the respondent (or in the case of the unemployed, of the respondent's spouse; for details see Siegel, 1971).

Previous researchers have found a relationship between age and political tolerance. Because younger respondents are likely to be more educated, and because the younger cohorts of respondents have experienced a political climate that should be more liberalizing and enlightening than that of their elders (a period effect), age should be inversely related to tolerance. The increasing commitment to civil rights for blacks, women and the poor, particularly on the part of the young in the 1960s, may have given the younger respondents a greater commitment to political tolerance, or so the argument runs. Thus, while the link between age and tolerance may be strongly affected by the influence of education (Stouffer, 1955; Nunn et al., 1978; Cutler and Kaufman, 1975), it appears from other studies that even after the influence of education is removed, age may still have a conservative effect on political tolerance.

Past studies (Stouffer, 1955; Nunn, et al., 1978) have also uncovered what appears to be a relationship between religion and tolerance of ideological nonconformity. Following Stouffer, Nunn and his associates found that in 1973, Jews and non-religious people were the most tolerant, followed by Catholics and Protestants, with little difference between the latter two religious groups. The percentage differences discovered were substantial, as approximately 46 percent of Protestants, 59 percent of Catholics, 88 percent of Jews, and 87 percent of non-religious people were categorized as "more tolerant" on Nunn's scale.

Other researchers have discovered the relationships between tolerance and additional social variables such as sex, size of city, and region. Our research, reported elsewhere (Sullivan et al., 1982) shows that all of these social background variables relate with target-group selection in such a way that the content-bias of the traditional measurement procedures may have produced misleading conclusions about the correlates and determinants of political tolerance. For example, the following respondents are most likely to select right-wing targets: those with more education, younger respondents, Jewish or non-religious respondents, urban residents, residents of the East, and so on. On the other hand, the following respondents are mostly likely to select left-wing targets, precisely those groups included in previous studies of tolerance: the uneducated, those of the older generation, Catholics or Protestants, rural residents, residents of the South and Midwest, and so on. Thus previous studies may have "stacked the deck" by asking about groups which are, in some sense, harder for certain respondents to tolerate.

Psychological Determinants of Tolerance. In addition to the studies which have focused on the demographic determinants of political tolerance, other studies have examined mainly the psychological and personality sources of political tolerance (Knutson, 1972; Zellman and Sears, 1971; Zalkind, Gaugler and Schwartz, 1975). The most consistent finding links self-esteem (or a closely related concept) to political tolerance. Most analysts have examined self-esteem within a trait conceptualization of personality, although Sniderman (1975) viewed low self-esteem as a hindrance to the social learning of political norms, including tolerance. Sniderman argued that low self-esteem leads to intolerance because it interferes with social learning. Although the norms of society may be tolerant ones, a significant portion of the public will neither learn nor adhere to these norms because this requires a facility for abstract thought that many do not possess. In particular, people with low self-esteem will "reject the norms of democratic politics not because they are motivated to do so but largely because their negative self-attitudes have impeded the learning of these values" (p. 178). While he concentrates on the social learning argument, Sniderman also argues that low self-esteem may interfere with the motivation to learn norms and ideals such as tolerance.

Sniderman used three different measures of self-esteem that correlate highly with one another and produce similar results. In the interests of parsimony, we selected his eight-item personality unworthiness scale as the measure of self-esteem we would use in our multivariate analysis. The scale has a reliability (coefficient alpha) of .69.

Probably the most dominant tradition in political psychology is research into the authoritarian personality (see Sanford, 1973). In essence, the authoritarian personality construct is based on a trait approach—an approach which assumes personality to be a collection of traits within the individual. Authoritarianism consists of a number of important characteristics including authoritarian submission, defined as a basic need to obey those in authority and to command subordinates; aggression toward outgroups, which means redirecting underlying hostility of authorities toward weaker scapegoats; stereotyped thinking—that is, intolerance of ambiguity and a strong tendency to think in terms of black and white categories—and other similar characteristics. Seen in these terms, the relationship of authoritarianism to political intolerance is probably close, although tolerance is an attitude rather than a personality trait and the two must be separated analytically.

A serious problem, from our point of view, are the criticisms that the traditional measure of authoritarianism, the F-scale, is appropriate to measure authoritarianism on the political right but not on the left. Theoretically, there is no reason why left-wing belief systems cannot be rigid or authoritarian. One major alternative to the F-scale is Rokeach's work on dogmatism (1960) which attempts to measure the degree of people's open-mindedness and flexibility without regard to ideological content. We include seven dogmatism items which create a scale with a coefficient alpha of .77.

Another strategy is to employ an indirect measure that taps a central aspect of the authoritarian syndrome. To accomplish this, we have included three items from Rosenberg's faith in people scale (1956) and two items from Martin's and Westie's threat orientation scale (1959). Since an important characteristic of the authoritarian personality is distrust of other people, their motives and their impulses, we deem the five items an appropriate, if simple, measure of the affective component of authoritarianism. These items create a scale with a coefficient alpha of .69.

Another major approach is that of Knutson (1972) who relies on the conceptualization of personality developed by Maslow. In this scheme, human personality is dependent on the satisfaction of various needs, ranging from the most basic (physiological and safety needs) to the more complex (affiliation, self-esteem, and self-actualization needs). These needs form a definite hierarchy with the more basic needs requiring satisfaction before the needs on the next level of the hierarchy can be met. Knutson has characterized these as lying along a continuum with "concern with self" at one end and "concern with self in relation to one's environment" at the other. The self-centered person is unlikely to experience empathy; since most of that person's energies are directed toward meeting personal, basic needs, abstract ideas such as tolerance are unlikely to receive much attention from such a person. Thus, at one end, individuals are expected to be self-centered and most of their behavior should be directed at immediate, concrete goal fulfillment. At the other end, individuals are expected to consider and examine broader perspectives and to be concerned with how they relate to others and to the social environment more generally. Using Knutson's formulation, we would anticipate a monotonic relationship with higher levels on the need hierarchy linked to greater tolerance.

In order to assess people's positions on Maslow's need hierarchy, we asked people in our national study to select their most important value from a list provided them. We selected one value that represented each level on the need hierarchy (see Inglehart, 1977; Marsh, 1977). The values are listed in Table 1 along with the need level they purport to measure and the percentage of our national sample selecting each as most important. The relationship is generally monotonic. There is a strong increase in political tolerance as one moves from the safety-security need level to the affiliation need level.

To anticipate our findings somewhat, we did not find that these four psychological variables were able to discriminate among the various approaches to personality and political tolerance. Our analysis showed all to be linked to political tolerance, and furthermore, that the most powerful analysis was to incorporate the four indicators as alternative measures (multiple indicators) of a single concept: personality. Since the four indicators are so highly intercorrelated and the measurement approach we use corrects correlations for attenuation due to unreliability, multicollinearity problems require us to use them as multiple indicators. Conceptually then, we combine the four personality variables in a single factor, perhaps best defined as "psychological security." The four indicators have much in common because each focuses, in importantly different ways, on the ability of the individual to confront reality and social experience, and to deal with a world that is complex, ambiguous and threatening. The variance shared by these indicators suggests that people vary in the extent to which they are psychologically prepared to engage a socially and politically complex environment.[1]

Political Determinants of Tolerance. The most serious omission in previous studies of political tolerance has been the failure to examine *political* tolerance as an inherently *political* concept with its source in *political* processes. We will attempt to correct this deficiency by examining the importance of three sets of political variables, including political ideology, political threat, and support for the general norms of democracy.

The differences between liberals and conservatives are sometimes thought to account for various degrees of political toleration. At times, the terms "tolerance" and "liberalism" are used in-

Table 1. Respondents' Choice of Most Important Need

Maslow Need Hierarchy	Value	Percent of Sample	Mean Tolerance Score*
1. Physiological Needs	Comfortable Life	24	14.9
2. Safety and Security Needs	Security	31	15.0
3. Affiliation and Love Needs	Affection	19	16.7
4. Esteem Needs	Esteem	20	17.7
5. Self-Actualization Needs	Originality	6	18.0
N = 1509			

Source: Computed from 1978 Political Tolerance Survey conducted by the National Opinion Research Center, University of Chicago.

*On the six-item scale which ranges from 6-30.

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[1] The excessive intercorrelations of these personality measures prevents any direct comparisons among the various psychological explanations. Such a result is not totally unexpected; unfortunately it may be predicted from several theoretical perspectives. Knutson (1972) demonstrates that the need hierarchy formulation can account for the development of a number of personality characteristics, including the ones used here. The data are also consistent with Rokeach's argument (1960) that low self-esteem will lead people to develop dogmatic belief systems and to exhibit little faith in other people. These results are consistent with the broad outlines of the theory of the authoritarian personality.

terchangeably so that the acceptance of norms of toleration is said to be a liberal position and the rejection of tolerance, or a more guarded acceptance, is said to be a conservative or "illiberal" position (see Lipset and Raab, 1970, pp. 432-33). Those who make this connection, however, are usually careful to distinguish between economic and non-economic issues, where the former refers to questions of the distribution of wealth and the latter to those of cultural conformity and nonconformity. In this sense, tolerance is understood to be part of the social or non-economic dimension of domestic liberalism. Tolerance is thus associated with issues such as the legalization of drugs, acceptance of cultural nonconformity and opposition to traditional women's roles.

From another perspective, social scientists such as McClosky (1960) and Eysenck (1954) have suggested that ideological identification is linked to personality characteristics that might lead us to anticipate a link with tolerance. Specifically, McClosky argues that support of certain conservative beliefs signals a personality unlikely to engage in political discourse, to adapt to social change, to extend universal norms to strangers, and to engage in stereotyping and discriminations. Thus this non-economic dimension of ideology may relate to tolerance in part because of certain underlying personality characteristics. While previous studies of political tolerance may have overestimated the impact of ideology due to an inherent left-wing bias, we advance the tentative hypothesis that those on the left will be more tolerant of extremist groups on the right than those on the right will be tolerant of corresponding groups on the left.

Two measures of ideology, in the left/right sense, have been adopted. The first is an index of ideological self-placement, a seven-point liberal/conservative scale on which respondents were asked to locate themselves. The seond measure is based on the ideology of respondents' two least-liked groups. As we demonstrated earlier (Sullivan, Piereson, and Marcus, 1979), respondents at the liberal end of the spectrum were most likely to select targets from the right, and vice versa. It is therefore possible to contrive a measure of ideology based on the target groups selected. Thus, those who chose two right-wing groups were classified as the most liberal, while those who chose two left-wing groups were classified, on a three-point scale, as the most conservative. Those who picked one group from each end of the spectrum were placed in the middle of the scale.

Previous studies have also shown that the level of intolerance in individuals is directly related to their perceptions of dissident groups as posing a threat (see Stouffer, 1955, Ch. 8). Thus intolerance arises from people's perceptions that dissident groups threaten values important to them or constitute a danger to the constitutional order. Yet it is also true that many people who perceive dissident groups as threatening are nevertheless prepared to tolerate them and to defend their procedural claims. It is appropriate, therefore, to test this general proposition within a multivariate framework with our data.

We attempted to measure the threat posed by each respondent's least-liked group by presenting a series of semantic differential adjectives about the group in question. We began with eight adjective pairs, which were factor-analyzed. The five adjective pairs with the highest loadings on the first factor extracted were selected for use as indicators of perceived threat: honest/dishonest; trustworthy/untrustworthy; safe/dangerous; nonviolent/violent; and good/bad.

While previous studies (Prothro and Grigg, 1960; McClosky, 1964) have concluded there is little relationship in the mass public between support for some general democratic principles and the specific applications of these principles, this conclusion—based on a comparison of marginal distributions—needs more examination. The observed consensus in favor of the abstract statements and the lack of any such consensus on the specific items were taken to mean that there was no relationship between the two. A more appropriate way to approach the problem is to measure the relationship between the degree to which individuals support the abstract principles and the degree to which they apply them in practice.

To examine this question, we repeated in our survey two of the questions used by Prothro and Grigg and four used by McClosky. These items, along with the distribution of responses in our survey, are summarized in Table 2. The first four items express democratic principles in highly abstract form, while the last two are more specific and hence more controversial. The distribution of responses to these items is very close to those reported in the earlier studies, despite Prothro's and Grigg's use of local samples, and the passage of nearly 20 years. It is still true that the overwhelming proportion of the American public supports the principles of minority rights, majority rule, equality under the law, and free speech when these principles are posed in an abstract form, as they are in the first four items of the table.

Although the marginal distributions of responses to questions ascertaining support for abstract principles and to questions ascertaining support for specific situations of political tolerance may vary substantially, it does *not* follow that there is no relationship between the two categories of questions. Among recent writers, only Lawrence (1976) has been sensitive to this problem as he tried to measure the relationship between respondents' positions on the general norms regarding tolerance and their willingness to apply them in practice. Using different items, Lawrence found considerable consistency between support for general norms and their application to specific circumstances, concluding: "Large majorities of the population in fact apply their tolerant general norms consistently on even the hardest . . . issues" (p. 93).

Although the proposition is often taken for granted that abstract beliefs influence responses to practical situations, it is not surprising to find that there is not a one-to-one relationship between the two. Competing values usually operate in practical circumstances, so that people are forced to find some compromise between or among them. Hence, we hypothesize that the stronger their commitment to the general norms of democracy, the more willing people should be to act on those norms. Table 2 presents the simple correlations between the various questions measuring support for the general norms and our six-item tolerance scale, which is based on responses to specific situations and political groups. These correlations, while not particularly high, are all in the expected direction. In interpreting these correlations, readers should bear in mind that there is little variation in the responses to these four items (note the distributions in the table), a circumstance that will usually reduce the size of the correlations.

In addition to the variables of left/right ideology, perceptions of threat and support for general norms concerning tolerance, additional political variables such as political interest, participation, and information were also examined. There is sufficient theoretical reason to expect a strong relationship between an individual's political involvement, for example, and political tolerance. Participatory democracy theorists (such as Pateman, 1970) have argued that the process of political participation is an educative one, and that tolerance is one of the natural outcomes of such a process. Empirical studies have found large differences in tolerance between the participatory elite and the non-participatory masses (cf. Stouffer, 1955, p. 57). We find that most of these differences are probably the result of the content-bias

Table 2. Support for General Principles of Civil Liberties (Percent)

General Statement[a]	Agree	Uncertain	Disagree	Agree with Prothro-Grigg or McClosky[b]	Correlation[c] with Six-Item Tolerance Scale (Specific Acts)
1. People in the minority should be free to try to win majority support for their opinions.	89	9	2	94-98	.25
2. Public officials should be chosen by majority vote.	95	3	2	94-98	.27
3. No matter what a person's political beliefs are, he is entitled to the same legal rights and protections as anyone else.	93	4	3	94	.22
4. I believe in free speech for all no matter what their views might be.	85	7	9	89	.29
5. When the country is in great danger we may have to force people to testify against themselves even if it violates their rights.	35	16	48	36	−.14
6. Any person who hides behind the laws when he is questioned about his activities doesn't deserve much consideration.	52	16	32	76	−.23
N = 1509					

Source: Computed from 1978 Political Tolerance Survey, conducted by National Opinion Research Center, University of Chicago.

[a] The first two questions listed are taken from the Prothro-Grigg (1960) questionnaire, while the last four are taken from McClosky's (1964). Our questions were presented in the form of five-point agree-disagree scales. The agree column reports the percentage that agree or strongly agree while the disagree column reports the percentage that disagree or strongly disagree. In the original studies, these questions were presented dichotomously so that respondents either had to agree or disagree.

[b] Reported in Prothro and Grigg (1960) and McClosky (1964).

[c] Correlations are Pearson's r.

of the Stouffer-based questions, because in our national sample we find that political participation relates strongly to target group selection, but not to tolerance (the higher participants dislike right-wing groups more). The parameter estimates in our model are unaffected by excluding these additional political variables.

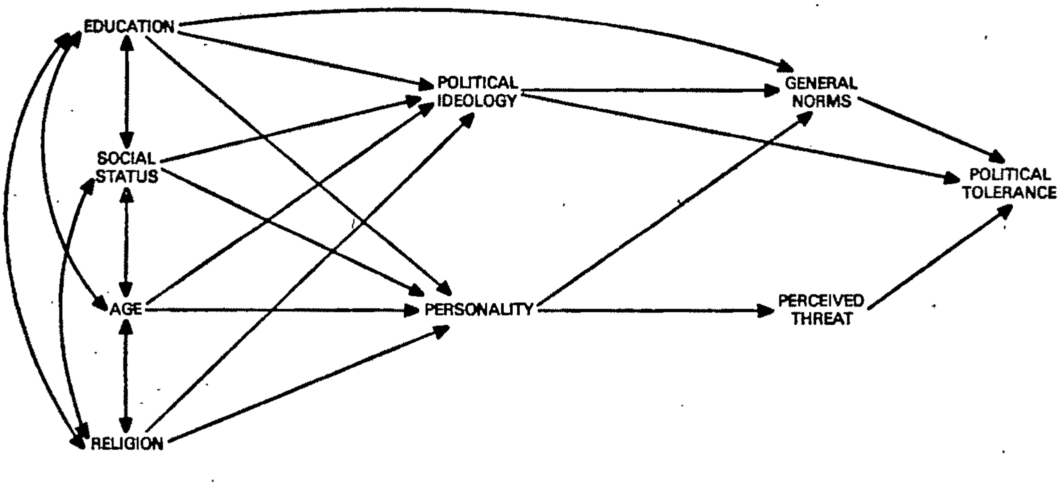
Multivariate Analysis of Political Tolerance

In this section, we specify a more complete model of political tolerance including variables from each of the three sets. In general, we assume that social variables are causally prior to psychological variables, which in turn precede political variables. Certainly these are dynamic relationships. As our analysis uses a cross-sectional survey, it must necessarily be a static one. We assume that this general causal ordering represents the major processes underlying the relationships among these variables.

Figure 1 presents the hypothesized relationships among endogenous and exogenous variables in our model of political tolerance. We assume the most important immediate causes of tolerance to be twofold: adherence to the general norms of civil liberties, and the perceived threat posed by the group toward which the tolerance (or intolerance) is directed. The reasons for our expectations are outlined above, and there is no compelling reason to expect that the impact of these two variables would be mediated by any of the other endogenous variables.

An individual's perception of another group as threatening is undoubtedly affected by psychological processes rather than resulting solely from realistic assessments of the danger of a particular disliked group. Some respondents may be psychologically predisposed to perceive threats whether they exist or not; and, thus, some people may be predisposed to be intolerant while others may not. The psychological characteristics discussed earlier, such as dogmatism and low self-esteem, might therefore explain perceptions of threat and, indirectly, tolerance and intolerance. If this argument were completely true, then levels of tolerance and intolerance in the society would be independent of actual political developments. We take the view here that perceptions of threat do involve evaluations of the political strength of, and the danger posed by, dissident groups, although since such perceptions are subjective, they will be affected by psychologial variables such as personality.

Turning to political ideology, we have already noted the liberal sources of tolerance. There is considerable reason to expect liberals to be more tolerant than conservatives. We fully expect some of this impact to operate through support for general norms of democracy, since the norms of democracy are themselves liberal norms. Thus liberals should be more likely to adopt these norms than are conservatives. There should also be some impact of ideology which operates directly on political tolerance, because while not all rank-and-file liberals and conservatives can be expected to comprehend and adopt the general norms most consis-



Source: Compiled by the authors.

Figure 1. Fully Specified Model of Political Tolerance

tent with their ideology, they might well be expected to adopt the more concrete implications of these norms.[1] However, there is no reason to expect that people's ideologies would relate directly to what they perceive as threats, since there are ample threatening targets in the eyes of both liberals and conservatives.

The role of personality as to its influence on political tolerance has been discussed above. It is unclear whether the impact of personality on tolerance should be expected to be direct, or to be merely indirect, through its link to perceptions of threat and support for toleration norms. Some of the impact of psychological factors is no doubt indirect, as we noted in its role in the development of the perceptions of threat from other groups and ideas. In adddition, those respondents who are more dogmatic, low on Maslow's need hierarchy, low in self-esteem and low in trust in people, can be expected to be more conservative than their opposite counterparts, and to be less likely to learn accurately the norms of the political system, leading us to expect a relationship between inner security and the support for general norms of democracy (Sniderman, 1975).

It is less clear what to expect regarding the direct impact of personality on political tolerance. Certainly, Sniderman's analysis (1975) suggests a direct impact, but it is plausible to explain this as working through the impact of personality on the learning of general norms. Perhaps those with low self-esteem are less tolerant merely because their lack of esteem interferes with learning general norms of democracy so that they cannot therefore apply them in specific situations. On the other hand, in addition to affecting the learning of general norms of democracy, low self-esteem may enhance the tendency to project personal inadequacies onto hated scapegoats, as suggested by the traditional literature on personality and politics. This could operate above and beyond this indirect effect, and in a sense, a comparison of the direct and indirect effects of personality on tolerance may provide a crude test of the social learning hypothesis as against the more traditional personality-trait hypothesis. The indirect effect of personality on toleration through support for general norms of democracy may represent the effect of personality through social learning, while the direct effect may represent its effect through traditional personality traits such as authoritarianism.

The four sociological variables in our model (all exogenous) include education, social status, age, and religion. Education is expected to have a major direct impact on three variables: political ideology, personality, and understanding general norms. Education has been shown to be related to tolerance in many studies, reviewed above, but these studies have not attempted to discern whether its impact is mediated by other variables. Furthermore, our evidence suggests that education relates more strongly with the selection of target groups than with tolerance per se (Sullivan et al., 1982).

There is good reason to expect education to have an impact on at least three endogenous variables other than political tolerance. It ought to have two opposite impacts on ideology. Previous studies have shown that education leads to greater economic conservatism, and greater social liberalism (Erikson and Luttbeg, 1973; Ladd, 1978). Thus the composition of our political ideology variable—social rather than economic—determines the direction of the relationship between education and ideology. Education also ought to affect personality—particularly those aspects of it that we have discussed here—self-actualization, dogmatism, and self-esteem. Persons with higher levels of education ought to be more competent in their environment, generally better able to manipulate and understand the forces which affect them, and thus better able to close the gap between the ideal self and the perceived self, i.e., to have higher self-esteem. They should also learn about diverse points of view and ways of experiencing life, thus becoming less dogmatic, even if they had a tendency toward closed-mindedness before achieving a high level of education. Finally, education ought to enable people to learn better the general norms of democracy. Since these norms are abstract, and require the ability to understand concepts, the highly educated should be more adept at learning and understanding them. Indeed, this was one of the major findings of both Prothro and Grigg (1960) and of McClosky (1964). The major unanswered question about the impact of education is similar to the question regarding the impact of personality: does it have any direct effect on political tolerance, above and beyond its impact on personality and support for general norms?

Social status should have an impact similar to that of education, while age should affect two of the political variables directly—older respondents should be more conservative (both on social and economic issues), and they should be more dogmatic and have lower self-esteem (see Erikson and Luttbeg, 1973). Religion is defined as secular detacchment. We have found that specific religious preference is related to target group selection, rather than political tolerance.[3] Secular detachment should have basically the same impact as age.

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[1] Although in the United States we fully expect even most conservatives to adopt the norm of tolerance, at least in the abstract, we do believe that conservatives will be more likely than liberals to trade off tolerance against other political values, such as stability and security. It is, of course, a matter of degree.

The model in Figure 1 presents the variables that we expect to have direct and significant impacts on one another. It is not a fully recursive model because several possible recursive relationships are not expected to be significant and have therefore been deleted. In estimating the parameters of the model, we estimate a fully recursive model, although generally we will report only those coefficients which are statistically significant. In this way, our a priori expectations can be compared with the modeling results; it will become clear which of the hypothesized direct paths, in Figure 1, are erroneous and should not have been specified, and it will likewise be clear which unspecified paths do in fact appear to exist between variables.

Parameter Estimation

We rely on maximum-likelihood confirmatory factor analysis (LISREL) to fit the model to our data. The problem of measurement error is addressed by allowing the researcher to specify first a set of theoretical relationships among unmeasured, theoretical constructs; and second, to specify a set of measurement relationships between these theoretical constructs and their empirical indicators. Using only information about the relationships among indicators, the researcher is able to obtain estimates of the relationships among the theoretical constructs (called *structural parameters*) and estimates of the relationships between each theoretical construct and its empirical indicators (called *epistemic correlations*).[4] The details of these statistical proccedures are presented in Jöreskog (1969, 1970, 1973); a brief summary may be found in the appendix. Most of the theoretical constructs are measured by several empirical indicators. The epistemic correlations help both to determing which indicators are the most reliable measures of each construct, and to assess the "true nature" of the construct.[5] Table 3 presents the epistemic correlations for those constructs with multiple indicators and for the six-item tolerance scale.[6] Single-indicator exogenous variables are not presented in this table.

The epistemic correlations are consistent with the definitions given to the constructs. Interestingly, the general norms construct seems primarily defined by the political process rules (majority vote and free speech) rather than by legal rules. The single value-actualization item relates only weakly to personality, and the common semantic differential format of the perceived threat indicators introduces some minor correlated error (−.13 between honest/dishonest and safe/dangerous).

Regarding the personality construct, the dogmatism scale is the "best" indicator. It practically defines the construct, although the self-esteem scale also has a large epistemic correlation. The other two variables—the value measure of self-actualization and the faith in people scale—are of lesser importance than dogmatism and self-esteem in defining the personality construct. Thus, although we label the construct psychological security, it is closely related to the traditional dogmatism-authoritarianism syndrome presented in the personality and politics literature, and in personality research generally. A lack of self-esteem is often hypothesized to underlie this syndrome, and the epistemic correlations suggest that this is the case here. The dogmatism scale also has the highest bivariate correlation with political tolerance.

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[3] We divided our sample into the same four religious groups: Protestants, Catholic, Jewish, and non-religious respondents. Using our content controlled measure we found almost no difference among Protestants, Catholics and Jews in their levels of political tolerance, although we did find some large differences in percentages selecting left-wing groups as least-liked targets. This again suggests the importance of the content bias of the original Stouffer questions, as Jews are more tolerant of left-wing groups but not of right-wing groups. We find that people who adhere to no particular religious faith are more tolerant than the more religious respondents, and in fact the differences are large. The only difference in tolerance scores among the various Protestant denominations is that Baptists are less tolerant than the others. To maximize the relationship with tolerance, we coded Baptists 1, non-religious people 3, and all others 2. When this is done, the correlation with our tolerance index is .26. Thus, although religion appears to be an important variable in understanding political tolerance, it is primarily a secular detachment from religion that is important, not whether one is Jewish, Catholic, or Protestant.

[4] Since almost all of the variables in this analysis are attitude or personality scales which have arbitrary units of analysis—and since the major concern is a comparison of the relative impact of different variables—we have relied on standardized rather than unstandardized coefficients.

[5] Readers versed in factor analysis, can interpret the epistemic correlations as factor loadings and the abstract constructs as factors. The pattern of loadings helps to label and interpret the factors.

[6] The epistemic correlation between the tolerance scale and the underlying construct is obtained by taking the square root of the scale's estimated reliability. Coefficient alpha for the tolerance scale is .79, so the epistemic correlation is fixed at .89. The other epistemic correlations are estimated using the excess information, provided by the over-identified model, using the LISREL program. In the case of the personality construct it will not generally be true that the epistemic correlations equal the square root of the estimated reliabilities. This is the case because the epistemic correlations reflect both the reliability of the appropriate scale and its relationship to the more general personality construct.

The relationship between the theoretical constructs are called structural parameters (*partial* path coefficients, corrected for measurement error). The structural parameters represent the strength of the relationships between each of the theoretical constructs included in the model, controlling for the other constructs. Figure 2 presents the political tolerance model with the exogenous and endogenous variables included. As we previously noted, political involvement is not included inasmuch as no direct or indirect relationships with political tolerance were found.

The perception of others as posing a threat has the strongest direct impact on political tolerance; the coefficient of −.43 shows that the more respondents perceived a threat from the target group the less their political tolerance. The general norms construct also has a strong impact (.33), confirming the hypothesis that the general norms of democracy do force some constraint on to specific applications of tolerance. There is a slight tendency for conservatives to exhibit less tolerance, all other things being equal, but the coefficient is small (−.10) and is statistically insignificant. We assume that the only reason conservatives are less tolerant than liberals is that they are less supportive of the general norms of democracy, since ideology and support for general norms are strongly related (−.41).

Psychological security has the hypothesized effects on political ideology and general norms. Respondents who are less dogmatic and have greater self-esteem tend to be both more liberal and more likely to support the general norms of democracy above and beyond that expected by their liberalism. Personality's direct effect on the general norms construct is in fact quite powerful, suggesting that support for the norms of democracy is not purely cognitive, nor is it purely a function of political ideology—the affect component of personality has a powerful influence on this support. Unexpectedly, psychological security and perceptions of threat are unrelated to one another. Our expectation, that perceptions of threat would be greater when the level of inner security is low, is clearly proved wrong.

The first question mark—whether psychologi-

Table 3. Epistemic Correlations among Indicators in Political Tolerance Model

Theoretical Construct	Indicator	Epistemic Correlation
Social Status	Occupation	.53
	Income	.87
Psychological Security	Value-Actualization	.43
	Self-Esteem	.67
	Dogmatism	.81
	Trust in Others	.50
Conservatism	Self-Placement	.44
	Least-Liked Group	.58
General Norms	Minority Rights	.44
	Majority Vote	.53
	Equal Legal Rights	.32
	Free Speech	.58
	Fifth Amendment	.32
	Legal Protection	.24
Perceived Threat	Honest/Dishonest*	.67
	Trustworthy/Untrustworthy	.63
	Safe/Dangerous*	.89
	Non-Violent/Violent	.62
	Good/Bad	.66
Political Tolerance	Six-Item Tolerance Scale	.89

Source: Computed from 1978 Political Tolerance Survey, conducted by National Opinion Research Center, University of Chicago. Complete listing of the actual items, and the questionnaires, are available from the authors.

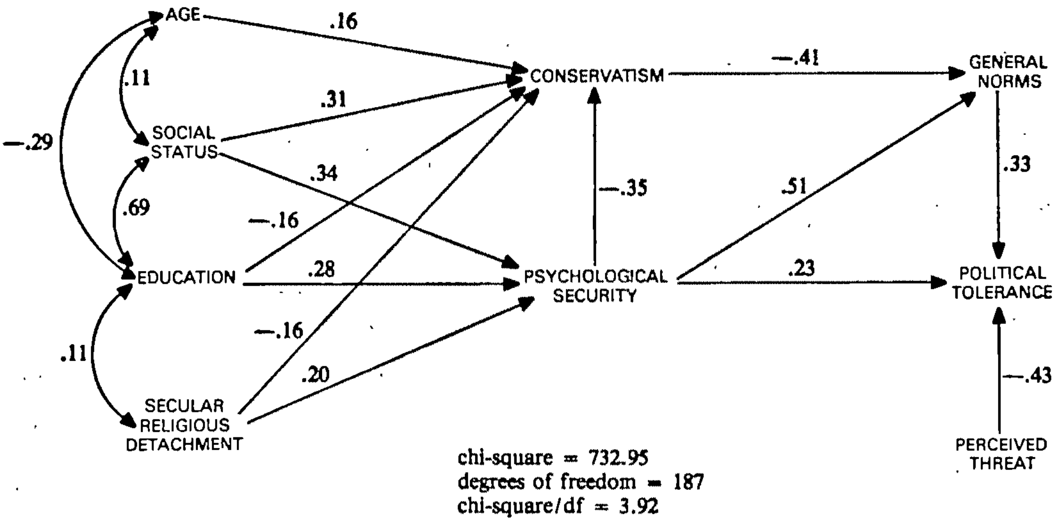
*Correlated error terms between these two items, r = −.13.

cal security has a direct influence on political tolerance—is answered in the affirmative (.23) in addition to its rather powerful indirect effect through the general norms construct. Persons with flexible, secure and trusting personalities are much more likely to be tolerant beyond that expected because of differences between them and people with opposite personalities as a consequence of the indirect effects through general norms of tolerance. The evidence is thus consistent with Sniderman's results (1975).

In Figure 2 four exogenous variables are included in the model—secular detachment, education, social status, and age. All but the third are single-indicator variables for which we assume perfect measurement. There is one significant correlation among error terms, here between income and age. Since age is assumed to be perfectly measured, the correlated error is between the error terms for the indicator (income) and the construct (age).

None of these exogenous variables has *any* direct effect on the constructs for general norms, political tolerance, or perceived threat. However, psychological security is strongly influenced by education (.28) and social status (.34); and political ideology is affected by social status (.31) while the impact of education is minimal (-.16). These results are discussed elsewhere (Sullivan et al., 1982), especially the finding that an individual's social status is positively related to the degree of conservatism, but is negatively and only indirectly related to psychological factors. Space considerations preclude the extended discussion necessary to unravel these relationships.

The model fits the data quite well, with a chi-square to degrees of freedom ratio of 3.92, generally considered acceptable.[7] The R^2 for the endogenous variables are: psychological security, .42; conservatism, .22; general norms, .60; and for political tolerance, .57. So the model does go a long way toward explaining support for the general


AGE
SOCIAL STATUS
EDUCATION
SECULAR RELIGIOUS DETACHMENT
CONSERVATISM
PSYCHOLOGICAL SECURITY
GENERAL NORMS
POLITICAL TOLERANCE
PERCEIVED THREAT
.16
.11
—.29
.31
.34
.69
—.16
.28
—.16
.11
.20
—.35
—.41
.51
.23
.33
—.43
chi-square = 732.95
degrees of freedom = 187
chi-square/df = 3.92

Source: Computed from 1978 Political Tolerance Survey, National Opinion Research Center, University of Chicago.

Note: Disturbance terms have been omitted for sake of clarity.

$x^2 = 732.95$
degrees of freedom = 187
$x^2/df = 3.92$

Figure 2. Model of the Sources of Political Tolerance

norms of democracy and specific applications of these norms to least-liked political groups.[8]

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[7] While the chi-square statistic provided by the LISREL program does yield a probability level for evaluating the goodness-of-fit of the model, this is not very useful in practice since the chi-square is a direct function of the sample size. Thus, for the number of cases we have here, any non-trivial model will fail to pass this test of statistical significance. A more useful test of fit in this case is to compute the ratio of chi-square to degrees of freedom. As a rule of thumb, ratios of less than five (for this number of cases) are indicative of a well-specified model (see Wheaton et al., 1977).

Conclusions

We have examined the impact of social, psychological and political variables on levels of political tolerance toward people's least-liked political groups, as reported by a national sample of respondents. From the analysis reported herein, we conclude the following:

1. Social variables generally have very little impact on individuals' support for the general norms of democracy or for political tolerance. Such variables have no direct impact, and have only minor indirect effects, mediated through such variables as political ideology and personality. Variables such as sex, region, race, and size of city, which previous research has suggested are strongly related to political tolerance, do not correlate with political tolerance once the content-controlled measurement strategy is adopted. Secular detachment, education, and age have a very small indirect impact on political tolerance, while, surprisingly, social status has a somewhat larger indirect impact, as it is one of the major factors influencing personality and political ideology.
2. Personality variables have both a powerful indirect impact on tolerance, through support for general norms of democracy and a fairly strong direct impact on political tolerance. Our results are quite consistent with those reported by Zellman and Sears (1971) and Sniderman (1975). We were unable to separate various aspects of personality, to examine the relative impact of self-esteem and self-actualization, for example, in order to test the relative merit of the Sniderman (1975) and Knutson (1972) theses. The presence of both indirect and direct effects *suggests* that the impact of personality is exhibited both by improving the learning of society's political norms, as suggested by Sniderman, and directly through the affective aspects of personality, as suggested by Knutson. We cannot press this line of inquiry too far, but it certainly suggests that both social learning and more traditional psychoanalytic and humanistic formulations should be taken seriously in any examination of political tolerance.
3. Two political variables—general norms and perceived threat—have a strong direct impact on political tolerance, though other political variables such as political interest, information and participation had no discernible impact. Our work is thus consistent with Lawrence's finding (1976) but somewhat inconsistent with the conclusions of Prothro and Grigg (1960) and McClosky (1964). Surprisingly, perceived threat is an exogenous variable in the model and thus is not merely or even partially a projection of psychological insecurity onto disliked political groups. The common view that inner needs dictate perception of threat is clearly not appropriate here.

On the other hand, general norms are strongly influenced both by political ideology and personality. Support for the general norms of democracy are thus partially the result of a political-ideological calculation of the importance of tolerance compared with other political values, but also partially the result of the degree to which individuals are open, flexible, and secure personally. Surprisingly, education does not have a direct effect on general norms, and even its indirect effects through ideology and personality are weak. It is plausible to suggest, therefore, that support for general norms is more the result of personality predispositions than of cognitive processes such as might result from civic education.

We by no means regard this analysis as definitive and the debate as closed. We do, however, suggest that we have taken the analysis a step further by adopting a content-controlled measurement strategy and by working with a more fully specified multivariate model, particularly one which takes an explicit measurement model into account in conducting the analysis.

The varieties of democratic theory are many and it would be inappropriate to say that these findings uniformly and consistently support any of these theories. However, some brief suggestive points may be made. The failure of political involvement—broadly investigated to include information, interest and participation—to have any direct or indirect effect on political tolerance, and the weakness of the ability of education to strengthen political tolerance suggests that participatory and populist versions of democratic theory have placed too much emphasis upon the presumed salutory impact of these variables, at least with regard to the American setting. On the other hand, the failure of measures of social status to play a powerful role in differentiating the politically tolerant from the intolerant suggests that conservative variants of democratic theory have placed too great a weight on the ability of the "better classes" to sustain the political norms of tolerance.

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[8] In our national sample, we included a subset of four items from Stouffer's 15-item tolerance index. When a scale of these four items is used in the model, several important differences occur. Briefly, the Stouffer index produces a significant negative path between conservatism and tolerance, confirming our suspicion of bias in the Stouffer items. We elsewhere describe this and other differences more fully (Sullivan et al., 1981).

Appendix

Since we had several measures of many of the important constructs in our model, we wanted to take advantage of this excess information to estimate the parameters of the model free from the effects of measurement error. Multiple-indicator models are appropriate in such a situation but the complexity of the model means that more sophisticated methods are needed (Sullivan and Feldman, 1979). To accomplish this, LISREL requires the specification of three sets of equations. The first is the set of equations giving the (hypothesized) relationships among the *true* or latent variables. To define those latent variables, two other sets of equations are necessary. One set specifies the *indicators* of the endogenous (dependent) variables as functions of these variables' true scores plus a random error component for each indicator, while the other set of equations does the same for the indicators of the exogenous (independent) variables. Thus, on the basis of these three sets of equations, all the relationships in the main and auxiliary (measurement) theories may be clearly specified as one of three types: free parameters (to be estimated), fixed parameters, and constrained parameters (unknown but set equal to some other parameter to be estimated).

In this form it is not possible to estimate directly the free or constrained parameters since there are too many unknowns in the equations. It is possible, however, to derive the variance-covariance matrix for the indicators on the basis of the hypothesized factor structure. Details may be found in Jöreskog (1973). This derived variance-covariance matrix should then equal the observed variance-covariance matrix of the indicators assuming correct specification of the model. The problem then becomes one of estimating the values of the free and constrained parameters so as to maximize the fit between the derived and observed variance-covariance matrices. If the model is identified, maximum-likelihood estimation procedures are used because for large samples these estimates are consistent (unbiased), efficient (having as small a variance as any other estimator), and approximately normally distributed. The estimators are also scale-in-variant, meaning that if the units of measurement have no real significance, as is the case here, the correlation matrix may be analyzed instead of the variance-covariance matrix. And finally, maximum-likelihood estimation generates a convenient statistical test for evaluating the adequacy of the model, the likelihood-ratio test which, for large samples (the case here) is distributed as chi-square and allows a test of the null hypothesis that the model specified by the fixed and constrained parameters is a perfect fit to the observed data. This simultaneously tests the fit of the structural relationships *and* the measurement component of the model. A large chi-square indicates that the hypothesized model does *not* fit.

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Issue Evolution, Population Replacement, and Normal Partisan Change

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Students of American political behavior have usually turned to "critical election" realignment theories to explain the dynamics of long-term change in the party system. These theories are problematic on both theoretical and empirical grounds. A theory that can account for change within the context of a stable electoral system is required. We propose a theory of issue evolution that explains normal partisan change, and illustrate the theory by examining the impact of racial issues on party identification. The theory looks to the evolution of new issues as the stimulus of partisan change and to the continuous replacement of the electorate as the mechanism through which such change is effected. In a comparison of three theoretical models of issue evolution, we conclude that racial desegregation has provoked a subtle but permanent change in the recruitment of new members of political parties.

Students of American political behavior have usually turned to "critical election" realignment theories to explain the dynamics of long-term change in the party system. Realignments, according to most theorists (Burnham, 1970; Key, 1955; Sundquist, 1973), are precipitated by the emergence of new issues about which the electorate has intense feelings that cut across rather than reinforce the existing bases of support for the political parties. It is often assumed that these cross-cutting issues, fueled by major, often highly disruptive social and economic conditions, lead to a fundamental alteration in the party system in that the majority party can no longer command decisive electoral support. One party's previously dominant position is ultimately usurped by a minority or third party, which becomes the chief beneficiary of the unfolding realignment process. According to these theories, partisan realignments involve not only massive changes in voting behavior, but more importantly, abrupt and permanent transformations in the party system as voters are moved to discard old party attachments in favor of new ones.

While realignment theories offer a plausible account of partisan change, we find them problematic both theoretically and empirically. The "critical" conception of realignment is oversimple and incomplete. Pushed to logical extremes, critical realignment models imply that all political eras are characterized by either radical change or none at all. Periods of rapid political destabilization are followed by generations when nothing happens.

Political theorists have not of course explicitly argued that "nothing happens" between realignments. Indeed, the cyclical realignment notions of Burnham (1970) and Beck (1974) explicitly postulate precursors to realignment. But given our understanding of the processes of attitude change, the discontinuity of critical realignment theories is not parsimonious. The linchpin of dichotomous notions of change is of course the event of spectacular magnitude, which temporarily moves the system from one state ("nothing happens") to another ("change"). What is problematic here is that some of the causal events are not very spectacular—what happened in 1896?—and quite a number of truly spectacular events (world wars, political assassinations, race riots in the cities) are followed by undisturbed party alignments.

Realignment theories are also deficient on empirical grounds. Most simply, there is no hard evidence supporting the postulated large-scale partisan conversions, since all realignments occurred before we began seriously to study individual party identification. Moreover, Kristi Andersen's analysis of the New Deal realignment period—the one case from which we can even extrapolate attitudinal data—sharply undercuts the notion of

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Some of the data used in this article were originally collected by the Center for Political Studies of the University of Michigan under a grant from the National Science Foundation. The data are available through the Inter-University Consortium for Political and Social Research. We are indebted to John McIver and Ann Shaw for research assistance. James Kuklinski, Leroy Rieselbach, Paul Beck, Glenn Parker, and several anonymous readers for the *Review* made valued comments on many earlier versions of this article. This research was supported by the National Science Foundatin under grant SOC-7907543. Manuscript preparation was aided by the Workshop in Political Theory and Policy Analysis.

massive partisan conversion (Andersen, 1976, 1979). The Republican party may have been the majority party before the realignment and the Democrats the majority party afterwards, but it is not the case that large numbers of Republicans became Democrats. Instead, Andersen argues that the partisan shift resulted from the mobilization of previously inactive partisans.

Realignment theories have produced a serious empirical anomaly as well. If one assumes that no realignment has occurred since the formation of the New Deal party system, one is hard pressed to account for changes of massive proportions which have occurred. The Republican party, nearly equal in numbers with the Democrats after the New Deal realignment, *is now less than half of what it was.* And the numerically negligible category of citizens declaring independence of partisan affiliation is now several times what it once was. These changes are of a magnitude that quite probably dwarfs the (unobserved) changes that occurred *during* the realignment. Explaining significant change over an extended period requires a theory of *normal* partisan change. Otherwise we are left with the palpably wrong conclusion that "no change" occurred or the prospect of searching for an unnoticed realignment.

A Theory of Normal Partisan Change

Clearly, an alternative theory of partisan change is required, a theory that can account for change within the context of a stable electoral system (Shively, 1979). We propose here a theory of issue evolution that explains normal change in the party system, and illustrate the theory by examining the impact of a particular evolving issue.

Drawing on V. O. Key's often-neglected article on secular realignment, we argue that issues can bring about significant political change by producing slow, steady shifts in the partisan complexion of the electorate. Through gradual replacement of the electorate, political issues have the capacity to stimulate processes that, in Key's apt description, "operate inexorably, and almost imperceptibly, election after election, to form new-party alignments and to build new party groups" (1959, pp. 198-99).

The theory of issue evolution is a theory of normal partisan change, emphasizing that the composition of the electorate is continuously undergoing alterations that, over the course of several decades, may have profound implications for the party system. As Key wrote (1959, p. 198):

> Events and communications of political import play upon the electorate continuously; election returns merely record periodic readings of the relative magnitudes of streams of attitudes that are undergoing steady expansion or contraction. Some elections may be "critical" in that they involve far wider movements and more durable shifts than do other elections. Yet the rise and fall of parties may to some degree be the consequence of trends that perhaps persist over decades, and elections may mark only steps in a more or less continuous creation of new loyalties and decay of old.

The theory looks to the evolution of new issues as the stimulus of partisan change and to the continuous replacement of the electorate as the mechanism of change. Every passing day marks the exit of old voters from the eligible electorate and the entry of new ones. And the "streams of attitudes" characteristic of those going out are not likely to be matched by the attitudes of those coming in for the simple reason that young voters can respond to new issues relatively unconstrained by the powerful anchor of established partisan predispositions.

Normal partisan change, according to this view, results from the evolution of new issues coupled with the process of generational replacement. Both of these factors account for the gradual character of the partisan transformation. New issues by themselves are probably not sufficient. While they may lead to temporary defections in voting behavior, new issues alone are unlikely to change fundamentally the long-standing party identifications of older voters (Converse, 1969). Nor does significant secular change develop merely as a response to population replacement. The socialization of new voters into the existing partisan alignment is likely to be substantially continuous so long as the issues underlying the existing political cleavage remain predominant (Irvine and Gold, 1980). New issues thus create the opportunity for political change; new citizens are its most likely agents.

Although built upon the concept of "secular realignment," the issue evolution theory thus differs from Key's notion in postulating the "issue attitude" as the cause of change and replacement the process through which it is effected.[1] These postulates are consistent with the spirit of Key's theory, but exploit well-developed theories of party identification and longitudinal measures of attitude, neither of which was available when Key wrote. The theory is in line with Andersen's focus on new people as the mechanism of partisan realignment (1976, 1979), but differs in emphasis on a continuous rather than episodic process. The abnormal changes associated with the New Deal realignment (i.e., the rapid mobilization of the previously inactive) are, like Key's "wider movements and more durable shifts," a special case of the normal pace of replacement.

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[1] It should be noted that secular partisan change among the electorate probably occurs in several different ways, only one of which is highlighted in this article. Such change, may for example, result from demographic trends in which one social group with distinctive partisan loyalties grows more rapidly than other groups in the population.

The evolution of party differences on a political issue has little of the drama and none of the upheaval associated with critical election realignments. Nor do issues that have interesting evolutionary histories necessarily have powerful short-term effects on voting behavior. If issues develop and evolve slowly, the snapshot picture given of them in cross-sectional designs is bound to underestimate their effect, a problem that concerned the authors of *The American Voter* (Campbell et al., 1960) early in history of mass behavior research. Only a longitudinal perspective can capture the long-term consequences of evolving issues.

Properties of Evolving Issues

All political issues can be described by a general evolutionary model; they emerge, develop, and are (sometimes) resolved within the political system. Yet many policy conflicts are so non-salient that they never capture the attention of mass electorates. Even salient issues, moreover, are sometimes too short-lived to exert much influence on underlying partisan loyalties. They affect immediate voting decisions, but leave the link between citizen and party unchanged. Some issues reinforce the existing party alignment, and are a source of stability rather than change in the party system.

Issues that bring about visible rearrangements in the partisan composition of the electorate would seem to share certain qualities. Three such properties are these:

1. *Issue preferences must be deeply felt.* Preference for one policy option over another is not sufficient to recompose the electorate. Intense feeling is required. That intensity requires appeal to potent symbols to activate less-than-political masses. The issue must have penetrated public consciousness to an unusual degree or it will have too little impact to be noticeable—even in the long term.
2. *Parties and candidates must take up visibly different positions on the issue.* No matter how salient a particular policy conflict happens to be, it is unlikely to foster a partisan evolution if opposing parties and candidates cannot be differentiated on the basis of the issue. This differentiation may come about because one of the parties chooses to ignore the issue while the other party takes a strong stand, or the parties may adopt positions that differ in some degree. The important point is that partisan evolution is likely to go hand-in-hand with issue differentiation, for the latter gives citizens the opportunity to make party choices on the basis of policy considerations. Under these circumstances, parties are likely to recruit new identifiers with distinctive issue positions. The process is reciprocal and interactive; issue distinctiveness allows parties selectively to recruit new identifiers which, in turn, leads to further party differentiation on the issue. Converse (1975, p. 111) has argued that "the degree of objective party differentiation on major issues" is "very critical in the shaping of public opinion"; it is equally critical for the partisan evolution of political issues.
3. *The issue must be long on the political agenda.* Rapid partisan change is unlikely under any but the most catastrophic social and economic conditions, because a number of inertial processes work against it. One restraining condition is that the limited turnover of party elites, and therefore of party elite attitudes, restricts the ability of parties to move quickly to new issue positions. Because turnover is slow, parties are heterogeneous and party issue cues mixed. Where the stimuli are mixed and slow to evolve, mass response should be more mixed and slower still.[3] Finally, because many established party identifiers will selectively perceive party issue cues, most partisan change will occur by the recruitment of new identifiers. Reorientation by generational replacement is necessarily a slow process.

Racial desegregation is one issue—quite probably the only issue in American politics today—that has all three attributes. It has nearly a full evolutionary cycle, developing from *partisan* obscurity during the New Deal itself, to dramatic significance during the 1960s, to diminished (but still strong) salience during the 1970s. It is also an issue about which preferences are deeply felt. Like few issues in American history, race touches a sensitive nerve. It is an issue, finally, on which parties and candidates have staked out relatively unambiguous positions—most notably in the 1964 presidential election. Parties and candidates are usually able to deflect attention from highly divisive issues (Page, 1978), but this has not been the case with respect to race.

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[3] The valence issue is an interesting case of speedy party positioning, but it lacks evolutionary potential because both parties rapidly move to the same position. "Law and order," for example, ceased to be of any political interest when every candidate became equally "for" it. This points out yet another requisite for the evolving issue; it must be a genuine policy conflict. Otherwise, rational party leaders (e.g., candidates) will converge to the same popular position.

The Partisan Evolution of Racial Issues

To trace racial attitudes and party identifications over time, we have used the 1972 SRC/CPS National Election Study because it includes the richest and most varied collection of racial attitude materials. We derive two racial scales from a factor analysis of these diverse items. The first taps attitudes toward desegregation, both in general and in the traditional battlegrounds of schools, jobs, housing, and so forth. The second racial factor, more diffuse and affective, taps attitudes oriented more toward racial protest (and protesters) than the substance of racial policy.[3]

We employ the racial scales, along with personal histories of party identification, to reconstruct the racial attitudes of the electorate for each presidential contest from 1952 to 1972. We recreate the electorate for each presidential contest back to 1952 by reclassifying 1972 respondents according to their reports of party identification change and by eliminating those not old enough to vote in each reconstructed year.[4] By tracing racial attitudes by changes in party identification, this reconstruction technique allows us to determine the racial component of long-term partisan change.[5]

The recreated electorates *cannot* be taken as representative of actual electorates for any given year, but only of those young enough to be sampled in 1972. They do not tell us accurately what past electorates were, but they do, within the limits of respondent recall, tell us where the 1972 electorate came from. Apparent evidence of secular change in racial liberalism particularly should be ignored, for it may well be an artifact of the biased age distributions of the simulated electorates.

Figures 1 and 2 trace the two dimensions of racial attitudes from 1952 through 1972 for self-identified Republicans and Democrats.[6] Figure 1 suggests that the dimension we have called "Black Protest Scale" separates Republicans from Democrats and has always done so. The slight widening of the gap between partisans appears to reflect the infusion of young voters into the Democratic ranks. However, because the partisan differences are nearly constant—the lines nearly parallel—Figure 1 suggests continuity along this dimension of racial attitude.

The trends of Figure 2 are not so easily dismissed. We have already noted the danger of inferring secular trends from these data, but the observed trends do establish a baseline against which changes among subgroups can be judged. That baseline highlights the one major instance of aggregate change in racial desegregation attitudes contrary to the trend, a move to racial conservatism by Republican identifiers in 1964.[7] That movement created a gap between the parties that has never been closed. After at least a decade of similarity to the Democrats, Republican identifiers moved to the racial right in 1964 and stayed more or less in that track while other elements moved uniformly to the left.

The divergence of the parties on the traditional segregation/integration dimension is not large. Neither party can accurately be called the party of segregation or integration. But a divergence did occur, and it went against a century of associating racism with the Democratic party.

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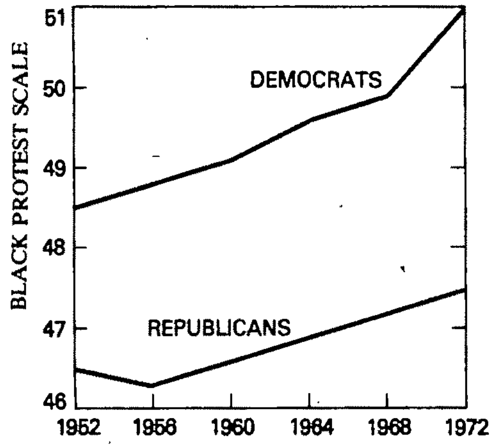
[3] See Carmines and Stimson (1980) for the factor derivation. It should also be noted that racial policy conservatism is not necessarily "racism." The primary referent of our measure is "government." Racism—not well measured in the survey tradition—is doubtless associated with racial policy conservatism, but the degree of association is an empirical question. The distinction is particularly important in assessing attitude trends. The new Republican racial conservatism may, for example, be a considerably different species from the old Dixiecrat variety.

[4] This is a variation of a technique employed by Kristi Andersen (1976, 1979). It differs in that it does not pool multiple surveys for the reconstruction and does not weight respondents by age.

[5] It should be noted that the reconstruction methodology is not without problems. Most importantly, Niemi, Katz and Newman (1980) have shown that there is substantial inaccuracy in the retrospective reports of party identification, particularly with regard to the full 7-point party identification scale. This inaccuracy, however, contributes only a minimum amount of unreliability to our analysis because: (a) we are not distinguishing between shades of party identification, and (b) the party changers are a very small group compared to the newly eligible partisans. Brody (1977) also finds that there is substantially greater stability in the "direction" of party identification than in its "strength."

[6] "Leaners" are treated as independents and, as a result, are excluded from the analysis. Thus the category self-identified Democrats includes both strong and weak Democrats; just as self-identified Republicans combines both strong and weak Republicans.

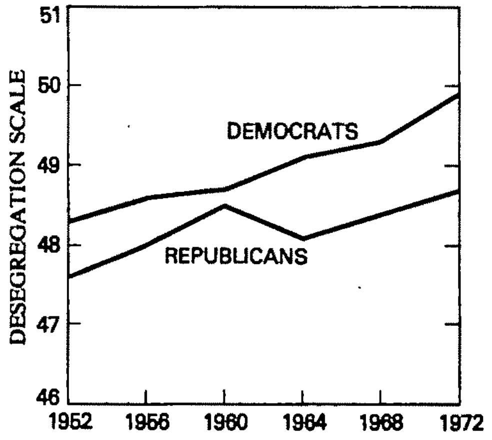
[7] We use the term "move" loosely. We cannot discriminate between individual attitude shifts and aggregate shifts caused by the gain and loss of identifiers. We assume the latter process is the major one.



Source: Computed from data collected in the 1972 Center for Political Studies Election Study, University of Michigan.

Figure 1. Attitudes toward Black Protest by Year

Because we can trace the entrance of new party identifiers over time, we can highlight the larger attitude differences among *new* partisans that are considerably diminished when aggregated with the old. In Figure 3 we plot the aggregated attitudes toward desegregation among new identifiers, by year and cumulatively since 1956. New party identifiers are in the main newly eligible voters, but the category also includes a very much smaller group, made up of those, formerly Independent, who have joined a party, or who have moved from one party to the other.



Source: Computed from data collected in the 1972 Center for Political Studies Election Study, University of Michigan.

Figure 2. Desegregation Attitudes by Year

After attracting new identifiers slightly more liberal than the Democrats in the period through 1960, the Republican party in 1964 not only moved to racial conservatism, but also did so quite dramatically. Those who report first identifying with the GOP in 1964 are strikingly more segregationist than any other group. About two-thirds of those new identifiers were newly eligible voters; their attitude distribution is, in any case, almost identical to the previously eligible voters reporting a new Republican identification. Movement away from the GOP contributed virtually nothing—contrary to much speculation at the time—to the new look of the Republican identifiers.

What is interesting about the 1968 new identifiers is not what did happen, but what did not. The appearance of George Wallace on the electoral scene in 1968 appears to have stopped for a time the racial polarization of party identification. Although the causes are no doubt multiple and difficult to sort out, it seems reasonable to assume that Wallace delayed the normal development of identification with party for his disproportionately young constituency. The Wallace candidacy removed a large number of racial conservatives form the ranks of new identifiers. The desegregation attitudes of those who did form identifications are slightly more liberal than would be expected and, more importantly, undifferentiated by party. Ideologues of the left may also have been slow to identify in 1968 because of the McCarthy/Kennedy insurgency in the Democratic party. But the number of new identifiers is too small to permit systematic analysis of these speculations.

Whatever was the case in 1968, the alignment of desegregation attitudes with party identification began again in 1972. Although the change was considerably less dramatic than in 1964, the Democratic party again went against its southern heritage to recruit distinctively more liberal identifiers and the Republican party moved slightly to the right.[8] That turn toward the right is less slight in contrast to the attitudes of other new identifiers; a small move to the right is stark against a trend in the other direction.

The net effect of adding new identifiers is shown in the cumulative portion of Figure 3. Since the Republican party has attracted both more liberal (before 1964) and more conservative identifiers over the 20-year span, the net effect—an average of both trends—is not large. But the inter-party differences in racial attitudes in 1964 and beyond are so large that they easily outweigh the earlier pattern. And the new identifiers (of Figure 3) today comprise more than half of each party's base. Thus we have a change of some permanence, for even if race now ceases to be a salient aspect of partisan divisions, the changes already effected will be carried into the future by the inertia of established loyalty.[9]

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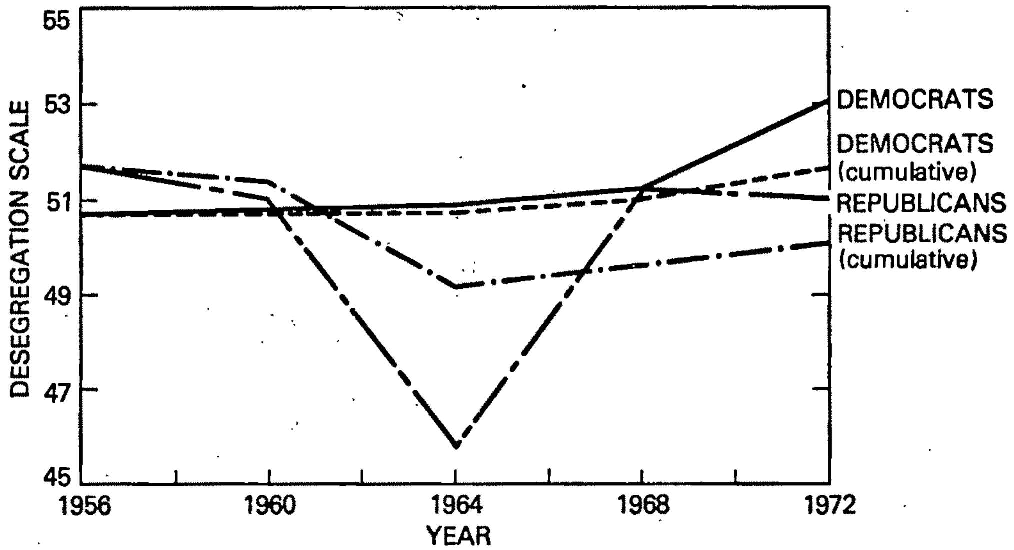
[8] The pattern is obfuscated by the fact that new Republican identifiers, while racially conservative relative to other new identifiers, are liberal relative to "old" Republicans. Thus the party in aggregate became more liberal by recruiting the most conservative group of new identifiers.

1976 and Beyond

The 1972 electoral contest is the centerpiece of this analysis for a number of reasons. It was the first U.S. presidential election subsequent to the electoral period when racial issues were generally believed to be highly salient. Thus it is important for us to speak to the contention that 1972 marked the end of racial politics. The quantity and diversity of racial attitude materials that the 1972 CPS study offers also are unsurpassed in the national election study series. And with neither Nixon nor McGovern closely identified in the public mind with the racial politics of the sixties, the 1972 study offers an opportunity to study the long-term impact of race relatively free from short-term disturbances.

The 1976 election, on the other hand, was clouded by Jimmy Carter's identification with southern regional pride, culture, and politics side by side with appeal to and support from black voters. This confusion makes the 1976 contest a poor vehicle for the study of the partisan evolution of racial issues. The 1976 CPS measures of racial attitudes also are not the equal of the earlier study. But to assert that an issue evolution was in progress compels us to look at this newest evidence.

Because we regard the 1976 contest with some suspicion, we treat it as an independent test of the issue evolution thesis.[10] Our method is as before:



Source: Computed from data collected in the 1972 Center for Political Studies Election Study, University of Michigan.

Figure 3. Desegregation Attitudes of New Party Identifiers by Year and Cumulatively Since 1956

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[9] The new partisans of Figure 3 are both black and white. But the interpretation of the figure would be considerably different if it were the case that blacks accounted for much of the observed change. They do not. Inter-party differences for whites only (Democratic mean minus Republican mean) are: 1952, -.04; 1956, -2.88; 1960, -3.89; 1964, +2.32; 1968, +.38; 1972, +.63. This is exactly the pattern of Figure 3, Republican racial liberalism through 1960 and conservatism in 1964 and after. Blacks are left in the figure out of theoretical preference, but their tiny numbers rule out significant impacts on the aggregated party groups.

[10] It is a genuinely independent test in the important sense that our analysis to this point was completed before we examined the 1976 study. Our predictions are

Table 1. Attitudes on Desegregation of 1976 and (Reconstructed) 1972 Party Identifiers*

Party	All Identifiers 1972	All Identifiers 1976	"New" Identifiers 1976
Democrat	50.73	51.18	54.40
Republican	48.05	48.22	50.20
Difference	2.68	2.96	4.20
	($p < .001$)	($p < .001$)	($p < .05$)
N	1035	1136	114

Source: University of Michigan Center for Political Studies 1976 American National Election Study.

*Entries are mean scores on desegregation scale. Higher scores indicate stronger advocacy of desegregation measures (see n. 11).

we project a scale of contemporary (i.e., 1976) attitudes toward desegregation[11] backward in time (to 1972) by reclassifying respondents who reported shifts in party identification or who were not old enough to vote at the earlier time. There are two possible outcomes of this analysis:

1. Party differences in attitudes toward desegregation could decline or even disappear over the 1972-1976 period. That would undermine the issue evolution thesis, leading to the inference that the observed party change was either weak or transitory.
2. Party differences on this issue could increase or stay about the same over the period. That would be evidence of the continuing partisan evolution of attitudes toward desegregation.

The outcome reported in Table 1 is clear. The desegregation attitudes of Republican and Democratic identifiers are significantly different in both 1972 and 1976, and the margin of difference grows. The partisan evolution of racial issues was continuing in 1976.

Comparison of the 1972 and 1976 cross sections is muddied by the fact that they are partially overlapping samples. We can get a somewhat cleaner view of change by looking only at new partisans,[12] those who either are newly eligible to vote or who report a change in party identification subsequent to 1972. That analysis, also reported in Table 1, shows that the Democratic party continued to recruit new identifiers more favorably disposed toward racial desegregation than their Republican counterparts, a difference that is statistically significant. It is notable that this occurred in the face of Jimmy Carter's evident appeal to white southerners of traditional inclination.

Table 2 is a verbal summary of the complicated changes we have observed since 1960. It displays a

Table 2. Sources and Consequences of Changes in Racial Attitudes, 1960-1976

	Sources of Change: New Democrats	Sources of Change: New Republicans	Net Difference Between Parties
1960	No Change	No Change	None
1964	No Change	More Conservative	New Difference
1968	No Change	No Change	Sustained Difference
1972	More Liberal	No Change	Increased Difference
1976	More Liberal	No Change	Increased Difference

Source: Authors' summary.

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thus a priori in more than the normal connotation of the term.

[11] The 1976 scale is composed of four items (school integration, neighborhood integration, "civil rights too fast," and [de]segregation) which were central to the 1972 desegregation scale (Carmines and Stimson, 1980) and continued in the later study. The scale is a sum of the four equally weighted items, expressed as a standard score with a mean and standard deviation of 50 and 10, respectively.

[12] Inter-party differences could also increase from the disappearance from the electorate of pre-realignment age cohorts, in effect, without continuing realignment. The separate examination of "new" partisans allows us to rule out this possible artifact.

growing split between the parties that is by no means symmetrical, a result of one powerful move to the right by the new Republicans in 1964 and two later counter-moves to the left by the new Democrats (in 1972 and 1976)—all superimposed on an apparent secular trend for the electorate.[13] Over this 12-year period the parties have become clearly differentiable, mainly as a result of the behavior of new partisans.

Desegregation has provoked a slow but significant shift in the partisan composition of the electorate. It has realigned the coalitional basis of the party system. It is a strange sort of realignment, however, for it appears to work to the detriment of the minority party. Not a new issue cleavage to balance the scale of party strength, desegregation appears rather to cause further imbalance. Why then would a minority party deliberately raise such an issue?

Segregationists as a pool of "available" party supporters are something of a mirage. They exist, and in no small number. But the slight association of segregationist views with age ($r = .15$) makes a conservative appeal on this issue a dubious long-run proposition for two reasons. One is that older voters with their established party loyalties are less available for conversion than their younger counterparts. The South in the last several elections illustrates the point: Republican success in attracting votes has been accompanied by failure to secure large numbers of conversions to the party (see Beck, 1977; Campbell, 1977a, 1977b; also see Wolfinger and Arseneau, 1978).

The regularity of human mortality is the second reason why an appeal to older voters with segregationist views is dubious in the long run. Older voters, once converted, cannot be counted upon to stay in the electorate very long. To court the old and forego the young has predictably devastating consequences. And that appears to be precisely what the Republican party has done. In the period since 1960, 51 percent of those newly identifying themselves with the Republicans are *not* newly eligible voters, while the comparable figure for the Democrats is 22.5 percent. The segregationists are a tempting target; elections can be won with them. But to court them may squander a party's future.

Modeling Issue Evolution

The concept of issue evolution is dynamic; it implies an ongoing process (Przeworski, 1975). We conceive the process of the partisan evolution of an issue as:

1. A party system that is in equilibrium with respect to issue X at time $t - 1$, followed by
2. A disturbance at time t which results in the selective recruitment of party identifiers with regard to X, followed by
3. Either continuing selective recruitment or decay of th time t impact at $t + 1$ and beyond.

Each party is assumed to consist of a stable base of identifiers plus an increment[14] added at each election. Evolving issues are assumed to affect only the increment, except under extraordinary conditions. The net impact of issues on the party system is thus limited by the size of the increments affected. As a proportion of the party, each increment is largest the year it is added, declining thereafter as newer increments are added.

The constraints on issue evolution are thus severe. Few issues are powerful enough to affect the recruitment of party identifiers. For issues powerful enough at one time—the controversy over the Vietnam War may be a good example—initial effects are small, since the increment of new identifiers is only a minor proportion of the party. And even that small effect decays as the affected increment becomes an ever-smaller proportion of the party. To exert substantial change, an issue must influence the recruitment of party identifiers over an extended period.

These simple assumptions allow us to model some possible alternative conceptions of the impact of desegregation on the American party structure. With regard to the apparent partisan evolution beginning in 1964, we can examine the predictions of three models. They are, in order of impact:

Model 1: "Flash in the Pan"

> The apparent 1964 partisan change was purely transitory, perhaps only a temporary reaction to Goldwater.
>
> *Prediction:* Party differences on the desegregation issue should revert to their pre-1964 level. That is not the case.

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[13] Throughout this entire period (1952 to 1976) Independents have been more liberal in their racial attitudes than the identifiers of either party. Converse (1976) has speculated that racial conflict (as well as public reactions to the war in Vietnam) contributed to the increase in numbers of Independents beginning in the mid-1960s. Some of our analysis is suggestive of the truth of Converse's conclusion, but a thorough analysis of this phenomenon requires a different analytic strategy than that developed in this article.

[14] We use the term "increment" in preference to the familiar "cohort" to maintain the theoretical distinction between new partisans (of any age) and newly eligible partisans.

Model 2: "Incremental Shock"

> The 1964 impact was permanent, but it affected only party identifiers recruited in that year.
>
> *Prediction:* Since we know (a) the attitudes of the two parties in 1964, and (b) the size of the 1964 party groups as a proportion of all partisans in later years, we can predict expected inter-party differences purely as a function of (a) and (b) and the assumption that later increments are undifferentiated by racial attitudes. The aggregate difference between the parties will be at maximum in 1964, reflecting the impact of the new partisans with their distinctive racial attitudes. The difference should decline steadily (and predictably) after 1964 as undifferentiated increments are added in succession to the previous party base. The predicted decay of party distinctiveness over time appears in the center column of Table 3.

Model 3: "Issue Evolution"

> The 1964 realignment was permanent, and the process it began is continuing.
>
> *Prediction:* Party differences do not decay over time.[15] This will be the case if and only if differences in successive increments are greater than or equal to existing aggregate party differences. The last column of Table 3 shows actual attitude differences between the party groups at time *t* (1964) and beyond.

The pattern of decay predicted by the "Incremental Shock" model does not occur. Differences remained about constant in the confusing 1968 three-way contest. In two subsequent elections they have increased.[14] We are left with the inference of Model 3, that an issue evolution has occurred. With the 1964-1976 increments now more than a third of all party identifiers, inter-party differences are growing by the year. It is likely, given the self-generating dynamic of issue evolution, that this process will continue. Even if it does not, what has already occurred by 1976—barring major realignment along some other dimension—will be carried into the twenty-first century.

Another Look at Issue Evolution: A Cross-Method Validation

Reconstructed party identifications have dominated the analysis to this point. The reconstruction technique gives us a portrait of a mass electorate undergoing slow, steady transformation. Such transformation would be expected to yield modest (but significant) empirical evidence, and that is what we have seen. But modest evidence—

Table 3. Party Differences in Attitudes Toward Desegregation: Expected[a] and Actual

		Ratio 1964 Identifiers to All Party Identifiers	Expected Party Differences	Actual Party Differences
T	1964	1.000	.993	.993
T+1	1968	.912	.906	.968
T+2	1972	.795	.789	1.197
T+3	1976	.715	.710	2.960[b]

Source: University of Michigan Center for Political Studies American National Election Studies for 1972 and 1976.

[a] Expected differences are simply the 1964 difference (.993) multiplied by the ratio of 1964 (and earlier) identifiers to all party identifiers in each year.

[b] Estimation of the exact 1976 difference is made problematic because measurement of desegregation attitudes is not perfectly comparable to the earlier scale. The 1976 scale is composed of the same dominant items and is standardized to the same mean and standard deviation (50, 10) as the earlier scale. An alternate estimate can be derived from combining the 1972-1976 change in Table 1 (based wholly on the 1976 scale) with the actual 1972 difference. That lower estimate (1.480) also supports our interpretation.

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[15] Readers familiar with the time-series tradition will recognize Model 2 (Incremental Shock) as a (stationary) first-order auto-regressive process and the expected inter-party differences in Table 3 as a variation on the characteristic exponential decay process. Model 3 is also AR(1), but it is nonstationary (i.e., not tending toward equilibrium), which accords well with our conceptualization of issue evolution.

[14] There is some irony in the fact that distinctiveness appears to grow in the face of declining salience. It seems reasonable to speculate that the open conflict of the 1960s was prerequisite to: (1) clarified party images and hence (2) continuing selective recruitment in the 1970s.

however appropriate to expectations—may have many explanations, one of which is artifact. It is thus reasonable to ask whether these findings would be sustained outside the context of the method in which they were developed.

The obvious alternative to reconstructing is to use contemporaneous reports from voters regarding their attitudes toward desegregation and party identification. The difficulties involved in such over-time comparisons from cross-sectional data are well known (see Achen, 1975: Bishop et al., 1978; Sullivan et al., 1978). The important point for our purpose, however, is that they are a different set of problems from those associated with the reconstruction technique. We have, therefore, conducted cross-sectional analyses of each of the SRC/CPS presidential election studies from 1956 through 1976. A racial desegregation scale[17] is developed from the best items of each. The result is arrayed in Table 4 in party bases, new increments for each year, and the difference (Democratic minus Republican) between new party increments.

The patterns of Table 4 are notably convergent with Figures 2 and 3. Although only the data from 1972 and 1976 are common to both analyses, with but very small deviations the same year-to-year patterns are found in both. The Republican ("base") party emerges are more racially liberal than the Democrats through 1960, followed by a turn to the right in 1964 and beyond. The interparty differences are highly significant for 1964, 1968, and 1976. But more important, the pattern across all six elections is as predicted.

The changes are again more visible among the new partisans, a category that includes the newly eligible and those reporting a recent party change. As is also shown in Table 4, those newly identifying with the Republican party tended to be more liberal in 1956 and 1960, but an abrupt

Table 4. Desegregation Attitudes of Partisans, Old and New

Year	Democrats Previous Base	Democrats New Increment[a]	Republicans Previous Base	Republicans New Increment	Democratic Minus Republican Difference in Increments
1956	49.132	49.606	50.252	51.131	−1.525
(N)	(404)	(35)	(284)	(23)	
1960	49.162	49.465	49.784	55.025	−5.560
(N)[b]	(640)	(59)	(436)	(27)	
1964	50.555	50.200	48.257	45.643	+4.557
(N)	(396)	(57)	(192)	(20)	
1968	51.528	53.610	48.126	50.923	+2.687
(N)	(462)	(38)	(236)	(18)	
1972	49.282	53.301	48.382	51.041	+2.260
(N)	(726)	(143)	(466)	(74)	
1976	50.808	54.403	48.025	50.202	+4.201
(N)	(645)	(76)	(378)	(38)	

Source: University of Michigan Survey Research Center American National Election Studies for 1956, 1960, 1964, 1968, 1972 and 1976.

[a] Newly eligible to vote or reported party identification change subsequent to the previous presidential election.

[b] Weighted.

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[17] Assuming measurement comparabilty across time is chancy at best; neither the items, their wording, or indeed the issue itself is constant from year to year. Thus we report the cross-sectional analysis with some trepidation and only in a secondary confirmatory role. Each desegregation scale is as before set to a standard metric (50,10). The 1972 cross section is based upon the desegregation factor we have already employed. Items used for other years are "Civil Rights Movement Too Fast/Too Slow" (1964, 1968, 1976); "Favor Desegregation" (1964, 1968, 1976); "School Integration" (1956, 1960, 1964, 1968, 1976); "Fair Treatment in Jobs/Housing" (1956, 1960, 1964, 1968); public accommodations (1964, 1968); and open housing (1964, 1968, 1976). The 1960 scale is subject to unusually large fluctuations from the combination of both few items and very few (unweighted) respondents.

change occurred in 1965. Beginning in that year and continuing unabated through 1976, newly identified Republicans were considerably more conservative on racial issues than new Democrats.[18] Differences between these much smaller groups are significant for 1960, 1964, and 1976, and nearly so for 1968 and 1972. The over-time pattern with the partisan increments, as with the base, is perfect.

The cross-sectional analysis thus tells essentially the same story as the earlier reconstructions. Partisanship and racial attitudes among the electorate are undergoing secular change; an issue evolution is in progress.

Conclusion

The picture presented of the electorate and the party system is often a quite static, if not stagnant one. Thought to be fixed in time, anchored firmly to the social structure, and dependent for vitality upon the economic system, mass political behavior is conceived of as both stationary and nonautonomous. Periodically, it is assumed, the mold is broken as abrupt and massive changes are manifested in a critical election realignment, resulting in the overturning of the old party system and the forging of a new one more responsive to the altered economic and social conditions.

Political reality, however, is considerably more complex and less dramatic than the contours painted in this oversimplified picture. The electorate is continuously undergoing small, incremental but persistent changes that over an extended period can leave an indelible imprint on the political landscape. The theory of issue evolution is a theory of normal partisan change—the gradual transformation of the party system caused by evolving issues and effected through population replacement.

How large is that change? Students of critical election realignments look for convulsive shifts in partisan alignment. What we have demonstrated is that one particular issue, racial desegregation, has provoked measurable change in the recruitment of party identifiers over a 12-year period, less than a full generation. Evolutionary change is difficult to appreciate, becuase it is never so large from one election to the next to attract much attention.[19] But its cumulative impact is substantial when extrapolated to the millions of voting-age citizens and its implications for the development of the party system are unmistakable.

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[18] Again, to be sure that these cross-sectional trends were not due solely to the behavior of black partisans, we conducted an identical analysis among new white partisans only. The *differences* in desegregation attitudes between new partisans were: 1956, −.162; 1960, −6.04; 1964, +4.06; 1968, +1.22; 1972, +.61; 1976, +3.29. These figures support the same interpretations; new Republicans are more liberal prior to 1964 and more conservative thereafter.

[19] The principal characteristic of evolutionary change in Darwinian biology is the notion that changes of considerable magnitude occur over very long periods of time by the successive replacement of generations of species, one by another. The generations differ only in the proportions of individuals possessing minor distinctive traits, but that is enough to effect large scale change over time. That is precisely the postulated causal mechanism of issue evolution.

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Incumbency, Electoral Safety, and Turnover in the House of Representatives, 1952-76

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Two hypotheses have received a great deal of attention from students of congressional elections. First, many analysts have concluded that an increase in the electoral value attached to incumbency accounts for the rise in incumbents' victory margins observed since the mid-sixties. Second, many scholars have attributed the decline in electoral turnover to the percentage increase in victory margins. Analyzing the electoral histories of 13 House cohorts (1952-76), this study reexamines these two hypotheses within the framework of a quasi-experimental design. The results presented here, in contrast to those of previous studies, cast doubt on the increased incumbency effect. Moreover, little relationship is found between an increase in electoral safety, as reflected by increased victory margins, and a decline in electoral turnover.

Most students of congressional elections have concluded that the electoral advantage derived from incumbency has increased since the mid-1960s. Longitudinal analyses of electoral margins (Born, 1979; Cover, 1977; Cover and Mayhew, 1977; Burnham, 1975b; Mayhew, 1974; Tufte, 1973; Erikson, 1972) indicate that members of the House of Representatives have shown a greater tendency to return to Washington via safe victory margins, the increase in electoral margins hypothetically due to the incumbency factor. In addition, several scholars have viewed the increase in electoral margins among incumbent House members as the primary factor behind the declining rate of electoral turnover (Fiorina, 1977b; Cover and Mayhew, 1977; Burnham, 1975a, 1974; Mayhew, 1974; Tufte, 1975, 1974, 1973).[1] The logic behind this position operates on the assumption that representatives from marginal districts are most vulnerable to partisan shifts in the electorate. Consequently, as the number of marginal incumbent districts dwindles, and the great majority of incumbents continue to seek reelection, the probability of electoral turnover declines. Thus the linkage between victory margins and turnover implies that percentage differences translate into electoral differences. Both hypotheses, then, are linked to the decline in the number of marginal districts among incumbent races: just as the increase in the number of safe districts is attributed to an increase in the electoral value of incumbency, so the decline in electoral turnover is attributed to the increase in the number of safe districts.

Explanations of the increase in the electoral value of incumbency have varied. Several imply that House members themselves have engineered the increased advantage, either through "collusive gerrymanders" during the redistricting of the 1960s (Tufte, 1974, 1973), expanded use of office perquisites such as the frank (Mayhew, 1974) and district travel allowances (Cover and Mayhew, 1977), increased attention to casework services (Fiorina, 1977a, 1977b; Mayhew, 1974), or more skilled "position-taking" (Mayhew, 1974). In contrast, other studies contend that the increased advantage finds its source not among incumbents' activities but in the electorate. According to these studies, the erosion of partisan loyalty, complemented by a larger pool of Independents, has created an electoral environment in which the incumbency cue can challenge the partisan cue in the voting calculus (Nelson, 1978-79; Cover and Mayhew, 1977; Cover, 1977; Ferejohn, 1977; Mayhew, 1974; Burnham, 1975a, 1974; Erikson, 1972). Still other research implies that the inflated victory margins of incumbents can be attributed to ineffective opposition. Often poorly financed and handicapped by visibility problems vis-à-vis their opponents, House challengers have been unable to neutralize the advantage of incumbency (Abramowitz, 1980; Mann and Wolfinger, 1980; Mann, 1978; Jacobson, 1978).

While the explanation of the increased electoral

I am particularly grateful to David W. Brady and David John Gow for their critical comments on earlier drafts of this article. In addition, I thank William Panning, Elise Marion Kornmayer, Robert Stein, and the anonymous referees for helpful suggestions. Last, I wish to record my intellectual debt to Joseph Cooper and Lawrence C. Dodd. An earlier version of this article was presented at the annual meeting of the American Political Science Association, 1980.

[1] This article distinguishes turnover that is the result of electoral competition in the general election from the overall level of turnover that may additionally result from death, retirement, or loss in the primary. Because of the relationships considered here, turnover is examined only insofar as it relates to partisan turnover in a general election.

advantage of incumbency remains unresolved, no one disputes the validity of the increase itself and few question its ramifications. To be sure, the difficulties inherent in partitioning the determinants of congressional elections undermine dependable replicability and cross-validation. Nevertheless, the methodologies employed in previous research have inhibited the development of explicit linkages among incumbency, electoral margins, and turnover.

For example, evidence suggests that the aggregate decline in electoral turnover covaries with increases in the number of safe seats (Mayhew, 1974; Tufte, 1973). But the relationship between turnover and prior safe margins is more tenuous in analyses of individual races (Mann, 1978; Erikson, 1976). Similarly, studies that have tested for an increase in the electoral value of incumbency have observed trends in either aggregate-level distributions (Burnham, 1975a, 1975b; Mayhew, 1974; Tufte, 1973) or summary statistics, such as mean percentages (Born, 1979; Cover, 1977; Cover and Mayhew, 1977; Mayhew, 1974; Erikson, 1971, 1972). The latter include the familiar "sophomore surge" and "retirement penalty" measures. Either approach, however, necessarily obscures more subtle action that could plausibly account for the increase in the number of safe incumbent districts.

This article reexamines the hypothesized relationships between incumbency and electoral safety in individual races and, in turn, between electoral safety and turnover. The method of analysis involves a comparison of the electoral patterns in 13 House cohorts (1952-76) within the framework of a quasi-experimental design. Evidence presented here conflicts with previous findings. Specifically, the results cast doubt on both the magnitude and the uniformity of the increased incumbency effect. Moreover, there is little evidence to support the hypothesis that an increase in electoral safety results in a decline in electoral turnover.

A Quasi-Experimental Design

In order to determine the electoral effects of incumbency, it is necessary first to control for incumbency. Since the electoral margins of open-seat winners are by definition not subject to an incumbency effect, they provide a baseline against which the electoral value of incumbency can be measured.

This study examines House members who are drawn from each class of open-seat winners over the period 1952-76. Though the 13 cohorts differ in that each represents a different generation of open-seat winners, they are equivalent in that each cohort initially controls for incumbency. Admittedly, insofar as open-seat winners comprise but a small fraction of the total House membership, the choice to examine only the electoral margins of these individuals jeopardizes the external validity of this study. Nonetheless, a comparison of representatives' electoral margins at the time of their open-seat victories with their margins as incumbents represents the only method in which a test for the incumbency effect can remain internally valid. Though both types of validity are desirable, internal validity is the sine qua non (Campbell and Stanley, 1963, p. 5).

The analysis focuses on 11 of the 13 cohorts, those first elected from 1952 through 1972. In the language provided by Campbell and Stanley (1963), the quasi-experimental design employed here entails an examination of members in the 11 cohorts (1952-72) before and after the test variable or treatment is introduced. The design may be diagrammed as follows:

	$Time_1$	$Time_2$	$Time_3$	$Time_4$
Group A (1952 Cohort)	O_{A1}	XO_{A2}	XO_{A3}	XO_{A4}
Group B (1954 Cohort)	O_{B1}	XO_{B2}	XO_{B3}	XO_{B4}
	⋮	⋮	⋮	⋮
Group K (1972 Cohort)	O_{K1}	XO_{K2}	XO_{K3}	XO_{K4}

For the members in each cohort, an O signifies the representative's electoral margin while an X signifies the incumbency factor. Thus the subsequent electoral margins of open-seat winners in each cohort are examined with incumbency held constant over three points in time. Since the design allows longitudinal analysis of the electoral effects of incumbency within each cohort, it will be referred to as the *group time-series*.

The design translates into a set of alternative patterns of reelection. These patterns are displayed graphically in Figure 1. The peak in the diagram represents the member's electoral margin at the time of the open-seat victory. Open-seat winners who were reelected as incumbents in the following three consecutive elections would then fall into one of the eight patterns shown in Figure 1. Thus the design generates expectations as to the patterns that should appear given the hypothesized relationships among incumbency, electoral safety, and electoral turnover. To facilitate analysis, these eight patterns are aggregated into three categories.

Marginal open-seat winners included in pattern 1 are termed *invariant*. These are representatives who continued to be elected by marginal percentages as incumbents. Pattern 1 implies a null relationship between incumbency and electoral safety in that incumbency exerts virtually no effect on the electoral margin.

Marginal open-seat winners who fall into patterns 3, 5, 6, and 7 are termed *variable*. These are representatives who as incumbents won by a safe margin at least once but did not maintain a safe margin in subsequent elections. Thus, among these individuals, the safe margin does not predict to the size of the future margin. Because their electoral margins are variable, in that they fluctuate between marginal and safe percentages, a null relationship between electoral safety and incumbency is implied.

Safe open-seat winners who fall into patterns 1-7 are also termed *variable*. Each of these patterns would capture safe open-seat winners who as incumbents won by a marginal percentage of the vote in at least one of their three subsequent reelections. Again, for them, the safe margin does not predict to the size of the future margin. Thus for safe open-seat winners, patterns 1-7 imply a null relationship between incumbency and electoral safety.

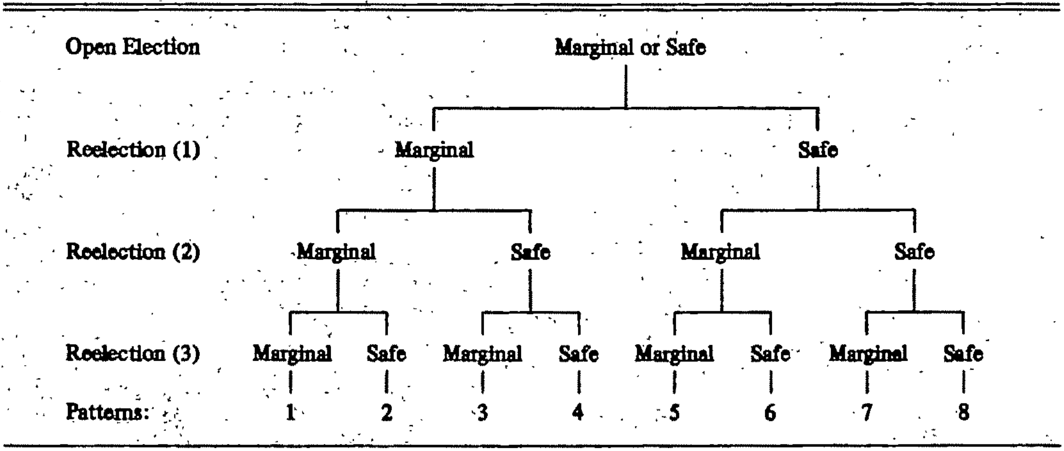
Marginal open-seat winners who fall into patterns 2, 4, and 8 are termed *single-peaked*, which is used here to indicate *non-decreasing change* over time (Black, 1958).[3] These patterns, in contrast to those discussed above, allow the inference of a positive relationship between incumbency and electoral safety. That is, patterns 2 and 4 imply that incumbency exerts an effect, albeit a delayed one, on the electoral margin. Pattern 8 implies that the effects of incumbency on electoral margins are immediate and prolonged.

In the same vein, safe open-seat winners who fall into pattern 8 are termed *single-peaked*. This would include those safe open-seat winners who as incumbents continued to be reelected by safe margins. This pattern, with qualification, implies a rejection of the null relationship.

According to the incumbency hypothesis, patterns of reelection should have changed during and after the mid-sixties. The expectation is that the percentage of marginal open-seat winners who exhibit single-peaked patterns of reelection has increased. An increase in the percentage of congressmen included in pattern 8 would provide the strongest support. Likewise, the percentage of safe open-seat winners who exhibit the single-peaked pattern of reelection represented by pattern 8 should have increased. Though an increase in the percentage of safe open-seat winners who sustain electoral safety does not allow the increase in electoral safety to be attributed explicitly to incumbency (since these members ran safe races before they were incumbents), it would not disconfirm the hypothesized relationship. Indeed, it is reasonable to infer that an increase in the electoral value of incumbency would account for an increase in the number of individuals who sustain their safe margins. In sum, the phenomenon that suggests a relationship between incumbency and electoral safety in this design is non-decreasing change over time in the electoral margins of individual incumbents.

This design also allows for disconfirmation of a

Figure 1. Patterns of Reelection Margins for Marginal and Safe Open-Seat Winners, U.S. House of Representatives



Source: Compiled by the author.

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[3] Single-peakedness here merely indicates non-decreasing change; this differs from Black's use of the term.

rival hypothesis. That is, aggregate analysis implies that before the electoral value of incumbency increased, representatives from marginal open seats would largely fall within pattern 1: as incumbents, their seats remained marginal (if they were indeed reelected at all). In contrast, after the value of incumbency increased, aggregate analysis implies that representatives from marginal open seats would largely fall into pattern 8: as incumbents they acquired and sustained electoral safety. However, an increase in the variability of the reelection patterns of marginal open-seat winners (patterns 3, 5, 6, and 7) could account for the increase in the electoral safety of incumbents reflected in the aggregate. But by the same token, a trend that shows marginal open-seat winners' electoral patterns increasingly fluctuating between marginal and safe percentages probably signals an increase in the importance of idiosyncratic or district-specific factors rather than a general increase in the electoral value of incumbency.

Of course, these patterns will not include all representatives from open seats because not all of them were reelected for three consecutive terms. Here, too, the expectation is that the rate of turnover observed across cohorts should have changed during and after the mid-sixties due to the increase in safe margins among incumbents. The design would reflect the general increase in safe margins evident among incumbents in three ways: (1) an increase in the percentage of *variable* marginal open-seat winners; (2) an increase in the percentage of *single-peaked* marginal open-seat winners, and (3) a decrease in the percentage of *variable* safe open-seat winners. Were any of these trends observed across cohorts, the expectation is that the rate of electoral turnover would have declined in kind. But we may infer that a decline in turnover has resulted from an increased incumbent advantage only if both single-peaked patterns of reelection increased and cohort turnover decreased.

In sum, the following patterns would support the hypothesized relationships among incumbency, electoral safety, and turnover:

1. An increase across cohorts in the percentage of marginal and safe open-seat winners who demonstrate single-peaked patterns of reelection;
2. An inverse relationship across cohorts between electoral safety and electoral turnover, and
3. An inverse relationship across cohorts between single-peaked patterns of reelection and electoral turnover.

The general design under which this study is conducted incorporates as a special case the "sophomore surge," i.e., the increase in votes received by the first-term incumbent (see, for example, Born, 1979; Cover and Mayhew, 1977; Cover, 1977; and Erikson, 1971, 1972).[3] Since the sophomore surge measure represents a comparison of the member's electoral margin at the time of his or her open election with that obtained in the next election, it shall be referred to here as a *pre-test/post-test*.

Despite the more limited time frame, I present results for the pre-test/post-test for three reasons. First, it permits straightforward comparison to previous findings obtained under similar conditions. Second, it allows analysis of additional classes of open-seat winners, i.e., those of 1974 and 1976. Third and most important, through comparing the results obtained under a pre-test/post-test with those obtained under the group time-series, short-term effects of incumbency on electoral margins and turnover are juxtaposed with long-term effects.

Thus, under the pre-test/post-test, an increase in the percentage of marginal open-seat winners of both major parties who were reelected as first-term incumbents by safe margins would imply an increase in the electoral value of incumbency. In addition, since the marginal representative is hypothetically more vulnerable to turnover, there should have been a disparity within each cohort between the reelection rates of marginal and safe open-seat winners. If, however, incumbency emerged as a protective mechanism, this disparity should have decreased across cohorts. Just as an increase across cohorts in the marginal-to-safe pattern would attest to an increase in the electoral value attached to incumbency, so would a decline in turnover among marginal open-seat winners. Both imply that incumbency has generated increased electoral benefits. Therefore, turnover should vary inversely with electoral safety across cohorts.

In sum, the following patterns would support the hypothesized relationships among incumbency, electoral safety, and electoral turnover:

1. An increase across cohorts in the percentage of open-seat winners who demonstrate the marginal-to-safe pattern;
2. A uniform distribution of the marginal-to-safe pattern across party lines;
3. A decrease in the disparity between the reelection rates of marginal and safe open-seat winners within each cohort, and

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[3] Cover (1977), Cover and Mayhew (1977), and Erikson (1971) "adjust" the mean surge with the national swing. Born (1979) and Erikson (1972) use the swing as a measure from which to tabulate deviations.

4. An inverse relationship across cohorts between electoral safety and turnover.

The Sample

The sample includes representatives drawn from each generation of open-seat winners over the period 1952-76 who ran in contested elections. The sample excludes non-incumbent representatives who defeated incumbents because it was assumed that the incumbency status of their opponent would have a deflationary effect on victory margins. Contested open seats were defined as those in which neither candidate won more than 95 percent of the vote (Mayhew, 1974).[4] Among contested open seats, the electoral margins of cohort members were classified as marginal or safe. A margin of 60 percent or greater indicates electoral safety (Cover and Mayhew, 1977; Erikson, 1976).[5] Table 1 provides an electoral profile of open-seat winners over the period examined, and indicates that the vast majority of open-seat races were contested elections.[6] Moreover, the number of uncontested open races declined over the period. Of contested open races, a majority of House members in each year entered office within the marginal range, the two exceptions being 1954 and 1966.

Results: Incumbency and Electoral Safety

First of all, let us compare the results of this study to previous findings. Table 2 reports the

Table 1. Electoral Profile of Open-Seat Winners in the U.S. House of Representatives, 1952-76

Year	Percent Marginal	Percent Safe	Percent Contested	Percent Uncontested	Total (N)
1952	62.5 (35)	26.8 (15)	89.3 (50)	10.7 (6)	(56)
1954	36.7 (11)	36.7 (11)	73.3 (22)[a]	26.7 (8)	(30)
1956	50.0 (14)	42.9 (12)	92.9 (26)	7.1 (2)	(28)
1958	64.3 (27)	28.6 (12)	92.9 (39)	7.1 (3)	(42)
1960	65.6 (21)	25.0 (8)	90.6 (29)	9.4 (3)	(32)
1962	66.7 (30)	31.1 (14)	97.8 (44)	2.2 (1)	(45)
1964	60.5 (26)	34.9 (15)	95.3 (41)[a]	4.7 (2)	(43)
1966	43.8 (14)	53.1 (17)	96.9 (31)	3.1 (1)	(32)
1968	58.6 (17)	37.9 (11)	96.5 (28)	3.5 (1)	(29)
1970	70.3 (26)	27.0 (10)	99.9 (36)	0.02 (1)	(37)
1972	57.4 (31)	37.0 (20)	94.4 (51)	5.6 (3)	(54)
1974	68.1 (32)	31.9 (15)	100.0 (47)	0.0 (0)	(47)
1976	52.8 (28)	43.4 (23)	96.2 (51)	3.8 (2)	(53)
Mean	58.3 (24)	35.1 (14)[b]	93.6 (38)	6.9 (2.5)	(41)[b]

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-78).

[a] Rounding error.

[b] Rounded to nearest integer.

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[4] The sample of House members reflects two further qualifications. First, it includes only those House members with no prior experience in Congress who ran in open races. This might appear self-evident given that open seats are used. But occasionally a former incumbent lost an election, and when his or her successor did not run in a later election, the former incumbent was again elected to the House. Thus, technically, a former incumbent could run in an open race. To control for all possible incumbency effects, I have excluded these individuals from the sample. Second, all candidates who ran in open special elections were excluded from the sample. Since different variables might logically come into play in special elections, for purposes of more rigorous comparison I have excluded those who entered office as the result of special elections.

[5] Though Tufte (1973), using a 53 percent cutoff point, finds a somewhat less dramatic decline in the number of marginal incumbent races, Mayhew (1974) shows that regardless of whether a 55 or 60 percent limit is chosen, the number of marginal incumbents over the 1956-72 period has halved. Though there is precedent for both the 55 and 60 percent limit, the latter was chosen here because it offers the more stringent definition of a safe margin, the effect of which is to bias the design against rejection of the null hypothesis.

[6] Analysis not presented here shows that, using the categories employed in previous research, e.g., Erikson (1971, 1972), the geographical distribution of open seats largely parallels that of the House. Data, of course, will be provided on request.

Table 2. Sophomore Surge across Marginal Open Districts in House Elections, 1952-76

Class	$\bar{X}$ Percent Surge	Standard Deviation	Percent Marginal-to-Safe*
1952	.97 (34)	4.7	20.0
1954	4.40 (10)	4.47	60.0
1956	–.21 (14)	6.99	28.6
1958	2.28 (25)	6.02	28.0
1960	4.14 (21)	4.77	38.1
1962	5.86 (29)	11.61	44.4
1964	8.71 (24)	17.50	54.2
1966	9.78 (14)	10.86	57.1
1968	8.06 (16)	7.70	50.0
1970	9.19 (25)	12.24	64.0
1972	5.73 (30)	11.84	46.7
1974	9.53 (32)	12.16	59.4
1976	12.64 (25)	15.90	56.0

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-78).

*These percentages represent the number of marginal open-seat winners who ran safe races as first-term incumbents. The percentages were calculated against the number of marginal open-seat winners in each class who participated in the subsequent general election.

mean vote increase acquired by marginal open-seat winners when they ran for reelection as first-term incumbents. Because this measure is calculated only across marginal open seats, it differs from previous surge measures. If the increased surge found in prior research applies to the disappearance of the marginal district, this method should replicate previous findings.

At first glance, the figures on the mean surge largely substantiate earlier reports. In the early sixties the average surge gradually increased, the first notable jump occurring in 1966. Members from marginal open seats in 1964 on the average increased their electoral margin by 8.71 percent of the vote in 1966. Such a percentage increase, of course, is likely to mean the difference between a marginal and a safe victory. Subsequent cohorts (1966-76) showed a similar increase in votes between the open race and the first election as an incumbent. The magnitude of the average surge did not drop to pre-1964 levels, with the exception of the 1972 marginal open-seat winners. Even in this case, the average surge fell only below that of the 1962 cohort and remained higher than that demonstrated by all other pre-1964 marginal open-seat winners.

But more important to the linkage between electoral safety and incumbency, after 1960 the mean surge loses considerable strength as an indicator of how uniformly the average vote increase was distributed among each class of marginal open-seat winners elected after 1960 were likely deviations increase dramatically. From these figures, it may be inferred that classes of marginal open-seat winners elected after 1960 were likely to contain representatives who as first-term incumbents ran well ahead of the average surge. The outlier is, in short, a post-1960 phenomenon. Thus during the period most pertinent to the incumbency thesis, the mean vote surge obscures the general impact of the incumbency factor.

To capture the general trend across cohorts, the last column in Table 2 reports the percentage of marginal open-seat winners who acquired safe margins as first-term incumbents. The percentage of marginal open-seat winners who won by a safe margin in their first elections as incumbents gradually increased from 28 percent in the 1958 cohort to 57.1 percent in the 1966 cohort. Though the percentage dropped with the 1968 cohort, it rose again in 1970. For the post-1964 period, only in the 1972 cohort did fewer than half of the members from marginal open seats win by a safe percentage of the vote in their first reelection.

The mean percentage of individuals who exhibited the marginal-to-safe pattern across the first six cohorts (1952-62) is 36.5 percent with a standard deviation of 14.3. The mean percentage across the next seven cohorts (1964-76) is 55.3 percent with a standard deviation of 5.8. Thus, cohorts in the second period (1964-76) increasingly and consistently demonstrated the marginal-to-safe pattern.

The absolute percentages, however, are the telling indicators. The peak percentage occurred among representatives first elected in 1970, when a record 64 percent of them became safe sophomores. Moreover, there is no single breakpoint that distinguishes the mid-1960s; rather, after 1958 there was a general trend toward an increase in the marginal-to-safe pattern. Although the evidence indicates that the marginal-to-safe pattern has increased since 1964, this pattern is still far from the rule.

The partisan distribution of the marginal-to-safe pattern casts further doubt on the uniformity of an increased incumbent advantage. Table 3 reports the percentage of Democrats and Republicans from marginal open seats who acquired a safe victory margin in their sophomore election. A comparison of the mean percentages indicates that the proportion of Democrats in the second period (1964-76) who acquired electoral safety as first-term incumbents has risen only slightly. The proportion of Republicans, on the other hand, has shown a marked increase, from an average of 30.5 percent for the first period to an average of 64.6 in the second. Overall, a comparison of the means for the two parties in the two periods shows that an increase in the marginal-to-safe pattern has occurred in both parties, though more demonstrably among Republicans.

The table also shows, however, that cohorts in the second period have paralleled those in the first, insofar as in each cohort the marginal-to-safe pattern has been largely limited to members of one party. Only in the 1968 and 1976 cohorts did a comparable proportion of marginal open-seat winners in both parties run safe races as incumbents, and even then the proportion of each party that did is hardly overwhelming. "Disparity" figures are provided in the last column on the presumption that the disparity between Democratic and Republican marginal open-seat winners who went safe as incumbents should have declined after 1964 if incumbents were uniformly benefiting from an increased advantage. A comparison of the mean disparity for the two periods indicates that, contrary to expectations, the disparity between the Republicans and Democrats who demonstrated the marginal-to-safe pattern was greater in the second period.

A possible explanation of the pattern of disparities may lie in the effects of presidential coattails.[7] The expectation is that members first elected in off-years should have been systematical-

Table 3. Percentage of Safe Democratic and Republican Sophomore Representatives from Marginal Open Seats, 1952-76

	Marginal Open-Seat Winners				
Class	Democratic Percent Safe	(TOTD)[a]	Republican Percent Safe	(TOTR)[a]	Disparity[b]
1952	33.3 (5)	(15)	10.5 (2)	(19)	22.8
1954	50.0 (1)	(2)	62.5 (5)	(8)	12.5
1956	66.7 (2)	(3)	18.2 (2)	(11)	48.5
1958	27.3 (3)	(11)	28.6 (4)	(14)	1.3
1960	25.0 (2)	(8)	46.2 (6)	(13)	21.2
1962	58.8 (10)	(17)	16.7 (2)	(12)	42.1
Mean	43.5		30.5		24.7
1964	15.4 (2)	(13)	100.0 (11)	(11)	84.6
1966	25.0 (1)	(4)	70.0 (7)	(10)	45.0
1968	55.6 (5)	(9)	42.9 (3)	(7)	12.7
1970	50.0 (5)	(10)	73.3 (11)	(15)	23.3
1972	100.0 (7)	(7)	30.4 (7)	(23)	69.6
1974	50.0 (11)	(22)	80.0 (8)	(10)	30.0
1976	56.3 (9)	(16)	55.6 (5)	(9)	.3
Mean	50.3		64.6		37.9

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-1978).

[a] TOTD and TOTR represent the total number of Democrats and Republicans, respectively, in each class of marginal open-seat winners who participated in the subsequent general election. Democratic and Republican percentages were calculated against TOTD and TOTR, respectively.

[b] Disparity is the absolute value of the difference between the Democratic and Republican percentage of safe sophomores.

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[7] I would like to thank William Panning for calling this to my attention.

ly advantaged (or disadvantaged) in the following presidential election year when they ran as first-term incumbents. So, for example, among 1966 open-seat winners, more Republicans than Democrats should have gone safe in 1968, given the 1968 pro-Republican presidential swing. Similarly, party members first elected under the auspices of an advantageous presidential swing should have suffered disproportionate losses in voting strength relative to other-party cohort members. Thus for example, Democratic open-seat winners elected in 1952 with Eisenhower should have surpassed their Republican counterparts in 1954. This reasoning explains the disparate marginal-to-safe patterning in all but two cohorts, 1960 and 1974. The expectation for both of these cohorts is that a comparable proportion of Democratic and Republican open-seat winners should have gone safe in 1962 and 1976, given the close presidential elections of 1960 and 1976. The evidence, contrary to expectations, shows a disparity between Democratic and Republican marginal-to-safe patterns. This explanation, finally, should be seen in light of the conclusions drawn in previous research that testify to the increasing divergence between presidential and congressional elections (Burnham, 1975a; Mayhew, 1974).

Thus far, the pre-test/post-test yields negligible evidence of a general increase in the electoral value of incumbency. I turn now to the more extended examination of the reelection patterns of 11 (1952-72) of the 13 cohorts obtained under the group time-series. The results presented in Table 4 show that, as expected, the percentage of marginal open-seat winners who continued to be reelected in the marginal range has shown an overall decline, with the exception of those first elected in 1966. Indeed, after 1966 this pattern of reelection died out; after 1966, all marginal open-seat winners ran at least one safe race as incumbents. This trend reflects the increase in electoral safety observed at the aggregate level in previous studies.

It can be inferred, in addition, that most House members from marginal open seats derived a cumulative electoral advantage from incumbency. In all but three classes of marginal open-seat winners (1962, 1966, and 1970), the percentage of House members with single-peaked electoral patterns was greater than the percentage with invariant or variable electoral patterns. In other words, the modal tendency in each class of marginal

Table 4. Percentage for Patterns of Reelection Margins for Open-Seat Winners in the U.S. House of Representatives, 1952-72

	Marginal			Safe	
Class	Invariant	Variable	Single-Peaked	Variable	Single-Peaked
1952	20.0 (6)	10.0 (3)	36.7 (11)	35.7 (5)	64.3 (9)
1954	12.5 (1)	25.0 (2)	50.0 (4)	22.2 (2)	66.7 (6)
1956	7.7 (1)	15.4 (2)	23.1 (3)	33.3 (3)	33.3 (3)
1958	9.1 (2)	27.3 (6)	36.4 (8)	25.0 (2)	75.0 (6)
1960	10.5 (2)	10.5 (2)	68.4 (13)	75.0 (6)	25.0 (2)
1962	7.7 (2)	34.6 (9)	34.6 (9)	53.8 (7)	30.8 (4)
1964	10.0 (2)	5.0 (1)	45.0 (9)	28.6 (4)	57.1 (8)
1966	22.2 (2)	44.4 (4)	11.1 (1)	26.7 (4)	73.3 (11)
1968	0.0 (0)	6.3 (1)	62.5 (10)	9.1 (1)	81.8 (9)
1970	0.0 (0)	47.1 (8)	29.4 (5)	16.7 (1)	83.3 (5)
1972	0.0 (0)	8.7 (2)	65.2 (15)	23.1 (3)	69.2 (9)

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-78).

Key: All percentages for marginal and safe open-seat winners were calculated against the number of marginal and safe open-seat winners, respectively, in each class who participated in up to three succeeding general elections. The column headings represent aggregations of the following reelection patterns, where "M" and "S" signify a marginal and safe victory margin, respectively.

Percentages for marginal open-seat winners:

Invariant = MMMM;
Variable = MMSM + MSMM + MSMS + MSSM;
Single-peaked = MMMS + MMSS + MSSS.

Percentages for safe open-seat winners:

Single-peaked = SSSS;
Variable = SMMM + SMMS + SMSM + SMSS + SSMM + SSMS + SSSM.

open-seat winners (other than the aforementioned three) has been non-decreasing change in electoral margins once representatives became incumbents.

Among the 1962, 1966, and 1970 classes of marginal open-seat winners, the majority of each class was split between the single-peaked and variable patterns of reelection. Thus across the entire period when marginal open-seat winners did not demonstrate non-decreasing change in their electoral margins, they were still more likely to be reelected by a safe percentage of the vote at least once than they were to continue to be reelected by marginal percentages.

But surprisingly, we find that the percentage of marginal open-seat winners with single-peaked patterns of reelection has shown *no general increase*. This is contrary to what was expected given the hypothesized increase in the electoral value of incumbency. The highest percentages of marginal open-seat winners with single-peaked patterns of reelection may be found in the 1960 (68.4 percent), 1968 (62.5 percent), and 1972 (65.2 percent) classes. As a rule, the incidence of single-peakedness among marginal open-seat winners has remained highly erratic over the entire period.

In contrast, safe open-seat winners have shown an increasing tendency toward single-peaked patterns of reelection. The percentage of safe open-seat winners who sustained their safe margins as incumbents steadily increased after 1958, from a low of 25 percent in the 1960 class to a high of 83.3 percent in the 1970 class. Indeed, the four most recent cohorts displayed four of the five highest values of single-peakedness among safe open-seat winners.

Hence, the linkage between incumbency and electoral safety is quite different for marginal and safe open-seat winners. Safe open-seat winners increasingly sustained their safe margins as incumbents. The relationship between incumbency and electoral safety among marginal open-seat winners, however, remains problematic.

To examine the relationship between electoral safety and incumbency for marginal open-seat winners in more detail, Table 5 provides a breakdown of the three patterns of reelection termed single-peaked. Within this subset of marginal open-seat winners, after 1962 the modal tendency has been for members to acquire and sustain electoral safety as incumbents. The 1964 and 1968 classes contained the highest percentage of marginal open-seat winners who displayed this pattern of reelection, which provides the strongest evidence for the hypothesized relationship between incumbency and electoral safety. The percentages of members who continued to be reelected by marginal percentages until their third reelection steadily declined from 1952 to 1966, with the exception of 1960. After 1966 there was an increase in this pattern of reelection though the percentages remained low. In contrast, the percentages of marginal open-seat winners who ran safe races in their second and third reelections show no general trend over time; however, after 1962 only in two classes (1968 and 1972) did any marginal open-seat winners display this pattern of reelection.

The reader will note that in many cases the percentages in Table 5 reflect a very small number of representatives. Therein lies the central point of analysis in this section. The expectation is that, given the hypothesized effect that incumbency began to have on electoral margins, a great many *more* members from marginal open seats would have fallen into these patterns during and after the mid-1960s. No such trend, however, occurred. In other words, the small *N* observed for many of the cells belies the incumbency thesis.

The data presented on the reelection patterns of marginal open-seat winners in Tables 4 and 5 yield three conclusions. First, since 1964 the percentage of marginal open-seat winners who have won at least one reelection by a safe percentage of the vote has increased. Second, a greater percentage of members have immediately and, as measured here, permanently, converted their marginal open seats into safe incumbent seats. But, third and most important, *sustained electoral safety has not been the norm*. There has been no general increase in the percentage of marginal open-seat winners whose electoral margins reflected non-decreasing change over time. Moreover, fewer than half of the marginal open-seat winners over the entire period won by safe margins as incumbents and maintained those safe margins in subsequent elections, the peak percentages being in the 1964 (45 percent) and the 1968 (43.8 percent) classes. Consequently, it may be inferred that the linkage between incumbency and electoral safety is highly variable and, as a rule, tenuous indeed.

Overall, the results of the pre-test/post-test and the group time-series allow somewhat different inferences. The pre-test/post-test shows a moderate increase in the number of open-seat winners who demonstrated the marginal-to-safe pattern, which implies an increase in the electoral value attached to incumbency. In contrast, the group time-series suggests that the increase in safe margins observed for incumbents cannot be directly attributed to the incumbency factor.

Incumbency, Electoral Safety, and Turnover

The pre-test/post-test again provides a preliminary view of the relationships among incumbency, electoral safety, and turnover. Table 6 reports the percentages of each class of open-seat winners

Table 5. Percentage of Marginal Open-Seat Winners with Single-Peaked Patterns of Reelection in the U.S. House of Representatives, 1952-72

Class	MMMS		MMSS		MSSS	
1952	16.7	(5)	10.0	(3)	10.0	(3)
1954	12.5	(1)	12.5	(1)	25.0	(2)
1956	7.7	(1)	7.7	(1)	7.7	(1)
1958	0.0	(0)	18.2	(4)	18.2	(4)
1960	26.3	(5)	10.5	(2)	31.6	(6)
1962	3.8	(1)	23.1	(6)	7.7	(2)
1964	0.0	(0)	0.0	(0)	45.0	(9)
1966	0.0	(0)	0.0	(0)	11.1	(1)
1968	6.3	(1)	12.5	(2)	43.8	(7)
1970	5.9	(1)	0.0	(0)	23.5	(4)
1972	21.7	(5)	8.7	(2)	34.8	(8)

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-78).

Key: The percentages were calculated against the number of marginal open-seat winners in each class who participated in up to three succeeding general elections. "M" and "S" signify marginal and safe victory margins, respectively.

who were reelected in the subsequent election. As was discussed earlier, the expectation is that safe open-seat winners should have displayed a higher rate of reelection than marginal open-seat winners. The disparity between the reelection rates of marginal and safe open-seat winners, however, should have begun to decline in the mid-1960s due to the increased incumbency effect.

A comparison of the reelection rates of marginal and safe open-seat winners in each cohort indicates, as was expected, that throughout the period marginal open-seat winners were less likely than safe open-seat winners to be reelected. Only in the 1954 and 1962 classes were a higher percentage of marginal open-seat winners reelected.

Paralleling the increase in electoral safety observed in the last section for first-term incumbents from marginal open seats, the rate of reelection for marginal open-seat winners tended to increase after 1964, reflected by the difference between the mean percentages for the 1952-62 period and for the 1964-76 period. The difference, however, between the mean rates of reelection among marginal open-seat winners for the two periods is minimal: 5.4 percent. This is primarily due to the inclusion of 1964 marginal open-seat winners in the calculation of the mean percentage for the second period. But, in contrast to previous studies, here we may locate the breakpoint in the 1966-68 interim. Marginal open-seat winners elected after 1964 showed the higher success rate.

If we exclude the 1964 class, the mean reelection rate for the second period climbs to 91.9 percent and the difference between the average reelection rates for the two periods in turn increases to 8.4 percent. This adjustment strengthens the predicted relationship between the increase in electoral safety observed in the last section and the decline in electoral turnover. As was expected, after 1966 the reelection record of the marginal open-seat winner increasingly approached that of his or her

Table 6. Percentage of Marginal and Safe Open-Seat Winners in the U.S. House of Representatives (1952-76) Reelected in the Subsequent Election

Class	Marginal		Safe	
1952	82.4	(28)	100.0	(14)
1954	90.9	(9)	88.9	(8)
1956	64.3	(9)	100.0	(12)
1958	84.0	(21)	100.0	(11)
1960	90.5	(19)	100.0	(8)
1962	89.7	(26)	84.6	(11)
Mean	83.5	(18.9)	95.6	(10.7)
1964	70.8	(17)	92.9	(13)
1966	100.0	(14)	100.0	(17)
1968	93.8	(15)	100.0	(11)
1970	92.0	(23)	87.5	(7)
1972	80.0	(24)	94.7	(18)
1974	93.8	(30)	100.0	(15)
1976	92.0	(23)	100.0	(22)
Mean	88.9	(20.9)	96.4	(14.7)

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-78).

Key: The percentages for marginal and safe open-seat winners were calculated against the total number of marginal and safe open-seat winners, respectively, in each class who participated in the next general election.

safe counterpart. Thus the results of the pre-test/post-test generally support the hypothesized relationships among incumbency, electoral safety, and turnover.

The question now is: does the correspondence between electoral safety and turnover hold when we examine additional elections under the group time-series? As was reported for the group time-series in the last section, the reelection patterns of open-seat winners have mirrored the general increase in electoral safety among incumbents observed in the aggregate. Safe open-seat winners were increasingly more likely to maintain their safe margins in subsequent reelections, and marginal open-seat winners were increasingly more likely to run at least one safe race as incumbents. Thus there is ample reason to suspect that the rate of cohort turnover should have declined as hypothesized.

Table 7 reports the percentage of open-seat winners who were reelected for at least three consecutive terms. Again, there is a disparity in the reelection rates of marginal and safe open-seat winners. As expected, safe open-seat winners were consistently more likely to be reelected than were their marginal counterparts.

Contrary to expectations, however, there has been no general increase in the percentage of safe open-seat winners who were reelected. Those who entered Congress in the second period (1964-72) showed a slight edge in the probability of reelection, reflected by the difference in the mean rates for the two periods. But, as was discussed earlier, the percentage of safe open-seat winners who sustained their safe margin increased over the period 1960-70. The data in Table 7 imply that there is little relationship between that increase and electoral turnover. Among safe open-seat winners, the increase in electoral safety has not resulted in a decline in electoral turnover.

Table 7. Percentage of Marginal and Safe Open-Seat Winners in the U.S. House of Representatives (1952-72) Reelected for at Least Three Consecutive Terms

Class	Marginal		Safe	
1952	66.7	(20)	100.0	(14)
1954	87.5	(7)	88.9	(8)
1956	46.2	(6)	66.7	(6)
1958	72.7	(16)	100.0	(8)
1960	89.5	(17)	100.0	(8)
1962	76.9	(20)	84.6	(11)
Mean	73.3	(14.3)	90.0	(9.2)
1964	60.0	(12)	85.7	(12)
1966	77.8	(7)	100.0	(15)
1968	68.8	(11)	90.9	(10)
1970	76.5	(13)	100.0	(6)
1972	73.9	(17)	92.3	(12)
Mean	71.4	(12)	93.8	(11)

Source: Based on data from Congressional Quarterly's *Guide to U.S. Elections* (1952-74) and *Congressional Quarterly Weekly Report* (1976-78).

Key: The percentages for marginal and safe open-seat winners were calculated against the number of marginal and safe open-seat winners, respectively, in each class who participated in up to three of the succeeding general elections.

Nor has it among marginal open-seat winners. Despite an increase in the percentage of marginal open-seat winners who as incumbents won at least one election by a safe margin, turnover did not decline. The peak reelection rates may be located, moreover, in the 1954 and 1960 classes, both of which preceded the general increase in the number of safe margins witnessed among members from marginal open seats. Oddly enough, a comparison of the mean reelection rates across classes in the 1952-62 period and in the 1964-72 period shows that the rate of reelection during the second period has declined somewhat. Thus, on the average, marginal open-seat winners who entered the House in the second period (1964-72) were slightly less likely to be reelected than earlier classes.

Overall, and in contrast to the results obtained under the pre-test/post-test, these findings indicate that there was *no substantial difference in the reelection rates of either marginal or safe open-seat winners in the latter period.* This evidence is clearly contrary to expectations and inconsistent with the hypothesized relationship between electoral safety and electoral turnover.

Conclusions

Though the data considered here do reflect the general increase in electoral safety among incumbents, the findings are largely inconsistent with the hypothesized relationships between incumbency and electoral safety and, in turn, between electoral safety and electoral turnover. The overall results suggest, first of all, that incumbency far from assures a safe victory margin. To be sure, after 1964 more marginal open seats did go safe, but a great many did not. Furthermore, the percentage of marginal open-seat winners who sustained electoral safety as incumbents showed no general increase. Also, contrary to Born's recent conclusions (1979), these results imply that the increase in victory margins has *not* been systematically distributed across post-1964 freshman cohorts. In most cases, the marginal-to-safe pattern has been largely limited to members of one party and it is likely that the increase in margins that has been documented with previous surge data has been subject to an outlier bias. In addition, change in the relationship between incumbency and electoral safety has differed among marginal and safe open-seat winners. Safe open-seat winners now have considerably less trouble maintaining safe margins than marginal open-seat winners have in acquiring them. Thus both the magnitude and the uniformity of the increased incumbency effect may be questioned.

Second and most important, the increase in the number of safe margins observed in the reelection patterns of both marginal and safe open-seat winners has not translated into a decline in electoral turnover. Despite an increase in electoral safety, open-seat winners appear no more insulated as incumbents now than they were 20 years ago. The electoral edge hypothetically provided by increased victory margins simply has not materialized.

Parenthetically, the results on electoral safety and turnover imply that the marginal/safe dichotomy may be somewhat artificially conceived. Certainly, from the member's perspective, "last time's" election is but one of a number of factors expected to affect his or her electoral future (Fenno, 1978, pp. 10-18). And, given the reelection patterns examined here, one might question whether it is appropriate to consider the 1950s representatives with four consecutive marginal victories less "safe" than their 1970s counterparts who have acquired and maintained safe margins.

The variation in the relationship shown here between incumbency and safe margins suggests that incumbents have not been across-the-board beneficiaries of a behavioral change among voters. Neither does it imply that representatives first elected after 1964 have systematically differed from those elected before 1964. At any rate, if voters have acquiesced to the incumbency cue, or if members have worked harder at district casework, pork, or communication, it has meant little in terms of increased electability.

The electoral records reported here imply that there may be more dissimilarities among incumbents than similarities. Hence the results fit most consistently in that body of research emphasizing the importance of district or contextual factors (Jacobson, 1980, 1978; Hinckley, 1980; Mann and Wolfinger, 1980; Abramowitz, 1980; Payne, 1979; Mann, 1978). The pertinent questions become: what are the characteristics of the individuals who can sustain a high victory margin, the dynamics of their campaigns, the makeup of their districts? Since the safe open-seat winner is now the only House member who can be fairly sure of continued safe margins as an incumbent, the most powerful value of incumbency may well be that of discouraging able opposition at the outset. Seen in this light and in view of increasing retirements, the results of the open election may prove especially important in shaping the electoral profile of future district races.

Ultimately, this study warns of the traps in examining these relationships in too limited a time frame. Extended observation performed in the group time-series had especially significant implications for the inferences drawn regarding the relationship between electoral safety and electoral turnover. Using the more limited pre-test/post-test, we observed an inverse relationship between electoral safety and electoral turnover. This relationship, however, washed out, as demonstrated in the findings for the group time-series. Similarly, the relationship between incumbency and electoral safety appeared stronger in the pre-test/post-test than was borne out by the group time-series. Since election statistics easily permit extended examination of individual electoral records, it may prove useful to incorporate this approach into future designs.

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Some Social and Political Dimensions of Nuclear Power: Examples from Three Mile Island

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Public concerns about the social and political implications of energy choices have found expression in the nuclear debate. The Three Mile Island accident, in its complexity and far-reaching consequences, served to focus criticism of nuclear power that had been building up for a decade. It revealed with striking clarity that technology is a social concept to be understood in terms of social and political relationships if it is to be brought under public control. The ramifications of the accident revealed the conceptual difficulties of assessing social risks and the political difficulties in managing them. This article draws examples from the Three Mile Island accident to review several characteristics of nuclear technology, its scale and costs, its complexity, its uncertain and unpredictable physical effects, and its indirect risks. It then explores the implications for social, political and administrative institutions as they grope for ways to manage the risks of nuclear power in the context of critical public scrutiny. Finally it identifies a range of issues calling for policy research.

Energy choices have critical social and political implications. The scale, the cost, the complexity, and the nature of the risks associated with various energy sources entangle such choices in a web of political, economic and organizational problems. Decisions at every level bear on the shape of industrial and urban development, the organization of major institutions, the forms of authority, and the distribution of risks (Noble, 1977; Winner, 1980).

Public concern about the broad-reaching social and political implications of energy choices has found expression in the nuclear power debate. The persistence of opposition to nuclear power in part reflects fear about the risks of this technology and its "apocalyptic image of extinction" (Lifton, 1976, 1979), a fear that is not susceptible to measurement by comparing risks and benefits. But nuclear power also symbolizes the dilemmas of many large-scale advanced technologies, and controversy has focused on the social properties of nuclear power as well as its immediate risks.

The Three Mile Island accident of March 1979, in its extraordinary complexity and far-reaching consequences, has served to focus the criticism of nuclear power that had been building up for a decade. The ramifications of this event have shown that the concept of risk, often defined in the rather narrow framework of health and safety, must be considered in far broader terms. They reveal both the conceptual difficulties in assessing social risks and the political difficulties in managing them.

Drawing examples from the Three Mile Island accident and its aftermath, this article briefly reviews several characteristics of nuclear technology —its scale and costs, its complexity, its uncertain and unpredictable physical effects, and indirect risks. It then explores some of the implications for major social, political and administrative institutions as they grope for ways to manage the risks of this cumbersome technology in the context of increasingly critical public scrutiny. The intention is to identify a range of issues suitable for and, indeed, calling out for policy research.

Characteristics of Nuclear Power

Scale. The siting of a nuclear power plant presents special problems simply because of its scale and its staggering capital costs. In 1969, nuclear plants, projected for completion in 9 years, were expected to cost about $226 per kilowatt hour. In 1978, with an anticipated 12-year construction period, estimates increased to $1,648 per kilowatt hour.

Like any large-scale project, a power plant dominates its environment, profoundly affecting the tax structure, real estate values, and economy, as well as the physical environment of a community. Effects may be positive or negative: real estate values may rise if the installation attracts industry and new people to the area, but also they may decline if the facility is viewed as undesirable. A local community may disproportionately benefit from the construction of a power plant through a more favorable tax base or increased employment. Or it may lose out because of health hazards or environmental risks. The labor necessary to build a power plant creates new markets and the need for new services. The very scale of such a structure may thus affect the distribution of social and economic privilege (Shields, 1979).

When a project fails, such impacts become obvious. The Three Mile Island accident had an

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Research for this paper was supported by a grant from the National Science Foundation EVIST Program.

enormous effect on the local economy of the Harrisburg, Pennsylvania region. Local estimates of wages lost as a result of the accident ranged from $5.7 million to $8.2 million in manufacturing, and from $2.8 million to $3.8 million in the nonmanufacturing sector. An estimated $6 million to $8 million were lost in revenues partly due to the decline in tourism. Losses in the agricultural sector were about $1 million. As some 34,000 people left the area, 256,000 person-days were lost. Of these, 15,000 lost pay as well (Flynn, 1979).

Orders for new nuclear plants had been declining for several years and the accident provoked a rash of further cancellations. Sixteen reactor orders in other parts of the country were withdrawn. Utilities in Ohio and Pennsylvania scrapped their plans for 4 nuclear plants despite an existing investment of $250 million.

Large-scale technological projects raise major questions of social equity. Given very long lead times, who should pay for construction costs? Are consumers today to pay for installations that will produce electricity for residents 15 years hence? The Three Mile Island accident also raised the issue of consumer liability in the case of accident. According to the President's Commission on the Accident at Three Mile Island (the Kemeny Commission), the cleanup, including emergency management and replacement of power, will cost from $1 billion to $2 billion (President's Commission, 1979). Some estimates, including the cost of building a new nuclear plant on the site, were as high as $3 billion (Technical Assessment Task Force, 1979). General Public Utilities Corporation (GPU), the holding company owning the Three Mile Island reactors, is deeply in debt and faces possible bankruptcy. Between January 1979 and January 1980, its net income dropped 51 percent and its liabilities increased 44 percent. It tried to pass costs on to consumers and won some emergency rate increases, but consumer pressure successfully limited this possibility. The increases as of March 1980 provided $150 million, and disputes continue over whether consumers or stockholders should foot the bill. Because these utilities employ 11,000 workers and control the supply of electricity in Pennsylvania and New Jersey (GPU owns Metropolitan Edison, Pennsylvania Electric Company and Jersey Central Power and Light Company), a bankruptcy could have disastrous consequences.

High capital investment encourages industrial commitment to existing technological systems, and limits the flexibility that may be necessary to adapt to safety requirements. The manufacturer of the Three Mile Island reactor, Babcock and Wilcox, had been warned of equipment problems. The pilot operator relief valve had failed at least 9 times prior to the accident. An earlier accident at a reactor in Toledo, Ohio had revealed several design problems, but the company failed to modify its technology to accommodate them. The nuclear industry is perpetually faced with accusations of falsifying records and disregarding regulations.[1] Given the high costs of construction and repair, financial penalties may be easier to absorb than the cost of major structural modifications in a nuclear facility. Indeed, the nuclear industry and its investors argue that required safety modifications have increased the cost of nuclear power to the point where new plants are financially "unattractive" to investors (Hebert, 1980).

Complexity. The complexity of a nuclear power plant, both in technological and organizational terms, and the interdependence of organizational and technical systems, contributed to the awesome difficulty of coping with the Three Mile Island accident.[2] The sophisticated organizational requirements of a nuclear power installation—the skills required to operate it, the organization of a managerial infrastructure, and the establishment of means of control—pose staggering problems of administration beyond the expertise and managerial capacity of most utilities. In this sense, the problems of Metropolitan Edison (the subsidiary of GPU licensed to manage the Three Mile Island reactor) were hardly surprising. The accident demonstrated the convoluted interrelationships between technical and human factors in the operation of technological systems and how this relationship reduces the predictability of risks and increases vulnerability. It suggested the difficulty of anticipating complications that include both operating errors and equipment failures, and of controlling breakdowns that ramify, often unpredictably, throughout a system.

During normal operations a reactor is run with the assistance of a computerized instrument and control system. Computerization is necessary to make rapid adjustments to changes in the reactor's equilibrium. The Babcock and Wilcox reactor model is especially dependent on the use of an automatic computer system because its sensitive design requires split-second reactions to small changes. The sequence of events during a minor accident at the Babcock and Wilcox Crystal River plant in Florida in February, 1980 suggests some of the dilemmas resulting from such dependence. The accident began when the computer misread a short circuit to indicate a cooling of the reactor. It responded by withdrawing the control rods and reducing the flow of coolant water. This overheated the reactor, driving the pressure up and then automatically shutting the reactor down. The pressure relief valve opened and the Emergency Core Cooling System pumped water into the reactor, eventually spilling 43,000 gallons of radioactive water on the floor. The reactor essentially "took off and did its own thing" (Marshall, 1980).

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[1] In March 1980, Commonwealth Edison Company, the country's largest utility, was indicted on charges of conspiracy and making false statements about breaches of security.

[2] The problems of managing large-scale technological enterprises have been discussed in the literature, especially in papers about innovation. See Stanfield (1976) and Horwitch (1979) for literature reviews.

At the Three Mile Island reactor, the control room had been designed so as to be useful during normal operations but not necessarily in a crisis. One hundred alarms went off in the first few minutes of the crisis. There was no way for operators to sort out the signals. When reliance on the technical systems failed, the operators were inadequately prepared to deal with the emergency. Some blame the limitations of training which focuses principally on normal operating requirements. Others wonder whether the demands on an operator during a crisis are within the range of human capability. It has been argued that accidents are, in fact, "normal," that is, intrinsic to complex, interdependent systems. To the extent that this argument holds, it warrants special concern, given the dangers that could result from a nuclear accident (LaPorte, 1980; Perrow, 1980).

Uncertainty and Unpredictability. Efforts to estimate the risks of an accident, or the severity of health hazards should one occur, are beset by technical uncertainties. The deluge of major studies on risk have only accelerated the technical debate. The Rasmussen Report (1974), commissioned at a cost of $3 million by the Atomic Energy Commission, argued that nuclear power presented only minimal risks as compared to other technologies. The report concluded that a major reactor accident was so unlikely that the overall risk was very small (1 percent per year for 100 operating reactors) even in a full-scale nuclear economy. Moreover, it claimed, most accidents would have minimal consequences for life and property. The Rasmussen report, however, faced a flood of criticism. The American Physical Society took issue with its estimation of the possible consequences of an accident. The Union of Concerned Scientists (1977) refuted its methodology and the very assumptions underlying the report. Fundamental disagreement among scientists persists, exacerbated by the potentially catastrophic nature of nuclear risk (Fischoff, 1979).

Similarly, efforts to predict the long-term effect of low-level radiation are obstructed by disagreement about the level at which radiation presents significant risks, and by inherent difficulties in sorting out its effects from other environmental insults. Here disputes among scientists have complicated the establishment of radiation standards for occupational safety.

The reaction of experts from federal and state agencies, industry, and anti-nuclear groups to the Three Mile Island accident illustrated the uncertainties about the risks of nuclear power and revealed how such uncertainty can be manipulated to serve particular ends (Nelkin, 1980). The early estimates of radiation released by the accident varied widely. On March 29, immediately following the accident, the utility detected 7 millirems of radiation, but the NRC estimated around 20 millirems, claiming that a person near the plant could have received as much as 100 millirems. In late April, the NRC reported that the radiation levels within a 5-mile radius were near normal, but on the same day a report from the airport three miles away reported radiation levels 10 times normal. A chemist from the anti-nuclear movement made an independent inquiry and found on April 11, that radiation was 5 times the normal level at a site 20 miles from the plant, and on April 16, it was 50 times normal at a site 30 miles from the plant.

Consistent measurement is important as a basis for estimating the seriousness of exposure to radiation. The variation in these reports allowed a selection of data that could support any interpretation. The very language used to describe the accident revealed the very diverse perceptions that enter such interpretations. Was it an accident or an incident? A catastrophe or a mishap? A disaster or an event? A technical failure or a simple mechanical breakdown? Utility officials talked of "miniscule" hazards, comparing the dosage to dental X-rays. Critics talked of cancer and of "the grim reality of death." Harold Denton, the chief of operations for the Nuclear Regulatory Commission (NRC), reassured the public that the radiation emission was "slight" and that sampling of milk, food, and water showed "negligible contamination." There was "no cause for alarm." At the same time, he urged pregnant women and children to evacuate the area and saw to it that potassium iodide, an antidote that blocks the accumulation of radioactive iodide in the thyroid, was on site.

Evaluation inevitably turned to a body count. In May, HEW Secretary Joseph Califano asserted that 1 to 10 additional cancer deaths (depending on controversial assumptions about the effect of low-level radiation) could be expected among the people living within 50 miles of the accident. An inter-agency task group estimated that the potential lifetime impact of the accident would be less than 2 fatal cancers and 2 non-fatal cancers among those living within 50 miles of the site.

Others estimated 20 additional cases of cancer, about half of them likely to be fatal, and one doctor predicted 50 additional deaths. Eventually, the task force studying radiation health for the Kemeny Commission concluded that the accident had a negligible health impact (President's Commission, 1979). However, locally in the Harrisburg area, where several long-term health studies are under way, debates continue over statistics. While most studies deny significant health effects, some claim increased infant morality and hyperthyroidism, and a dramatic array of health problems among domestic animals. The major finding of the Kemeny Commission, however, was the important psychological stress on the local population.

Indirect Risks. The Three Mile Island accident posed a number of unexpected social and psychological problems that must be included in an extended definition of risk. A major problem emerged following the official advice to evacuate pre-school children and pregnant women living within a 5-mile radius of the power plant. While this was only an evacuation "advisory" (that is, not an order) about 60 percent of those people living within 5 miles, 40 percent of those in the 5- to 10-mile ring around the plant, and 32 percent of those in the 10- to 15-mile ring left the area on a voluntary basis. People began to withdraw funds from the banks, overloaded the telephone lines, and pulled their children out of school (Henderson, 1980).[3]

Little thought had been given to the implications of such an action. The governor of Pennsylvania, Richard Thornburgh, expressed his concern about its possible effects:

> The movement of elderly persons, people in intensive care units, babies in incubators, the simple traffic on the highways that results from even the best of an orderly evacuation, are going to exert a toll in lives and injuries. Moreover, this type of evacuation had never been carried out before on the face of this earth, and it is an evacuation that was quite different in kind and quality than one undertaken in time of flood or hurricane or tornado. . . . When you talk about evacuating people within a 5-mile radius of the site of a nuclear reactor, you must recognize that that will have 10-mile consequences, 20-mile consequences, 100-mile consequences (Thornburgh, 1979, p. 21).

Studies of the behavioral effects of the accident found the major short-term health impact to be psychological stress and demoralization among those living near the plant and faced with continued uncertainty about their fate (Dohrenwend, 1980). Those who actually worked at Three Mile Island were the most demoralized and mistrustful people in the area, worried both about their health and about losing their jobs should the reactor close (Chisolm, 1980; Logan and Nelkin, 1980). But even in the larger community, psychiatrists reported severe demoralization and mental health problems.[4] These problems faded with time, but anger and distrust remained, exacerbated by the uncertainties over the logistics of the cleanup operation. Civic leaders have warned of violence if the utility were to vent Krypton gas from the crippled reactor—violence from a generally law-abiding and conservative population that prior to the accident had few negative feelings about nuclear power at all. The mayor of Middletown, Pennsylvania jokingly proposed that the banks add a radiation count to their displayed time and temperature clocks, but then observed, "I'm quite sure they'll never tell us the truth about the venting" (Reid, 1980). A telephone survey of area residents found that most people believed they were given misleading information by the utility and the NRC. Meanwhile, a local pro-nuclear group has formed to protest the public image that the town is in a state of hysteria (Flynn, 1980). The issue has thus polarized the communities near Three Mile Island with profound effects on social relationships.

Other unexpected risks were associated with this accident. The general difficulty of keeping track of plutonium is a problem of increasing concern in the regulation of nuclear power. Safeguards, claimed investigators from the General Accounting Office (GAO), are inadequate. Neither existing accounting systems nor physical security arrangements can prevent sabotage or the diversion of fissionable materials (GAO, 1980). These problems are exacerbated in the case of accidents. The cleanup of Three Mile Island requires that about 1500 persons, mainly contractors, have unescorted access to the site, precluding effective precautions against sabotage from within.

These social and political characteristics of nuclear power technology can serve as a framework for analyzing a variety of policy questions concerning this and other large-scale technological systems. For example, what are the social and economic impacts of nuclear power plants on their local communities? How are the costs and benefits of this technology distributed? As the industry modifies reactors, at what point does the cost of this technology become impractical? And if the risk of a penalty is more practical than compliance with costly regulations, what are the implications for safety?

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[3] According to a calculation by the Natural Resources Defense Council (1980), if an identical accident had occurred at Indian Point, New York and evoked a parallel public response in this heavily populated area, one million people would have been evacuated.

[4] This was one of the conclusions of the Kemeny Commission. The State of Pennsylvania commissioned a psychological impact study run by Dr. Evelyn Bromet of the University of Pittsburgh.

We need to know more about the relationship between technical and human factors in the operation of large-scale technological systems. How do these relationships bear on their vulnerability to accidents? If in fact accidents are normal in complex systems, is the further complexity added by new safety requirements counterproductive? The proliferation of disputes among scientists concerning the risks of nuclear power suggests another set of research questions. What is the effect of the increasing polarization of scientists on the decision-making process?

Considerable data have been collected on public attitudes towards nuclear power and on the changes in attitudes as a consequence of accidents. Relatively little work has focused on the occupational and social class correlates of such attitudes. What is the effect of confusion and uncertainty on public views toward nuclear power? What in fact is the source of persistent and often profound skepticism about this technology? Is it simply the fear of risk, or as some would claim, the apocalyptic image that links this technology to nuclear war (Lifton, 1979)? How does opposition link with political ideologies? (Nelkin and Pollak, 1980). Finally, the characteristics of nuclear power have had profound implications for a number of social, political and administrative institutions. Here too the Three Mile Island accident has suggested an array of vital issues amenable to policy analysis.

Institutional Implications

The Three Mile Island accident occurred at a time of intense popular opposition to nuclear power. It substantially contributed to the growing problems of credibility faced by the nuclear establishment—that social network comprised of industry, public officials, administrators and scientists concerned with the implementation of nuclear policy. In the country at large, the accident hardened existing attitudes towards nuclear power, and caused some increase in opposition, as well as considerable distrust in the responsible authorities. These trends continued even a year after the event. A series of polls indicated that the percentage of people perceiving nuclear power as "very safe" was 26 percent in October 1978, 21 percent in April 1979 (right after the accident) and 15 percent in February 1980. The percentage of people judging the technology "not so safe" or "dangerous" increased from 28 to 30 to 38 percent in the same set of polls. By February 1980, polls indicated that 20 percent would like the nuclear program to stop, 47 percent would allow operating plants to continue but wanted no further growth and only 22 percent wanted further expansion (Mitchell, 1980). In this context of growing mistrust about nuclear power, the accident affected the role and responsibility of a number of institutions. The nuclear industry, public officials, the regulatory system, the courts, the scientific community and the media were all involved.

The Nuclear Industry. For the nuclear industry, the future of a $100 billion investment at stake, the Three Mile Island accident presented a serious problem of public image. Thus the industry proceeded with a massive effort to assure the public that nuclear power is a safe and controllable technology. Industry representatives talked of their "energy awareness programs" and their "truth squads" (Starr, 1980). The Electric Power Research Institute (EPRI), a consortium of utilities, set up a Nuclear Safety Analysis Center to study the Three Mile Island acident and to persuade the public that nuclear power is safe and necessary. Its estimated annual budget is $8 million. The industry also set up the Institute for Nuclear Power Operations to improve reactor operating standards and to monitor utilities to assure compliance to these standards. With 200 professionals, its budget is about $11 million.

The accident stimulated a number of proposals to restructure the industry. These ranged from a single national nuclear utility, to a set of nuclear corporations—all designed to reduce the fragmentation that exists at present. Alvin Weinberg has proposed a "moratorium" on new sites through a program that would cluster nuclear plants in designated areas and restrict their construction elsewhere (Weinberg, 1980). Proposals on ways to reduce dissent have flourished: a blackout on news within a 50-mile radius of accidents, restrictions on citizen intervention in licensing hearings, preemption of local control over transport and siting, and exemption of certain Nuclear Regulatory Commission decisions from the requirements of the Freedom of Information Act.

Public Officials. The Three Mile Island accident also presented a problem of credibility for public officials. Convinced of the need to expand the nuclear program and to speed up the licensing procedures, President Carter was directly concerned with easing public skepticism about nuclear policy by demonstrating that the executive was "doing something" about risk. The problem for Carter was less to evaluate the future of nuclear policy than to assure its public acceptability. He appointed the Kemeny Commission to this end.

To help establish its own credibility, the members of this commission were selected to represent very diverse interests. Its chairman was John Kemeny, president of Dartmouth and a mathematician. Its members included a biochemist, a nuclear engineer, a physicist, a governor, an ex-presidential aide, an industrialist, a trade unionist, a sociologist, a professor of journalism, an environmentalist, and a housewife active in civic affairs. Kemeny explained this diversity in terms of the need for credibility:

> I think it would be wrong for the Commission to consist of a bunch of engineers. We are sort of a national jury on this issue and would you want the jury to consist of experts? I think it would be a mistake; you want the average American to believe what this Commission says (Kemeny, 1979).

The Kemeny Commission investigation was only one of six major inquiries that were initiated as public officials at various governmental levels faced the implications of the accident. The NRC, the Senate Subcommittee on Nuclear Regulation, and the Energy and Environmental Subcommittees of the House Interior Committee each formed special inquiry groups. In addition, the governor of Pennsylvania formed a special commission and the utilities organized a Nuclear Safety Analysis Center. Beyond these major investigations, more specialized studies were sponsored by the Department of Health, Education and Welfare, the Environmental Protection Agency, the Center for Disease Control, the Food and Drug Administration, the National Cancer Institute, and a variety of local and state government agencies. The confusion among government agencies with overlapping spheres of influence was apparent as all these groups sought to deal with the emergency and later to establish blame. The organization of the regulatory system was clearly a major problem.

The Regulatory System. The scale and complexity of a technological facility is matched by the size and complexity of the regulatory apparatus required to control it. Technologies of scale can blur the distinction between public and private interests. Given the size of the nuclear enterprise, government has become a partner in the nuclear field, developing through its contracts and subsidies a stake in the promotion of nuclear power. This partnership reduces the ability of government to exercise independent regulation and control. Regulatory agencies seeking to function in such a context are often attacked for their promotional philosophy, and the agencies responsible for regulating nuclear power are no exception. The 1974 amendments to the Atomic Energy Act reorganized the administrative structure responsible for regulation, with the explicit purpose of separating the functions of promotion and control. This legislation abolished the Atomic Energy Commission (AEC) and set up a separate Nuclear Regulatory Commission. However, much of the existing staff, with their industrial alliances, remained. A 1976 study of the NRC staff found that 65 percent of its 429 senior members had been employed in private energy enterprises holding NRC licenses, permits or contracts, and 73 percent of NRC consultants maintained simultaneous relationships with the agency and industries with NRC licenses or contracts (Kneier, 1976). The NRC, claimed this study, is tied to the interests of the industry to the extent that it cannot effectively guide a cautious and safe development of nuclear technology.

According to a study by the General Accounting Office (1980) as well as an analysis commissioned by the NRC itself (Rogovin, 1980), the agency has changed more in its title than in its essence. It has retained the AEC mentality, just as it has retained it staff and most of its procedures. Like the AEC, the NRC has been mainly concerned with expediting the development of nuclear power. It did not "burden" the industry with requirements to "backfit" older plants and bring them up to current safety standards. It downgraded penalties and enforcement sanctions. It failed to respond to questions of safety raised by some individuals within the agency. Nor did it engage in comprehensive evaluations of past operating experiences or systematically review the series of minor accidents that have plagued the development of nuclear technology. Focusing on licensing rather than generic problems, the NRC failed to establish adequate mechanisms to monitor safety-related problems, relying rather on licensees to detect and correct deficiencies without independent commission verification. As the Kemeny Commission reviewed the history of the NRC, it found it "erring on the side of industry's convenience rather than carrying out its primary mission of assuring safety" (President's Commission, 1979).

The complexity of nuclear technology and its broad-ranging impacts on the social environment create further problems of regulation. Organized to deal with specific industrial activities, regulatory agencies are unsuited to cope with systemic problems. The difficulty of responding to the Three Mile Island accident suggests the inherent managerial problems in coordinating intricate inter-organizational relationships.[5] For example, a veritable alphabet soup of public agencies was officially coordinated through an Interagency Radiological Assistance Plan designed specifically to mobilize expertise in cases of emergency. The response to Three Mile Island revealed that the existence of this plan was not well known, links between agencies were poorly established, and lines of responsibility weakly defined. At the state level, despite the existence of several environmental and health agencies, neither plans for emergency evacuation and medical care nor a capacity to monitor radiation levels were evident.[6] NRC Commissioner Joseph Hendrie described the dilemma: "We are like blind men staggering around" (Nuclear Regulatory Commission, 1979).

As a result of the accident and subsequent criticism of the NRC, its staff grew by 14 percent from 2,841 to 3,240 in one year, and its annual budget increased from $325.8 million to $423 million. Requirements were developed for additional training of reactor operators. Emergency plans include telephone hotlines to a commission emergency response center. The inspection system was improved and the structure of the commission itself reevaluated. But "improved" regulation will inevitably increase operating costs and it may also exacerbate the problems of complexity, compounding the difficulties of management and the risk of systemic effects.

The Courts. The courts have been deeply involved in the nuclear debate. The role of the courts in technological questions has generally expanded with the extension of the legal doctrine of standing. Courts are defining responsibility for damages caused by technology, imposing injunctions against possibly harmful developments, and galvanizing administrative agencies into more effective regulatory action. But legal action in many technological areas, including nuclear power, poses special problems for the courts as they increasingly become the channel through which aggrieved citizens react to unpopular technological decisions.[7]

The Three Mile Island accident generated an unprecedented legal battle to establish liability. The Price-Anderson Act provides guaranteed government subsidization of insurance liability against damage claims caused by nuclear accidents. The act set a ceiling of $560 million liability in claims. Private insurance offers protection of up to $125 million; the government is responsible for the difference. Following the Three Mile Island accident, 30 neighbors of the plant immediately filed a class action suit for a total of $560 million. Another suit against the designer of Three Mile Island sought damages of $1.06 billion. Complaints have included loss of income, devalued real estate, endangered health and "diminution of the enjoyment of life." It is argued that the limit set by Congress in the Price-Anderson Act is unconstitutional if the defendants can be proven guilty of gross negligence. Other legal battles over who should absorb the costs of accidents have been instigated by consumer advocates. And General Public Utilities Corporation, owner of the Three Mile Island plant, is suing Babcock and Wilcox to recover $500 million in damages that occurred as a result of the accident and plans to seek further damages for continued loss, estimated at $400 million. Babcock and Wilcox denies liability. The variety of issues under consideration by the courts have been vividly described by a nuclear law expert as "a can of worms."

The Scientific Community. The scientific enterprise is another institution profoundly affected by the controversial dimensions of nuclear technology. Scientific standards have long had a kind of universal appeal, serving as a model of rationality and a widely accepted basis of consensus on many policy questions. But the increasing involvement of scientists in controversial areas of public policy also has a deleterious effect on the image of technical expertise as an objective, apolitical and compelling source of information. Disputes among experts have called attention to the technical uncertainties that compound questions of risk. They have exposed the wide range of social and political variables that enter energy choices. And they have highlighted the role of technical expertise as a political resource employed by both sides of the nuclear debate to provide legitimacy and to reinforce existing beliefs.[8]

The confusion of information, evaluations, and conflicting claims that emerged from the Three

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[5] For general discussion of problems of coordinating interorganizational relationships, see McEachron and Tiege (1977).

[6] Since 1975, licensed power plant operators have been legally required to make agreements with state and local authorities about emergency plans, but as of March 1979, only 10 of 43 states with nuclear facilities had emergency response plans that met NRC specifications.

[7] For analysis of these problems with respect to environmental disputes, see DiMento (1977).

[8] For analysis of the political role of scientists in disputes, see Nelkin (1975).

Mile Island accident had just such effects. Hundreds of scientists from the NRC and other federal and state agencies, and utilities, and the industry, descended on Three Mile Island. In addition, "critical scientists" representing the nuclear opposition entered the debate. For example, a consulting firm had been formed by three General Electric nuclear engineers who had resigned from the company in 1976 in protest against what they felt was inadequate attention to nuclear safety and insoluble problems of human error associated with nuclear power. These scientists work for the anti-nuclear movement and were the technical consultants for the movie, *The China Syndrome*, released just prior to the accident and coincidentally portraying a very similar accident.

As these scientists assessed the problem, its dimensions and its causes were as varied as their interests and their concerns about the future of nuclear policy. The genuine technical uncertainty involved in evaluating risk allowed them a very wide range of interpretation. With a few standards to guide evaluation, scientists could easily make different judgments about the seriousness of health and environmental risks. Predictably, these judgments corresponded to these scientists' prior attitudes towards nuclear power (Nelkin, 1980).

Scientists at Three Mile Island were far more than simply investigators seeking the causes and dimensions of the accident. They served as publicists, image makers, as sources of credible information, as contacts for the persistent and ever-present press. They served to "represent" the views of both pro- and anti-nuclear groups, all with intense concerns about the future of nuclear power. Their public disputes in this and indeed in other controversies, creates a striking paradox. While inevitably undermining the credibility of scientific expertise as an objective basis for policy, public conflict has vastly expanded the expert's policy role. Special studies, commissions, and review boards have proliferated. Over $10 million was spent on official Three Mile Island inquiries, and hundreds of contracts for research on the causes and impacts of the accident were awarded.

The scientists' expanding influence following the Three Mile Island accident suggests the heavy dependence on the judgment of an expert elite whenever problems of technological risk arise. For power tends to gravitate towards those who can manage the critical information. As problems of risk assume increased policy importance, this tendency to augment the power of expertise raises serious questions about political legitimacy, exacerbating existing tensions over democratic control of technology and deepening public distrust (Kasperson, 1979).

The Media. The accident also had important implications for the media. The nuclear debate has increasingly engaged the press since the 1970s. Print media coverage of nuclear power increased by 400 percent between 1972 and 1976 (Rubin, 1979). But at Three Mile Island, the complex technical character of the events and the factual uncertainty among experts and regulatory officials came into conflict with enormous pressures for immediate reporting, to create a hopelessly confused situation. The public's right to information and the problems of communicating information about technology became the focus of a subsequent investigation.

The task force from the Kemeny Commission assessed these problems and reviewed the ability of the media to present timely, accurate and understandable information about the rapidly changing situation (Rubin, 1979). It found a number of basic problems. At least 500 journalists came to Three Mile Island. At first there existed no central information facilities, and no one to answer basic technical questions. The NRC also lacked the organization to deal effectively with public relations during the crisis. The utility was cautious and lacking in candor. With its reputation on the line, Metropolitan Edison "showed a marked capacity for self-deception [or] it hid behind technical jargon to obscure answers to troublesome questions" (Rubin, 1979, p. 9). Eventually the utility delegated the communication of official technical data to the NRC and avoided public interaction. However, it hired a team of public relations experts to present its views to the community.

Many reporters were poorly informed about rudimentary details of nuclear power, and this problem was compounded by the obfuscation caused by the technical language in which details were presented. Even those scientists who were willing to openly discuss the problems with reporters often had difficulties explaining themselves in non-technical terms. The problem, claimed the Kemeny Commission task force, was less the sensationalism of the press than the nature of the information actually provided to journalists—information often misleading and distorted by deliberate cover-ups and suppression of facts—as well as by genuine technical uncertainty.

In this context, press coverage was often confused, inaccurate, and indeed, as a consequence of inadequate information, sensationalist. The experience created an awareness of the importance of accurate communication—the need for journalists to develop some knowledge about nuclear technology, and especially for the nuclear establishment to provide a more candid and prompt response to demands for news.

Conclusions

Proponents and critics of nuclear power clearly view the risks of this technology through very different lenses. To those convinced of the importance of the nuclear program, the fact that the accident fell short of a meltdown demonstrated that the reactor safety systems basically worked, and that the public need only be reassured of their reliability. To critics, the accident revealed the potentially catastrophic risks of nuclear technology, but it also indicated serious political, psychological, and institutional ramifications that are neither easily measured nor resolved. As economic priorities and energy problems create powerful pressures for technological advancement, these less immediate and often vague social impacts of technology have tended to be dismissed.

I have used the Three Mile Island accident as an example through which to suggest a series of major institutional and policy issues surrounding complex technologies. The very scale of technological commitments; their complexity and the nature of their risks, generate their own institutional reality—a reality that calls for social science research. The area of regulation of technology is the most obvious link between technology and social science. Indeed, the problem of the NRC as both a promoter and regulator of nuclear power are familiar in the context of regulatory politics. But special problems arise when the stakes are very high. What kind of structures can be organized to enhance the accountability of this regulatory system, and the controllability of the nuclear industry with its monopoly of expertise? How does this industry deal with risk and the increasing pressure for greater public accountability?

The rapid increase in litigation as a means to force accountability presents a number of other problems suggested by the aftermath of Three Mile Island. The courts are poorly equipped to deal with the esoteric and uncertain scientific information that confounds the nuclear debate. Judges are accustomed to ruling on precedent, not on statistical assessments of potential risks. And they are not trained to evaluate the highly technical arguments that are increasingly involved in recent techno-political disputes. Moreover, many technologies raise problems that require reanalysis of legal principles: what is the appropriate role of law in assessing long-term effects of an accident such as Three Mile Island? How can courts be expected to have the competence to deal with technical issues? And how can they deal with questions of fairness when technological developments justified by the "public interest" infringe on local community interests? Who is liable for major accidents? Who is responsible for the long-term social costs that may be incurred?

The psychological stress observed in the Harrisburg region suggests a number of questions about the sociology of risk perception. What social processes are involved in the perception of risk by different groups? What are the discrepancies between perception and reality? How are these perceptions influenced by factors such as the availability of choice, the credibility of responsible institutions or the prevailing state of technical uncertainty?

Disputes among scientists not only affect public policy, but the very institution of science as well. What is the effect of such disputes on the scientific community and its traditional image of neutrality? On the role of science as a legitimating institution? What is the impact of technical uncertainty on the possibilities of public participation? On the credibility of policy-making institutions?

Finally, in the age of media politics, the role of the press looms large. Who speaks for government in communicating with the media? How can journalists develop the competence to deal accurately with events such as the Three Mile Island accident?

These are but a few of the many questions that have emerged from a social analysis of the Three Mile Island accident. It must be emphasized that social research on this event was an afterthought—the results of efforts by a small group of people who decided to collaborate in calling attention to the sociological ramifications of this event (Wolf, 1979). Eventually, however, the Kemeny Commission discovered that the accident was indeed a "people problem" that derived from the predispositions, the "mind sets," the myths of rationality and control held by key participants who tended consistently to dismiss the importance of human factors in managing nuclear technology:

> The Commission's attention slowly but steadily shifted almost completely from equipment to people. And we found that this was one of the most horrendous "people problems" we had ever encountered (Kemeny, 1980, p. 81).

Eventually, too, the problem was conceived less as a technical than a *social* system failure that derived in part from the nature of social relationships among the leading actors in the nuclear enterprise (Sills, 1980). In this sense, the Three Mile Island accident revealed with striking clarity that technology is a social concept to be understood in terms of social and political relationships if it is to be brought under public control.

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A Comment on Dorothy Nelkin's "Some Social and Political Dimensions of Nuclear Power: Examples from Three Mile Island"

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Dorothy Nelkin has written an informed, comprehensive, and useful summary of the diverse social and political aspects of the accident at the Three Mile Island nuclear power plant, and I have few quarrels with her conclusions. This agreement will come as no surprise to those who know that Nelkin and I have been closely associated on a number of nuclear power-related social science activities, including two on Three Mile Island (TMI) itself. The first such activity was a workshop on the social science aspects of TMI which we organized under the auspices of the Social Science Research Council, with funds from the President's Commission on the Accident at Three Mile Island (see Wolf, 1979); the papers presented at the workshop are to be published in early 1981 (Sills, Wolf, and Shelanski, forthcoming).

The second of these activities was a conference on the lessons and implications of TMI, held in April 1980. Sponsored by the New York Academy of Sciences and co-chaired by Thomas H. Moss and myself, the conference brought government officials and industry executives who had been participants in the post-TMI events together with natural and social scientists who had studied the accident. Nelkin was one of the major speakers. This is the first opportunity we have had to speak directly to the political science community about the social and political meaning of the accident at TMI.

Nelkin's article concentrates upon substance, upon those social and political dimensions of TMI that should interest political scientists. The paper is a model of substance, in fact, and it never raises the difficult question of why social scientists are asked to comment on "aspects," or "effects," or "dimensions," or "implications" of energy, rather than on energy itself. "Energy," I believe, is the product of social, economic, and political processes that define certain raw materials (coal, uranium, sunshine, wind) as resources. According to this formulation, "energy" is more of a social science concept than a physical science one.

The subject of my brief comment is thus more epistemological than it is substantive. My method, I fear, is polemical—in the theological sense of refuting what I take to be errors of doctrine.

The major doctrinal error that I wish to refute is the implicit or explicit assumption that energy is a technical subject to be studied by natural scientists. (This is how "energy research" is defined by the U.S. Department of Energy.) A corollary of this assumption is that social scientists should study not energy but the "implications" of energy, or the "effects" of energy on society. Because the issue is framed in this way, social scientists are usually brought in after the project is under way and the study design has been set. Sometimes a more explicit practice called "exclusion" is followed. Under this practice, the study directors acknowledge the importance of social and political factors, but deliberately exclude them as being beyond the "scope" of the study. An example is the MIT Energy Self-Sufficiency Study, as noted in the Harvard Business School Energy Project (Koreisha and Stobaugh, 1979, pp. 263-64).

A classic example is the well-known project of the National Academy of Sciences, the Committee on Nuclear and Alternate Energy Sources (CONAES), a project on which Nelkin and I both worked. More than a year had elapsed after CONAES was formed before our Working Group on Sociopolitical Effects was established. We worked hard and eventually published a report, but it is doubtful whether it had much impact. A prominent sociologist on the parent committee resigned more or less in disgust before the CONAES report was published.

Consider also the President's Commission on the Accident at Three Mile Island. In its report—which I generally admire—the commission stated that a principal cause of the accident was the "mindset" of personnel in the nuclear industry. What the report did not say is that the very same "mindset" that led to the accident also influenced the way the commission went about its task and recruited its staff. As the commission organized itself, it initially adopted a model of the accident that assumed it was an engineering failure that led to a plant shutdown and the release of radiation. The point was to find out who was responsible.

Because of this interpretation of the accident, the majority of the commission's staff consisted of nuclear physicists and engineers, accident analysts on leave from NASA, radiologists, lawyers, and journalists. Initially, little thought was given to the problems of organization and management, to say nothing of social behavior.

Interestingly, however, the final conclusions in the commission's report have very little to do with this interpretation; rather than its being an engineering failure, the commission concluded that the accident was the result of a social systems failure, although it did not use this term.

A view of the accident as primarily a social systems failure gained prominence in the commission's thinking during its investigation. According to its report, "As the evidence accumulated, it became clear that the fundamental problems are people-related problems and not equipment problems" (President's Commission, 1979, p. 8). John Kemeny, the chairman, noted in a 1980 speech at the Massachusetts Institute of Technology that "The Commission's attention slowly but steadily shifted, almost completely, from equipment to people. And we found that this was one of the most horrendous 'people problems' we had ever encountered" (p. 67).

The commission found that the crux of the "people problems" was in the perceptions and predispositions of operators, managers, vendors, and regulators. As the commission report stated, "In the testimony we received, one word occurred over and over again. That word is 'mindset' " (p. 8).

The attitudinal and ideological components of the word "mindset" are easily recognizable in the intellectual and research traditions of the social sciences. In psychology, there is the concept of the "response set"; in sociology, there are "dispositions"; in anthropology, there is "culturally determined"; and in economics, "propensity."

In sociotechnical systems such as nuclear power reactors, operators and equipment are of course not readily separable. The commission pointed to their inappropriate isolation from each other as a root cause of the accident:

> The most serious "mindset" is the preoccupation of everyone with the safety of equipment, resulting in the down-playing of the importance of the human element in nuclear power generation. We are tempted to say that while an enormous effort was expended to assure that safety-related equipment functioned as well as possible, and that there was backup equipment in depth, what the NRC [Nuclear Regulatory Commission] and the industry have failed to recognize sufficiently is that the human beings who manage and operate the plants constitute an important safety system (p. 10).

The NRC commissioned its own investigation of the accident by a special inquiry group directed by a Washington lawyer, Mitchell Rogovin. In its January 1980 report, the group also concluded that "the principal deficiencies in commercial reactor safety today are not hardware problems, they are management problems . . . problems [that] cannot be solved by the addition of a few pipes and valves—or, for that matter, by a resident Federal inspector" (Rogovin, 1980, Vol. 1, p. 89).

It would be pleasant to assume that this rather dramatic "discovery" by non-social scientists that the accident at TMI was a social science event would have an impact upon the nuclear energy establishment. It would be pleasant, but it hasn't happened. And the reason of course is that mindsets are not changed easily, particularly when history, career patterns, funding, and prestige are all on the side of considering nuclear energy as the nearly exclusive terrain of natural science and engineering.

It is obvious that nuclear fission is a subject for physicists to study, but nuclear energy—and, more broadly, energy from any source—is only in a relatively trivial sense a problem for physical scientists and engineers. In a technical sense it would be easy to produce adequate supplies of energy: both the popular and the scientific press report many schemes that would provide "unlimited" energy. The problem is not the techniques for producing energy; rather, it is that of producing it from raw materials that are owned by others (Arabs, American Indians, farmers) at a political and economic price we are willing to pay, making it widely accessible in socially acceptable forms, and avoiding its negative impacts. In short, energy is in large part a problem of social and political organization, and thus a problem for the social sciences.

The accident at TMI provides a dramatic example of the social nature of nuclear energy. When the Atomic Energy Commission commissioned the $3,000,000 Rasmussen Report (1974), it undoubtedly thought that it had taken a giant step forward in allaying public fears of accidents at nuclear power plants, since the report concluded that nuclear energy was much safer to produce than other forms of energy. However, the response to TMI demonstrated how little public perception of risk has to do with the kinds of calculations presented in this report. Risk analysis involves far more than the computation of probabilities. Individual and cultural variation must be considered, as must the enormous difference between risks that are taken for granted versus those that seem exogenous to a person's normal pattern of life.

Defining energy as a social rather than a physical concept will not take place this year or next: there is too much at stake. But there are several signs of hope. Social scientists themselves are beginning to study energy and as recent bibliographies (e.g., Frankena et al., 1979) and survey articles (e.g., Wilbanks, 1980) demonstrate, the interest of social scientists in these problems is spreading at a fairly rapid rate. Time is also on our side: the generation of physicists and engineers which developed nuclear weapons and nuclear power in the 1940s and 1950s is aging, and is being replaced in part by a generation that has been forced to acknowledge the social nature of energy. Although the social science community is not adequately prepared either to prevent or to study the "next" nuclear accident, it is much better prepared than ever before. Nelkin's article may thus be viewed both as the apogee of a concern for the social science "aspects" of nuclear energy and the harbinger of a time when these aspects will constitute the core (pun intended) of nuclear energy research.

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COMMUNICATIONS

ON ARTICLES

Comment on Smith (Vol. 74, September 1980, pp. 685-96)

TO THE EDITOR:

The ideological sophistication of the American electorate has increased during the postwar years. This finding has been reported in studies that analyzed the coded responses to the eight basic open-ended University of Michigan Survey Research Center questions about parties and candidates used to measure levels of conceptualization (Field and Anderson, 1969; Nie, Verba, and Petrocik, 1976, pp. 110-22), and has also been reported in studies that analyzed the verbatim responses to these same questions (Klingemann and Wright, 1973; Miller and Miller, 1976, pp. 844-45; Pierce and Hagner, 1980, pp. 72-73). Norman H. Nie, Sidney Verba, and John R. Petrocik provide the most comprehensive of these analyses, for they examined all six presidential elections from 1952 through 1972, and have now studied all seven elections through 1976.

In his recent article, Eric R. A. N. Smith (1980) calls all these studies into question. Smith analyzes the coded responses for the 1956-58-60 SRC panel data to demonstrate that among individuals, levels of conceptualization as measured in 1960 are only weakly related to these levels as measured in 1956. He argues that since level of conceptualization is supposed to measure how people think about politics, it should be a fairly stable attitude trait. Low over-time correlations, he argues, suggest that the party and candidate questions measure "the *rhetoric* of contemporary political discourse rather than the actual *process* of political evaluation" (p. 685). "The measures of the levels of conceptualization," he claims, "seem to be tapping the language that the evaluations take after they have been formed, rather than the actual processes of evaluation" (p. 694).

Smith raises some compelling logical arguments, especially when he discusses the processes through which people learn to evaluate parties and candidates. Yet his empirical findings are not conclusive, as he himself realizes. In fact, Smith's most important analysis, a three-wave reliability estimation of the party component of levels of conceptualization that employs the Nie, Verba, and Petrocik measure, may raise some doubts about his own conclusions. He reports that "virtually *all* of the change in the observed measures over the 1956-1960 period is due to measurement error. The underlying trait is nearly constant" (p. 693). In view of these findings, proponents of the conventional interpretation of these measures may be forced to concede that the Nie, Verba, Petrocik measure is unreliable. But they could argue that, once this measurement unreliability is taken into account, the "true" over-time stability of levels of conceptualization is high, precisely what one would expect if these procedures measured the way people think about politics.

Although Smith rejects the conventional interpretation of these measures, he accepts the conclusion that there has been a change in overall "levels of conceptualization," claiming that this means only that there has been an increase in the use of ideological rhetoric. Even if we accept Smith's basic argument, we might still want to know the extent to which the use of such language has changed during the postwar years.

The logical place to turn for the most complete analysis of change is the "enlarged" edition of *The Changing American Voter.* However, the newer edition raises serious problems. Although Nie, Verba, and Petrocik (1979) claim that levels of conceptualization declined in 1976, this conclusion is questionable. Moreover, there are major, unexplained differences between levels of conceptualization as reported in the 1976 edition of *The Changing American Voter* and those reported in the 1979 edition.

Let us begin with the Nie, Verba, and Petrocik

I am grateful to John H. Aldrich, Bernard M. Finifter, John E. Hunter, Frank A. Pinner, Charles O. Press, Brian D. Silver, and especially Ada W. Finifter, for suggestions on earlier versions of this comment. Two anonymous referees for the *Review* made helpful suggestions. The data used to attempt to replicate the Nie, Verba, and Petrocik results were provided by the Inter-University Consortium for Political and Social Research, which is not responsible for my analyses or interpretations. Rick E. Rollenhagen assisted with these data analyses.

claim that levels of conceptualization declined in 1976 (see 1979, pp. 365-67). In reporting a downturn, Nie and his colleagues state that they had anticipated a decline, given the nature of the 1976 election. As they write, "In the absence of a polarizing choice in 1976, we would expect the public to return to earlier models of thinking, to conceive of the candidates less in issue and ideological terms and to have less coherent issue positions" (1979, p. 365).

It is questionable whether levels of conceptualization actually declined in 1976. Let us recall that Campbell and his colleagues (1960, pp. 222-23) based their measure of levels of conceptualization upon the responses to eight open-ended questions about political parties and presidential candidates. Nie and his colleagues built three measures of levels of conceptualization, one based upon responses to the four party questions, one to the four candidate questions, and one that combined responses to all eight questions. Only the last measure can be compared with the reports of scholars who have used the verbatim responses. Though there may well be advantages to dividing levels of conceptualization into a separate party and candidate component, and although analyzing the party component separately was essential for Smith's three-wave panel analysis, the combined trend line becomes the most important to follow.

Unfortunately, Nie and his colleagues do not report these results for 1976. They report (1979, pp. 366-67) the percentage of ideologues for the candidate questions (7 percent), the total percentage of ideologues and near-ideologues for the candidate questions (16 percent), and the percentage of ideologues for the party questions (35 percent). Given these results, we know, by definition, that the *combined* percentage of ideologues for the party and candidate questions must be at least 35 percent. Yet the combined percentage of ideologues for the party and candidate questions in 1972 was only 33 percent (1976, p. 115). The claim that levels of conceptualization declined in 1976 is therefore valid for only one method of measuring these levels, and is not valid for the method (combining party and candidate responses) originally advanced in *The American Voter,* and which is used in much of Nie's, Verba's, and Petrocik's own analysis.

But just what was the percentage of ideologues in 1972? That depends on which edition of *The Changing American Voter* one refers to. Although we are told that the 1979 version of the book remains the same as the 1976 version (1979, p. viii), except for the added chapter on the 1976 election, and although we are provided with neither a warning nor an explanation, the percentage of ideologues differs in the 1979 (p. 115) and the 1976 (p. 115) editions of the book. In fact, Figure 7.3, which reports the total percentage of ideologues and near-ideologues, has been replaced by a new Figure 7.3. Figure 1 of this comment presents the percentage of ideologues and of ideologues plus near-ideologues in the two editions of the book. For each measure, the solid line shows the results as published in 1976, the broken line as they were published in 1979. As Figure 1 reveals, there are differences between the results for ideologues for all years except 1956. For the combined percentage of ideologues and near-ideologues, the results are always different, and are dramatically different for 1964 and 1968.

Which version of the data should we accept? As there is no discussion of the newly reported results, Nie, Verba, and Petrocik provide us with no guidance. Nor can available survey data be used easily to choose between the two sets of results, since the published coding procedures (1976, pp. 373-75; 1979, pp. 403-405) are applicable only to the party questions. As these procedures provide no warning that the categories are applicable only to the party questions, anyone who directly followed them would inadvertently be applying a meaningless schema to the four candidate questions.[1]

[1] I did use the available SRC data for 1964 and 1968 to attempt to determine which set of figures was more nearly correct. By following the procedures spelled out in Appendix 2C of *The Changing American Voter,* I came close to replicating the total percentage of ideologues and near-ideologues reported in the 1976 edition. However, a close examination of ICPSR 1964 and 1968 election codebooks (as well as those for 1952, 1956, and 1960) reveals that this coding schema is, in fact, applicable only to the party questions, because the same code numbers that refer to ideological type *party* references frequently refer to nonideological type *candidate* references. (For example, value "270" for the party questions refers to, "Like their liberal philosophy," or "Don't like their liberal philosophy," while the same value for the candidate questions refers to "Good family life; like his family," or "Don't like his family.") I then used my own coding decisions to attempt to build a measure of ideology and near-ideology for the candidate questions, and by using my own procedures (along with *The Changing American Voter* procedures for the party questions) came close to reproducing the results of the new Figure 7.3. However, in no case could I exactly reproduce the results reported in either version of Figure 7.3. Moreover, although I derived exactly the same results for the party questions reported in Figure 7.1, I could not reproduce the results for the candidate questions reported in Figure 7.2 of *The Changing American Voter.* In fact, so many coding decisions are necessary to create a coding schema for the candidate questions that it would be difficult to assure that any newly constructed schema would be comparable to that used by Nie, Verba, and Petrocik.

Whether these problems bear upon Smith's analysis is difficult to say. Smith reports (p. 687, n. 4) that "a complete description of the measure" is found on pp. 18-23 and in Appendix 2C of *The Changing American Voter.* It is possible, however, that Smith did not follow the Nie, Verba, and Petrocik procedures, since the percentage of ideologues and of near-ideologues in his analysis (see Table 3, p. 689) differs substantially from the percentages reported in either edition of *The Changing American Voter.*[2] Even if he did use the published Nie, Verba, and Petrocik procedures, this error would not affect his analysis of the levels of conceptualization measures developed by Field and Anderson, or of the party component measure based upon the Nie, Verba, and Petrocik schema. Fortunately, Smith's three-wave panel analysis is based only on the party component.

Despite the difficulty in replicating the Nie, Verba, and Petrocik results, it seems reasonable to assume that the original *Changing American Voter* report was wrong, and that the newly published figures are more nearly correct. How else can we explain the fact that a new figure was substituted for the old?[3]

If the newly reported results are correct, the problems are more far-reaching than a mere discrepancy between one edition and its successor, since the newly reported results do not correspond with the Nie, Verba, and Petrocik interpretation of changing levels of conceptualization. Rather than increasing between 1960 (the nonideological Kennedy-Nixon contest) and 1964 (with Goldwater's explicitly ideological appeals), the percentage of ideologues marginally declines. Rather than a dramatic 18-percentage-point jump in ideologues and near-ideologues, there is a negligible 3-percentage-point increase. As opposed to the dramatic step-function increase reported in the 1976 edition, we now see a far more gradual rise in the percentage of ideologues and near-

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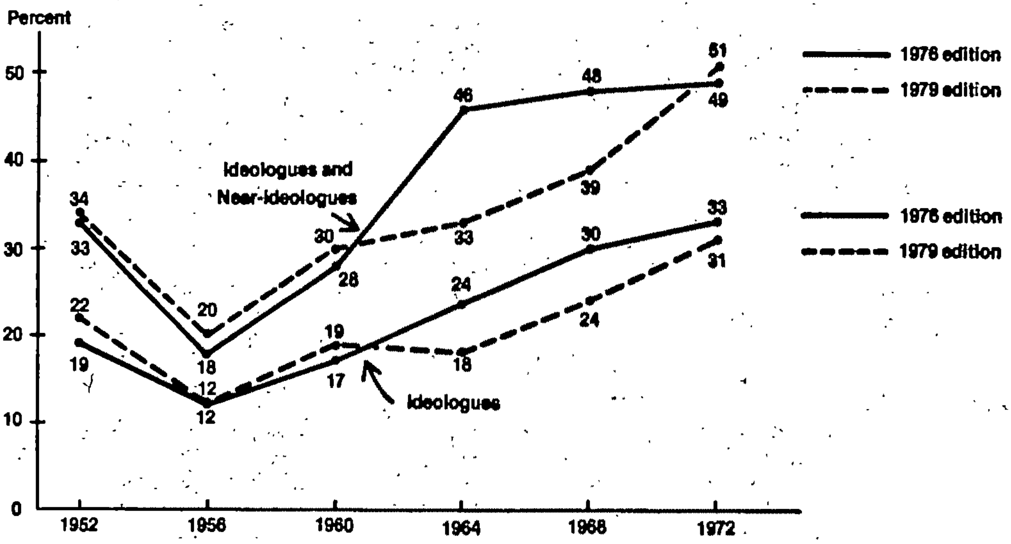
[2] One would expect some differences between Smith's results and those reported in *The Changing American Voter,* since Smith's table is based only upon respondents in both the 1956 and 1960 waves of the SRC panel, while Nie, Verba, and Petrocik's analysis is based upon the 1956 and 1960 electorate samples. But the differences between Smith's results and those reported by Nie and his colleagues are greater than one might expect from systematic biases caused by panel attrition.

[3] Assuming that the authors have corrected an earlier error, they are certainly to be commended. It would have been helpful, however, if they had explained the reason for the change, and the implications of this change for other figures reported in their chapter on changing levels of conceptualization.



Sources: Solid lines based on Norman H. Nie, Sidney Verba, and John R. Petrocik, *The Changing American Voter* (Cambridge, Mass., Harvard University Press, 1976), p. 115. Broken lines based on *The Changing American Voter,* enlarged edition (1979), p. 115.

Figure 1. Ideological Evaluation of Candidates and Parties from 1952 through 1972 (Percentage of Ideologues and of Ideologues and Near-Ideologues in the Two Editions of *The Changing American Voter*)

ideologues. In short, the processes that Nie and his colleagues claim led to the increase in levels of conceptualization do not correspond to the data as reported in the 1979 edition. Overall levels of conceptualization do not appear to respond to short-term political conditions in the way Nie, Verba, and Petrocik claim.

A comprehensive search for the causes of changing levels of conceptualization poses interesting research problems. The newly reported trend line suggests that rising levels of conceptualization may result from long-term secular changes. For example, increases in education among the electorate may be more important in creating these changes than Nie and his colleagues claimed. Perhaps television, by lowering the cost of political information, has led to gradual increases in ideological sophistication. Perhaps, too, learning to become ideologically sophisticated is a long-term process, which is reinforced over a series of ideological elections, or by living in a more polarized political era. Exploring these possibilities is a major research project that could yield important methodological insights by shedding light on the robustness of these measures in the face of alternative coding procedures. Such research might or might not contradict the main thesis of *The Changing American Voter,* but, regardless of the outcome, would give us a more solid understanding of the processes through which levels of conceptualization change over time.

If we accept Smith's basic thesis, such an analysis would be less important. Even so, we might wish to understand the processes through which the use of ideological rhetoric increased during the postwar years, for this increase may reflect real-world change in political behavior. Once again, we would need better research designs to explain change more adequately.

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Reply

TO THE EDITOR:

Paul R. Abramson (1981) and Eric R. A. N. Smith (1980) both comment on the "levels of conceptualization" measure in *The Changing American Voter.*

First to Abramson: He has indeed caught a large discrepancy between the original edition of *The Changing American Voter* and the enlarged edition. The new data would weaken our argument as to a shift in 1964. Abramson surmises that we corrected the data when we prepared the enlarged edition and quite properly suggests that we should, under the circumstances, have made some comment.

In fact, the story is somewhat different, but almost as embarrassing. The enlarged edition differs from the earlier edition only in the addition of a new chapter on the 1976 election. The earlier chapters are unchanged. We did, however, take the opportunity to correct typographical errors in the text and data. In the case of Figure 7.3, there were some minor changes to be made (the percentage of ideologues in 1972 should be 45 percent rather than 49 percent as in the original edition; the combined percentage of ideologues and near-ideologues should be 23 percent instead of 24 percent in 1964). Rather than make those minor changes, we substituted completely incorrect data in the enlarged edition. In short, Figure 7.3 in the original edition is correct; the new enlarged edition is wrong in that respect. The appended Figure 1 contains the correct data for the combined party and candidate measure which should replace the marginally incorrect Figure 7.3 in the original edition as well as the grossly mistaken Figure 7.3 of the enlarged edition. Figure 1 also brings the data series up through 1976.

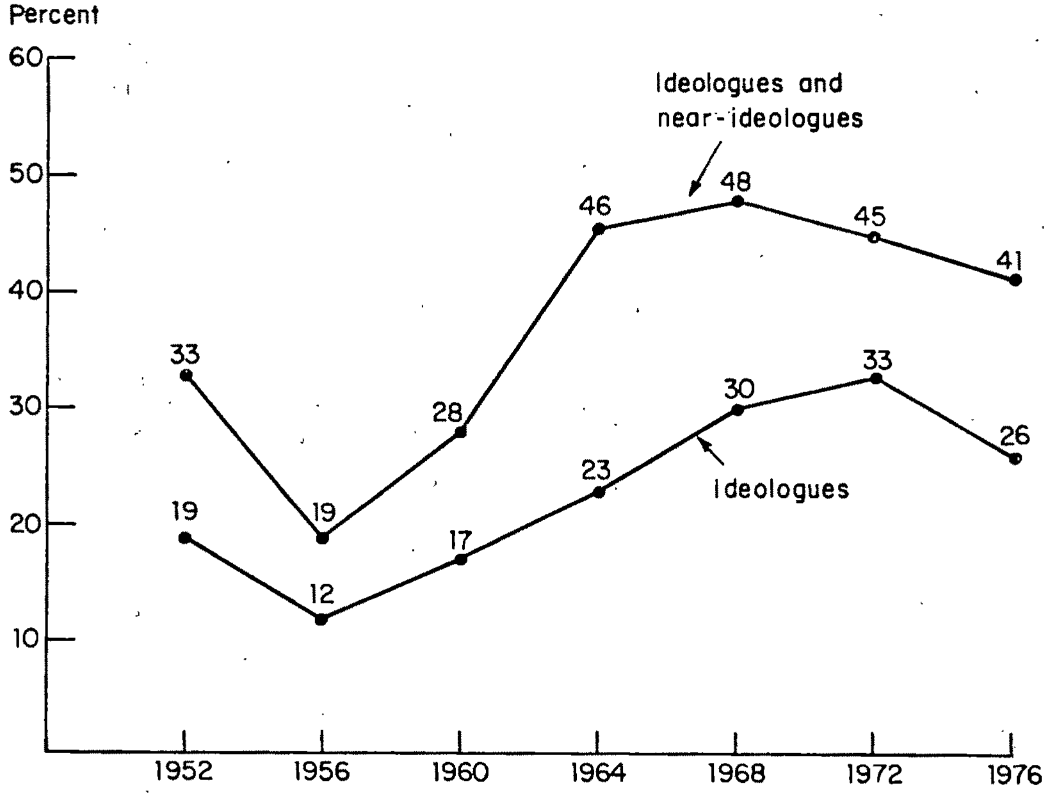
We are grateful to Abramson for calling the error to our attention.

How could it happen? you may ask. The specific mistake was a clerical confusion among several variables with SPSS acronyms that were almost identical (users beware!). It was—as it almost always is—human error, not machine error. And unfortunately, we were the humans.

The same clerical confusion led to another mistake which we would like to correct as well. It relates to another point that Abramson makes. On page 366 of the enlarged edition, we report that "the proportion of ideologues vis-à-vis parties rises from 22 percent in 1972 to 35 percent in 1976." The correct figure for 1976 should be 24 percent. This figure is consistent with a decline in the combined candidate/party measure in 1976.

To summarize, now that we have got the data correct: There was an increase in the proportion of respondents using ideological or near-ideological terms between 1960 and 1964. However, the increase on the combined candidate and party measure derives, as we point out in *The Changing American Voter*, almost entirely from a change in the terms used to refer to candidates. The party references change very little. The situation is similar in 1976. There is a sharp drop in ideological and near-ideological references to candidates but almost no change (indeed, a slight increase) in such references to political parties. All this is consistent with the argument made in *The Changing American Voter* that respondents are sensitive to the stimuli they are offered. The candidate rather than the party stimuli change from election to election.

This brings us to Eric R. A. N. Smith's article, "The Levels of Conceptualization: False Measures of Ideological Sophistication." Smith analyzes our measure of "levels of conceptualization" based on categories of the CPS master codes. He concludes that "instead of reflecting conceptual sophistication, the measures are reflecting short-term forces unrelated to conceptual sophistication" (p. 674). "They do not and can-



Sources: Norman H. Nie, Sidney Verba, and John R. Petrocik, *The Changing American Voter*, enlarged edition (Cambridge, Mass.: Harvard University Press, 1979), p. 114, Figure 7.3 (as corrected in this communication); Eric R. A. N. Smith, "The Levels of Conceptualization: False Measures of Ideological Sophistication," *American Political Science Review* 74 (1980), p. 689, Table 3.

Figure 1. Corrected Combined Ideologue/Near-Ideologue Distribution, 1952-1976

not show that the electorate is becoming more ideologically aware" (p. 676). This is parallel to our own conclusion. As we wrote in the original edition of *The Changing American Voter,*

> The data on levels of conceptualization do support the hypothesis that the way in which citizens conceptualize the political realm is dependent on the political content to which they are exposed (p. 121). To understand the sources of these changes [in attitude consistency and levels of conceptualization], it may be better to look outside of the individual citizen to the real world of politics. The way in which people think about the political world is not merely the result of their social and psychological characteristics—the education they have or their cognitive capacities. The way people think about politics is also a reflection of the stimuli offered them by the political world. . . . If greater political sophistication were the explanation, one would expect more frequent political reference among the upper educated and/or a greater change in that frequency among those citizens with higher levels of education. Rather the data suggest that the world of politics impinges on the lives of all citizens (pp. 151-52).

This conclusion in the original edition of *The Changing American Voter* is reinforced by the findings reported in the enlarged edition that attitude consistency and level of conceptualization both fell in 1976 from their 1972 levels. The explanation has to lie in the nature of political discourse in 1976, not in a sudden decline in political sophistication.

Smith obviously misreads us when he says we argue that the change in our measure indicates "real change in the level of conceptual sophistication" (p. 694). Our interpretation is quite consistent with Smith's argument that the measures represent responses to "contemporary discourse" in a particular election.

Not only is Smith's overall interpretation consistent with our own, but also his data are consistent with our own qualification of that conclusion: that there is evidence that the measure may pick up long-term "cognitive limitations, especially at the lower reaches of the levels of conceptualization scale" (p. 122). Smith's data (Table 3, p. 689) show that of those respondents categorized as "apolitical" in 1956 because they gave apolitical or nature-of-the-times responses, only 4 percent had risen to the near-ideologue category in 1960 and 1 percent to the ideologue. In comparison, about a third of those in the middle categories had risen to one of the upper two categories in 1960.

We also agree with Smith's strictures as to the value of the master codes for building measures that achieve any depth in exposing the degree of conceptual sophistication of the electorate. It is doubtful that the level of conceptualization concept, as Converse meant it, can be validly measured by the mechanical use of the open-ended master code. One probably has to read the individual protocols to distinguish between those people who use general concepts for nuanced and complex interpretations of the political process and those who have learned a number of "buzz words" to describe contemporary affairs. In short, an analysis of the level of conceptualization of the public using the master codes illustrates the responsiveness of the population to contemporary politics rather than some fundamental change in the sophistication with which they analyze political matters.

Where we disagree with Smith is in his finding that our measure of levels of conceptualization shows so much temporal instability as to be an unreliable measure of the terms which respondents use to evaluate candidates and parties. The reason we doubt the relevance of his results to our measure is that he is apparently using a quite different measure. Table 1 compares our level of conceptualization measure with Smith's for both the party and candidate measures in 1956 and 1960. In both years Smith finds many more ideologues and near-ideologues than we do. We cannot help but wonder if a large portion of the instability that he reports simply reflects the use of a measure that is less demanding than ours. If he has misclassified many respondents (and we are certain that he has identified as ideologues or near-ideologues many respondents who would not make it by our criteria, and certainly not by a closer content analysis), the low correlations in his tables may reflect the looser definition he is using.[1] The difference between our measure and Smith's means that one cannot use his research to evaluate our analysis.

Lastly, even if the levels measure used by Smith

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[1] We may be partially to blame for this discrepancy. The codes presented in Appendix 2-C of *The Changing American Voter* are valid for creating a level of conceptualization measure applicable to political parties. Somewhat different codes have to be used for candidates. (In addition, there are some year-by-year modifications that must be carried out.) However, several years ago we sent Smith a copy of the full SPSS code used to build the two separate measures. In a recent letter to John Petrocik, Smith indicated he was having difficulty replicating our findings. We were not quite sure why and sent another copy of the code. In any case, the letter (dated July 31, 1980) arrived after Smith's article was in page proof and was too late to have any effect on the material he has published.

The categorization using the master code requires adjustments in 1968 and 1972. Those who want copies of the code needed to create the categorization, including these adjustments, should contact one of the authors.

Table 1. Comparison of Smith's Combined Level of Conceptualization Measure with Ours (Percent)

	Ideologues	
	Smith	*The Changing American Voter*
1956	24	12
1960	31	17
	Ideologues and Near-Ideologues	
	Smith	***The Changing American Voter***
1956	37	19
1960	53	28

Sources: Norman H. Nie, Sidney Verba, and John R. Petrocik, *The Changing American Voter*, enlarged edition (Cambridge, Mass.: Harvard University Press, 1979), p. 114, Figure 7.3 (as corrected in this communication); Eric R. A. N. Smith, "The Levels of Conceptualization: False Measures of Ideological Sophistication," *American Political Science Review* 74 (1980), p. 689, Table 3.

were the same as our own, we are not sure that one can justify Smith's crucial test of the reliability versus stability of the measures. He uses the Wiley and Wiley technique on the three-wave 1956, 1958, and 1960 data. However, our measure does not meet the appropriate statistical criteria for such analysis. Smith believes our measure "approximates an interval measure" (p. 793, n. 11). We are sure it is not an interval measure and doubt that it even approximates one. The level of conceptualization typology is clearly multi-dimensional. There is perhaps an abstractness dimension separating ideologues from apoliticals; but we have no idea what dimension orders the other categories. If the levels are categorical (and we think they are) nothing is learned from the Wiley and Wiley technique, since it is inappropriate to perform such statistical computation on categories.

In sum, though we agree with the main thrust of Smith's interpretation of his findings, we do not believe that the measure he uses is comparable enough to our own to allow of close comparison, nor are we convinced by the statistical techniques he uses to analyze the measure.

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Reply

TO THE EDITOR:

The letters by Paul Abramson (1981) and by Norman Nie, Sidney Verba and John Petrocik (1981) raise interesting points which require comment. I shall address myself to Abramson's comments first.

Abramson discusses my analysis of the party component of the Nie et al. level-of-conceptualization measure in which the reliability of the measure and the stability of the underlying trait over time are estimated.[1] He notes that one interpretation of these data is that the measures are very unreliable, but that the underlying trait—conceptual sophistication—is very stable. If the estimates of reliability and stability were taken in isolation, ignoring the other available data, then such an interpretation might be reasonable. But other data are available, and these data lead to the conclusion that the underlying trait being measured is not level of conceptual sophistication. In other words, the data suggest that the measure is not valid.[2] Let us briefly review the construction of the level measures and then consider the evidence.

The level-of-conceptualization measures are based on eight open-ended questions asking what the respondent likes and dislikes about each of the major parties and their presidential candidates.

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[1] The reliability of a measure is the proportion of true score variance out of the total variance (which equals the true score variance plus the random error variance). The stability of a trait is the correlation of the true scores at two points in time.

[2] Loosely speaking, a measure is valid if it measures the phenomenon which it is claimed to measure. Validity is a matter of degree, expressed in a validity coefficient. More formally, "The most common way of explicating the general concept of validity in statistical terms is as a *validity coefficient*, the (absolute value of the) linear product moment correlation between the test and a specified criterion" (Lord and Novick, 1968, p. 261).

Campbell and his colleagues (1960), who create the original measure, believe that all eight questions tap the same underlying trait, namely, the level of sophistication at which people conceptualize politics. So they create a single measure based on the eight questions. When Nie, Verba and Petrocik deal with the measure, they first separate the questions on the parties from the questions on the candidates and construct two subscales, or "components." They then combine the two components into an overall measure.

Because the two components measure very closely related phenomena (if not, in fact, the same phenomenon), they should be highly correlated. The correlation coefficient between the two components is a test of validity, a validity coefficient. Looking at the data, we find that the correlation between the party and candidate components in 1956 was only .28. In 1960, it was .31. Both of these validity coefficients are extremely low, indicating that the two components are only weakly related, and that the overall measure is not valid.

When faced with findings such as these, we must ask why the validity coefficients should be so low. One explanation is that the spontaneous use of ideological rhetoric (e.g., words such as "liberal" and "conservative") in responding to the eight open-ended questions is a poor indicator of the level of sophistication at which people think about politics. This explanation is supported by findings by Field and Anderson (1969, p. 388) and Pierce (1970, p. 34) that the use of ideological rhetoric varies independently of concern for substantive ideological problems and knowledge about the meanings of the words "liberal" and "conservative." Thus it is not at all clear what people's use of ideological rhetoric tells us about how they think about politics. While, as Abramson points out, these data are not conclusive, they surely cast doubt on the validity of the level-of-conceptualization measures.

The second question raised by Abramson is whether I correctly replicate the *Changing American Voter* candidate component of the levels measure. Nie, Verba and Petrocik raise the same question and suggest that my findings are worthless because I did not replicate their work exactly. I did make a mistake in recreating their candidate component measure; however, the mistake was not large and the corrected data support my position more strongly than did the original, incorrect data.

Tables 1 and 2 present the corrected data. Table 1 shows a cross-tabulation of the overall level-of-conceptualization measure in 1960 with the same measure in 1956. The test-retest reliability coefficient of .31 differs by only .01 from the coefficient originally reported. Table 2 shows a cross-tabulation of the candidate component measure in 1960 with the same measure in 1956. The originally reported reliability coefficient was .20. The correct reliability coefficient is actually only .15. This is the only substantive difference from data presented in the article. It indicates that the candidate component measure was slightly less reliable than stated in the original article. Thus none of the conclusions based on these data is affected in any way.

The principal criticism by Nie and his colleagues is that my findings do no more than "parallel" what they say in *The Changing*

Table 1. The Nie, Verba and Petrocik Levels of Conceptualization, 1960 by 1956 (as Corrected)*

	1956								
1960	Apolitical	Party	Groups	Issues	Groups/ Issues	Near-Ideologues	Ideo-logues	Total	N
Apolitical	38%	10%	7%	8%	1%	5%	1%	8%	94
Party	17	22	14	16	6	9	5	13	158
Groups	16	13	26	10	11	9	9	14	163
Issues	18	28	19	32	23	23	9	23	273
Groups/Issues	6	9	14	12	31	7	10	14	170
Near-Ideologues	4	12	6	11	6	22	20	11	130
Ideologues	2	6	14	13	21	25	47	18	216
Total	7%	17%	17%	21%	18%	7%	13%	100%	
N	84	204	209	253	215	87	152		1204
	Tau-b = .31								

Source: Data from the 1956-1960 National Election Panel Study, Center for Political Studies, University of Michigan. John Petrocik provided instructions for coding.

*This ise the corrected version of Table 3, from Eric R. A. N. Smith, "The Levels of Conceptualization: False Measures of Ideological Sophistication," *American Political Science Review* 74 (1980), p. 689.

Table 2. The Nie, Verba and Petrocik Candidate Component, 1960 by 1956 (as Corrected)*

	1956								
1960	Apolitical	Party	Groups	Issues	Groups/ Issues	Near-Ideologues	Ideologues	Total	N
Apolitical	37%	12%	13%	12%	12%	9%	13%	16%	194
Party	30	41	30	36	24	36	40	36	428
Groups	8	8	10	5	12	9	0	8	92
Issues	17	29	29	32	30	24	0	27	329
Groups/Issues	4	4	9	7	12	6	7	6	67
Near-Ideologues	3	5	6	5	4	6	20	5	58
Ideologues	2	2	3	3	7	9	20	3	36
Total	16%	40%	9%	25%	6%	3%	1%	100%	
N	190	483	107	300	76	33	15		1204
	Tau-b = .15								

Source: Data from the 1956-1960 National Election Panel Study, Center for Political Studies, University of Michigan. John Petrocik provided instructions for coding.

*This is the corrected version of Table 6B, from Eric R. A. N. Smith, "The Levels of Conceptualization: False Measures of Ideological Sophistication," *American Political Science Review* 74 (1980), p. 692.

American Voter. That is not the case. My position is that their level-of-conceptualization measures are neither reliable nor valid.

They deny this, claiming that their measures are both valid and reliable. But this implies that the conceptual sophistication of one's thinking about politics can change radically from one year to the next. Such a notion of conceptual sophistication is difficult to accept.

One problem with evaluating the claim of "parallel" findings is that it is not clear what Nie, Verba and Petrocik think their measures reflect. At one point, they argue that I have misread them when I say that change in their measure indicates "real change in the level of conceptual sophistication." Apparently they see some difference between the terms "level of conceptualization" and "conceptual sophistication." Whatever this difference is, they fail to explain it. Since I follow Campbell et al. (1960, p. 222), in treating the terms synonymously, I have no idea of how I might have misread them. In sum, they are not entirely clear about what they mean by the term "level of conceptualization."

Nie and his colleagues close by criticizing my use of the Wiley and Wiley technique for estimating reliability and stability. In doing so, they say that the level of conceptualization typology is "clearly multi-dimensional" and should be treated as a categorical variable. But if it so "clearly" multi-dimensional, what are some of the other dimensions? They have no idea. They present no evidence.

The interval-level assumption underlying my use of the Wiley and Wiley technique is obviously not fully met. But it is valuable because it allows us to approach the problem of the reliability of the measures from a second perspective. The test-retest reliability coefficients yield a finding of low reliability. The Wiley and Wiley technique, based on different assumptions and different data, yields the same answer. The conclusion that the measures are unreliable was further supported.

In conclusion, I think that the comments by Nie, Verba and Petrocik fail either to demonstrate that their measures are reliable and valid or to show that my findings are consistent with the analysis they present in *The Changing American Voter*.

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Comment on the Merelman-Jennings Exchange (Vol. 74, June 1980, pp. 319-41)

TO THE EDITOR:

Richard Merelman and I continue to have different perspectives regarding the description of the basic shape of schooling and the conflict between democratic values and the basic shape of schooling, these being the subjects of the last two sections of his "Reply" (1980, pp. 339-40). However, I think the relative merits of our not-so-serious differences can be safely judged by the readers without a point-for-point elaboration on my part.

A far more fundamental and less subjective set of differences involves the key issue of whether schooling contributes to the development of democratic values (1980, pp. 338-39). Merelman's first impulse is to dismiss as irrelevant my basic point about the widely observed relationship between education and democratic values among adults. It is irrelevant, he says, because he was talking about pre-adults. It puzzles me as to why the later effects of pre-adult education are not germane to his argument, since education is usually seen, both empirically and normatively, as preparation for later life. Indeed, I thought that was the whole point of Merelman's brief, for if education is not related in either a good or bad way to the quality of (adult) democratic life, then why bother talking about it?

In any event, Merelman is incorrect even with respect to pre-adults. If the numerous studies of pre-adult socialization have shown anything, it is that knowledge of and commitment to democratic values and practices undergo enormous progression over the pre-adult years. Of course the schools do not deserve sole credit for that accomplishment. But it is foolish to ignore this developmental profile in making grand assumptions about the inefficacy of schooling.

To compound his predicament, Merelman rejects his first impulse and decides to challenge my assertions about the salutary effects of education on adult values and the suggestive similarity between that nexus and the one between education and occupational attainment. He does so by referencing the literature on the association between education and political knowledge rather than democratic values. He would have been better off sticking to values, though even there the evidence is decidedly against him. In the first place, he casts the link between education and occupational attainment in a very generous light by citing some of the most handsome results ever obtained in that literature. Somewhat more modest figures, for example, are reported by Treiman (1977, pp. 208-13). It is also worth noting that education is often (and legitimately) used in estimating occupational status scores, as in the famous Duncan indexes, thus helping ensure vibrant relationships between the two.

Second, and much more seriously, Merelman severely understates the education-political knowledge link—as scholars who have worked in the area would instinctively suspect. He relies on correlations based on very gross, single-item indicators which, by the way, happened to be among the very few out of hundreds reported by Hyman, Wright, and Reed (1975) as product-moment rather than gamma coefficients. A little more sensitivity on this point would have alerted Merelman to the presence of several data sets which could have been used to demonstrate more accurately the strength of the education-knowledge relationship.

I ransacked a few large-scale American studies that contained knowledge questions. In some instances the investigators had already fashioned summary scores; in other instances I did so by generating simple additive indexes. The results are instructive.

1. 1960—Almond and Verba, *Civic Culture*, national cross section, number of cabinet positions; r = .51.
2. 1961—Matthews and Prothro, southern participation study, representative cross section of the South, six questions on current history and institutions; r = .49.
3. 1964—SRC/CPS election study, national cross section, four questions regarding elections; r = .41.
4. 1965—Jennings and Niemi, political socialization study, national cross section of parents, six questions on institutions, current and past political history; r = .48.
5. 1967—Verba and Nie, *Participation in America*, national cross section, five questions on names of state and local officials; r = .36.
6. 1970—SRC/CPS election study, national cross section, four questions on institutions and laws; r = .41.
7. 1972—SRC/CPS election study, national cross section, five questions on institutions and congressional elections; r = .38.
8. 1973—Jennings and Niemi, socialization

study, national sample of parents, same six questions as 1965; r = .48.

9. 1973—Jennings and Niemi, socialization study, national sample of 1965 high school seniors, same six questions as above; r = .48.
10. 1974—Barnes and Kaase, et al., *Political Action,* national cross section, four open-ended questions yielding levels of ideological thinking index (à la Converse); r = .42.

Now these results represent a quantum jump over those reported by Merelman. Moreover, some of them rest on questions dealing with and occurring in conjunction with national elections, surely a circumstance that, due to the activation effect of elections, serves to depress the relationship. Nor have most investigators devoted much effort at all to developing more reliable and valid indicators of knowledge. In light of these considerations and the strong findings, coupled with a slight deflation of the generous estimates regarding status attainment, the distance between the putative effects of schooling on political knowledge and on status attainment is spectacularly far less than we were led to believe. Therefore, I must again question the empirical basis upon which Merelman erects his argument. It is correspondingly specious to make much of the invidious comparisons rendered by the causal model he offers, which struck me as strained in any case.

Because our disagreement was over the development of values rather than knowledge, this discussion is slightly tangential. However, Merelman chose to develop a point on these grounds, so it became my task to consider that argument. But the connection to the values question is reasonably clear. Whether through socialization, certification, or both, schooling does contributue to the development of democratic values, just as it does to political knowledge. (Incidentally, the selective critique by Jackman, 1978, has already been questioned—see Corbett, 1980.) The question remains, and here I really think Merelman does agree with me, how does schooling work its effects? Given universal education, much homogeneity in the educational system, the widespread mass culture, and the simultaneous presences of other causal agents, answering this question is no mean task. But it seems to me a much more promising approach than making erroneous assumptions about no effects and then trying to figure out why no effects exist.

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Reply

To the Editor:

I am pleased that M. Kent Jennings should wish to continue our dialogue about my "Democratic Politics and the Culture of American Education." Jennings is one of the more talented survey analysts of our discipline, and his work on political socialization and the politics of education has been invaluable. Unfortunately, however, as with all good survey researchers, there is an occasional tendency to assume that numbers convey single, unambiguous meanings, a tendency which sometimes makes the analyst mistake the trees for the forest.

Jennings still apparently misunderstands my reluctance to discuss adult political values and information. My essay was directed towards primary and secondary schooling; the "basic shape of schooling" does not even apply to universities, much less to later years. It therefore seems to me that data gathered from children and adolescents are most helpful in addressing the questions I raised. Even these data are flawed, of course, but they present fewer problems of interpretation than do data on adults, for the passage of years makes interpretation of adult data in terms of presumed childhood and adolescent influences difficult. Obviously, Jennings is correct in arguing that adult attitudes and information do constitute the "payoff" for what I am talking about in primary and secondary schools, but I operated on the assumption that you learn more about what concerns you by looking directly at *it,* rather than by looking at something problematically related to it at some years' remove.

But in his communication Jennings contends that even in pre-adult years "knowledge and commitment to democratic values and practices undergo enormous progression. . . ." He cites no evidence for this contention, so I looked myself at data on age-related information change from childhood to late adolescence. (And information *is* more changeable than democratic values, as Jennings admits.) The record is mixed. My own cross-sectional study in Los Angeles (1971) reported a roughly 30 percent improvement from 6th to 12th grade in political information (Merelman, 1971, p. 85). Andrain (1971, p. 77) reports more movement (approximately 35 percent from 5th to 8th grade). In the fullest recent study (Torney, Oppenheim, and Farnen, 1975, p. 107) American students from 10 to 18 years of age were tested, and those from 14 to 18 improved an average of 23 percent in cognitive items; incidentally, and not surprisingly given my argument, this improvement was the lowest of any country examined in this comparative study. A brief perusal of cohort data—obviously more reliable—in the recent National Assessment of Educational Progress summary (1979, p. 22) suggests that on retested items from 1969 to 1976 there was considerable variability in growth. For example, in 1971-72 17 percent of the 13-year-olds knew how the president was nominated, as opposed to 36 percent of the 17-year-olds in 1975-76, an improvement of only 19 percent. Other items showed more improvement, although there is a clear decline in what the 13-year-olds of 1975-76 knew in comparison with what the 13-year-olds of 1969-70 knew. Ditto with 17-year-olds.

All we can really do is to estimate, and let us do so generously. Suppose we put the figure at a 40-percent improvement rate in political information from 6th to 12th grade. Impressive? Only if we ignore the fact that much of this improvement is not the school's doing. At least three other factors must be considered: (1) genetic and social maturation of every child's intellectual abilities, a process which the school affects only marginally; (2) exposure to information about politics from sources other than the school; (3) improved guessing abilities and practice effects. Again let us estimate the effect of these factors. My feeling is that they explain perhaps 3/8 of the variance, namely 15 percent. This estimate leaves us with a "true" 25-percent increase in political information from 6th to 12th grade that is attributable to schooling. This figure, in turn, works out to approximately a 4-percent improvement per year. Given the fact that social studies classes take up about one hour each school day during the school year (an average of 240 days), what we have in effect is the equivalent of six 40-hour weeks of social studies teaching increasing political information at a 4-percent clip per annum. I'm not sure I'd call that a good return on investment; perhaps Jennings would.

Jennings observes near the end of his communication how difficult it is to make inferences about school effects, given the homogeneity of the public school system. I couldn't agree more; indeed, one of the things I attempted to do in my piece was to explain why the system tended toward homogeneity in the first place, namely, because of our desire to teach political equality in school. We are certainly under no such injunction to practice equality in mathematics teaching, by contrast, where we find that ability grouping takes place early and rapidly becomes associated with differential achievement (Rakow, Airasian, and Madaus, 1978, pp. 15-21). If it is unacceptable to differentiate by ability in social studies teaching (as opposed to mathematics), surely we should inquire into the cultural and political reasons why this is so, rather than simply to lament the difficulty of interpreting the problematic numbers that emerge from this practice.

Jennings comes closer to the mark in his comments on the adult-level data. Nevertheless, he too appears to read fairly selectively on the subject. Contrary to his assertion, Treiman (1977) does not report a "more modest" correlation of education and status than I reported. Indeed, on page 115 of his book his "purer" status measure yields a net regression coefficient of .58 with educational attainment, only trivially different from the .60 I originally reported. And, while the educational attainment-political information correlations Jennings remarks for adults are now at least in the same ball game as the status-attainment correlation, they are still lower. Nor are they entirely persuasive. For one thing, they include the effects of college, which, as Hyman, Wright, and Reed point out (1975, p. 107), importantly improves the ability to deal with complicated ideas. They also include the motivational consequences of differing levels of educational attainment. In a society which so values "education" (at least as measured in years, if not in quality) as our does, less-educated people may feel themselves slightly degraded and thus withdraw from political involvement—a motivational rather than a learning result of schooling—which could help explain at least some part of the correlations Jennings reports.

The problem of complicated ideas emerges most clearly in a comparative context. Thus, the Barnes, Kaase study, to which Jennings provided so much of importance, reports lower levels of ideological conceptualization in the U.S. than in the Netherlands, although each country presents a more or less equally difficult ideological continuum (the latter complicated by religion, the former by the absence of a strongly socialist mass party) and despite the fact that the U.S. sample contains over twice as many college-educated as does the Netherlands (19-8 percent) (Barnes, Kaase et al., 1978, pp. 246, 269). In the U.S., apparently, the more difficult the political conceptualization the poorer the return on education becomes, a result entirely consistent with my argument.

What I found disturbing in Jennings' letter is its absence of reference to what I consider the heart of my piece, namely, the last two sections on the basic shape of schooling and on the effects of American democratic pressures on that shape. I wish I could believe Jennings' assertion that our differences on these points are "not so serious," for then I could assume he accepts my argument. I have a feeling, however, that our differences are more serious than he admits, and, at the risk of becoming tedious, I should like to address the problem again briefly, lest the statistical trees Jennings discerns distract us from the forest. After all, the seriatim recitation of correlation coefficients based on indices or even scales of values and information, useful though it is, does not capture very well the texture of social and cultural adaptations to politics that I was trying to describe.

I contended that schooling is basically hierarchical, and I implied that schools function best in teaching complicated democratic ideas when they frankly admit the hierarchical structure of education, and thus are able to legitimize it with their own expertise. I argued that American schools were reluctant to proceed in this way, because they interpreted democratic values as an attack on their own expertise, on rank, on grading, and even on intellectual distinction. I then argued—in contrast to the hidden curriculum theorists—that schools *do* teach democratic values, but more in the form of tacit practices and "styles of behavior" than as complex cognitions.

Let me provide some examples. My 10-year-old daughter informs me that in her class they learn about "rights," such as the right of children not to be struck by other children, the right to share, and the right to take turns. She knows of no other rights. Too early, you say; give the schools time. Well, my 13-year-old tells me that in his classroom they don't talk much about rights in their social studies class; once somebody suggested discussing the rights of homosexuals, and everybody laughed nervously. His textbook lists the Bill of Rights and says that these rights are good things, but it never gets around to explaining why they are good, and why these rights are better than some others—or none at all. His teacher tells me that some of her colleagues do teach democratic theory, but the practice varies from teacher to teacher. She didn't seem too worried one way or the other, but she did worry whether students are "good" citizens in her classroom and whether or not they are nasty to each other in classroom discussions.

My daughter says that her principal gave a little talk a couple of weeks ago about democracy; he said, "We live in a democracy, and we should be graateful for our school and our principal." He wasn't explicit about how these things fitted together. I don't think he is a cynical man or an "authoritarian." True, his sentence is self-serving, but it also reflects puzzlement. He wants to believe in democracy and he wants to retain his authority, but my guess is that he is uneasy about the connection between his two goals. His teachers aren't much up to the theory of the thing either, and it is certainly not a good idea to teach ideas which could turn into ammunition students might fire against the school. If you're not sure you're any better at democratic theory than your students are, maybe it's better to ignore the subject, and instead emphasize "good school citizenship" and informal democratic practices.

Well, these are just anecdotes, unrepresentative statistically and perhaps biased in the interpretation and the telling. But I suspect cultures record themselves as surely in anecdotes as in tables of coefficients, and I think that by now Jennings and I have provided a choice along this line. I only ask the reader to talk to some kids before deciding which of us—if either—is right.

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Comment on Born (Vol. 73, September 1979, pp. 811-17)

To the Editor:

Richard Born, in a recent article in this *Review* (1979), purports to refute Erikson's finding (1972) of a relatively uniform increase in incumbency advantage. Born concludes (p. 816) that "No general, pro-incumbent trend has emerged; rather, incumbents in the aggregate have become more secure only because freshmen elected since 1966 have derived greater electoral rewards from their first term." Such a finding, if substantiated, has obvious implications for efforts to find the causes of improved incumbent performance. If we accept Born's results, we should be looking for something which would affect the *timing* of the bestowal of incumbency benefits, not something which would cause a quantum leap in the size of these benefits.

Born's Analysis

Born maintains that Morris Fiorina's recent work (1977) on the relationship between the growth of the bureaucracy and increased incumbency advantage suggests two readily testable hypotheses. First, the first-term incumbents (sophomores) are largely, if not solely, responsible for the recent increase in incumbent performance and second, that this changed performance on the part of first-term incumbents is a result of generational replacement. The second argument is clearly attributable to Fiorina. In an attempt to reconcile the gradual growth of the federal bureaucracy with the much more abrupt shift in incumbency advantage, Fiorina argues (1977, pp. 54-55) that the relatively large turnover in congressional membership in the mid-sixties resulted in an influx of new members more highly attuned to the potential benefits of a home style which emphasizes the role of the congressman as an intermediary between his constituents and the bureaucracy.

The pattern produced by generational replacement should be a sudden improvement in 1964-66 freshman performance. This improvement would be restricted to freshmen in that all veterans would be members of pre-1964 cohorts. In the succeeding election pairs, veteran performance should gradually improve as the veteran ranks continue to be swelled by post-1964 members who are replacing older-generation veterans who have left the House due to retirement or defeat. As Born himself reports, his analysis demonstrates the reverse, with veterans improving their performance suddenly in 1964-66, then declining in subsequent elections. Born nonetheless continues to maintain that his results support Fiorina's generational replacement hypothesis.

What Born has in fact constructed is a view of generational replacement quite distinct from that described by Fiorina. Born's new generation is distinguishable from the old generation only in regard to their improved performance in their first reelection attempts. In subsequent reelection bids, they derive no more advantage from their incumbency than do members of the old generation. Thus, Born's findings are at odds not only with past empirical findings, but also with the theoretical framework in which Born himself casts them.

Born's Measures

In order to test his hypothesis of sophomore culpability, Born analyzes data on congressional election returns for the period of 1952 to 1976. Born measures incumbency advantage in two distinct ways. First, in demonstrating the overall shift in incumbency advantage, he presents the relatively straightforward average incumbent electoral margin, adjusted for national party trends. He finds a mean increase of about 3 percentage points in incumbent safety in the period after 1965 compared to the pre-1965 period. Turning to the question of first-term versus veteran performance, he changes his measure to the average inter-election vote shift, adjusted for party trends. This change is unfortunate. While the former measure taps the phenomenon of interest fairly directly, the latter fails to measure this phenomenon and is biased toward a finding of limited veteran improvement.

Bias is introduced when inter-election vote shift is used as a means of comparing first-term performance with veteran performance. The measure is simply not comparable across these groups. For first-term incumbents, the degree to which they run ahead of their first election bid is a reasonable, and often-used, measure of incumbent advantage, as it is a comparison of an incumbent performance to a non-incumbent performance. The same is not true for veteran incumbents. Here the comparison is between two incumbent performances.

Perhaps the easiest way to illustrate this incomparability is to examine these two measures for a set of hypothetical election data. Table 1 contains the average electoral margins for candidates making their initial election bids (assumed to be 55 percent), their first reelection bids (assumed to be 57 percent), as well as for those incumbent candidates seeking reelection for at least the second time (assumed to be 65 percent). In addition to the average election returns for these groups in ten hypothetical elections, Table 1 contains the inter-election vote shifts that would be generated by the given margins. A uniform turnover rate of 20 percent per election, produced by the departure of veteran incumbents, is assumed.

It is also assumed that no change in the advantage of incumbency has taken place across this period; however, between elections four and five, the members of our legislature enacted legislation giving incumbents a 5-percent bonus to be added to their share of the vote in all future elections. Looking at the pre/post summary of average performance, the effect of this act on incumbent margins is quite clear: both first-term and veteran incumbents average five points higher in the elections following the act than they did in those preceding it. The resulting inter-election vote shifts, however, are not so straightforward and merit an explanation of their computation. The inter-election vote shift for first-term incumbents consists of nothing more than the difference between average first-term performance and average initial election margin. Thus, before the enactment of our five-point incumbent bonus, first-term re-election bids averaged two percentage points higher than initial elections, and with the addition of the five-point bonus, averaged seven points higher.

The computation of inter-election vote shift for veterans is somewhat more complicated. In moving from election one to election two, for example, 80 percent of the veterans in election two were also veterans in election one and thus averaged 62 percent in both elections, for an inter-election vote shift of zero. The remaining 20 percent of the veterans in election two were first-term incumbents in election one, and thus average 57 percent in election one and 62 percent in election two, for an inter-election vote shift of five points. The overall average veteran vote shift is only 1 percent. The movement from election four to election five produces a substantially different inter-election vote shift, due to the addition of the five-point bonus. Eighty percent of the veterans were veterans in both elections and are thus moving from 62 percent in election four to 67 percent in election five due to the bonus. The remaining 20 percent are doubly blessed. Their average first-term performance of 57 percent in election four is boosted five points as a result of their movement into the veteran category *and* an additional five points as a result of the bonus, yielding a total shift of ten points. The result is an overall veteran inter-election vote shift of six percentage points. By election six, however, the effects of the five-point bonus have been absorbed in higher levels of both first-term and veteran performance, with the result being that the remaining inter-election vote shifts reflect only the traditional one-point gain for the veteran category produced by turnover.

If one were to analyze the effects of the five-point bonus using Born's approach of comparing inter-election vote shifts, the implications would be rather startling. A plan which at the outset seemed eminently fair now appears rather severely biased in favor of first-term incumbents. First-term incumbents, as a group, appear to have received full advantage from the legislation (as evidenced by the five-point increase in their inter-election vote shift following the legislation). Veteran incumbents, on the other hand, exhibit an average improvement of only .8 percentage points in their inter-election vote shift in the post-legislation period. Clearly, a comparison of inter-election vote shifts across these two groups is not

Table 1. Hypothetical Election Results (Percent)

	Average Incumbent Vote			Inter-Election Shift	
Election	Initial Election	First-Term	Veteran	First-Term	Veteran
1	55.0	57.0	62.0	+2.0	+1.0
2	55.0	57.0	62.0	+2.0	+1.0
3	55.0	57.0	62.0	+2.0	+1.0
4	55.0	57.0	62.0	+2.0	+1.0
5	55.0	62.0	67.0	+7.0	+6.0
6	55.0	62.0	67.0	+7.0	+1.0
7	55.0	62.0	67.0	+7.0	+1.0
8	55.0	62.0	67.0	+7.0	+1.0
9	55.0	62.0	67.0	+7.0	+1.0
10	55.0	62.0	67.0	+7.0	+1.0
Pre-bonus		57.0	62.0	+2.0	+1.0
Post-bonus		62.0	67.0	+7.0	+1.8
Shift		+5.0	+5.0	+5.0	+ .8

Source: Compiled by the authors.

a very useful way to assess changes in electoral advantage.

Given the enormous simplification of electoral reality in the above example, the similarity of the results to those presented by Born is striking. Born finds a durable increase in inter-election vote shift for sophomores after 1965, but finds only a momentary surge for veterans. As our hypothetical example has made clear, *such a result is by no means incompatible with a uniform increase in incumbency advantage across tenure levels.* The error is not in the inter-election vote shift figures themselves, but lies instead in Born's treatment of sophomore and veteran vote shifts as comparable measures of incumbency advantage. In sum, Born's findings do not provide convincing support for either Fiorina's notions or for Born's own distinctive view of generational replacement.

JOHN R. ALFORD
JOHN R. HIBBING

Oakland University

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Reply

TO THE EDITOR:

John Alford and John Hibbing have misrepresented my findings in a number of important respects. Their central allegation is that I have not substantiated the hypothesis that generational replacement is responsible for the dramatic rise in the safety of House incumbents since the mid-1960s. Specifically, I am accused of the following two transgressions: failing to recognize that the pattern of inter-election shifts presented in my Table 2 (p. 814) could have been produced by a uniform increase in the electoral advantage of incumbency across seniority levels, and controverting myself by reporting a decline in veteran performance after the putatively safer new generation members have been added to their ranks.

The bulk of Alford's and Hibbing's efforts consist of an eminently sensible "simulation" of what we could expect to see if all incumbents regardless of seniority were to receive simultaneously the same one-time electoral boost. The inter-election changes associated with freshmen and veterans, of course, would abruptly jump by like amounts in that election year. Following this uniform surge, we would subsequently find that biennial shifts between the initial non-incumbent year of election and the first reelection bid would continue to reflect the enhanced value of running as an incumbent, while veterans—experiencing the new margin of safety in both years of the election pair—would see their inter-election shifts settle back to the levels typifying the pre-surge period. These results, assert Alford and Hibbing, parallel those turned up in my original study.

Contrary to Alford's and Hibbing's claim, however, my analysis turns up no such pattern. As revealed in my Table 2 (1979, p. 814), the 1966-68 takeoff point for freshman inter-election gains is one which certainly does not extend to veteran members; the freshman shift increases to .072, at least .037 greater than any recorded previously, while that for veterans actually *decreases* by .007. These opposing trends thus show a sudden large electoral boost confined to first-termers alone, rather than any uniform rise for incumbents generally. Additionally, Alford and Hibbing chose to ignore the fact mentioned in my article (p. 814) that there is virtually no correlation between changes in "sophomore surge" values and inter-election shifts for veterans over the 1966-76 period, once again demonstrating that the margins of these two groups are being propelled by different forces. Thus, their "simulated" election results remain exactly that and nothing more.

Alford and Hibbing further misrepresent my article, as well as disregard their own later statements, when they argue that I report an enervation of veteran strength as new-generation freshmen replace departing old-generation members. But as they themselves subsequently recognize, my analysis of veteran inter-election shifts focuses solely on how the performance of incumbents at t_1 *compares* with that at t_2, rather than on the electoral effect of generational replacement. This analysis, then, certainly was never intended to reveal whether the larger first-term vote gains of members elected to Congress since 1966 have been translated into increasing veteran safety. However, Alford and Hibbing neglect to mention that this question is in fact addressed and answered affirmatively in the next section of the article (1979, pp. 814-16),, where it is plainly demonstrated that the replacement effect is substantially stronger during the new generation period than before.

Alford and Hibbing, then, have attacked a set of straw men, rather than the actual contents of the study. Fiorina's hypothesis that a generational transformation in electoral proficiency has produced the greater safety of House incumbents since the mid-1960s is not imperiled by this communication. However, the particular causes of this new-generation safety—whether arising from more intensive cultivation of their districts or elsewhere—still remain to be isolated.

RICHARD BORN

Vassar College

Comment on Ransom (Vol. 74, March 1980, pp. 141-48)

TO THE EDITOR:

In his review essay, "Being Intelligent About Secret Intelligence Agencies," Harry Howe Ransom wrote:

> Until disproved, the following hypotheses, partly intuitive, appear to represent reality: intelligence systems tend to report what they think the political leadership wants to hear. . . (p. 147).

The view that intelligence caters to the preconceptions of the political leadership seems widespread in both academic and journalistic circles. Intelligence practitioners know it to be untrue.

One factor does, unavoidably, exert pressure toward conformity on the individual intelligence analyst. That is the need to defend and argue cogently for whatever position one takes. If an analyst takes a position that is controversial or counter to the conventional wisdom, that analyst will have to do more. He or she will have to do more research, defend the position in greater detail, and the process of review, approval, and coordination will be longer and slower. That is obviously the way it should be; analysts should not be able to rip off an unconventional analysis and send it to the White House without the most careful review. But when analysts are working under extreme time pressure, as most analysts are, it means they have to make a decision whether or not they have the time to do what is required to defend a novel or minority view.

Obviously, this does exert some pressure toward conformity, but several points are important in interpreting the significance of this fact. This is not necessarily conformity to the views of the political leadership. It is, first of all, pressure toward agreement with analysts in competing centers of analysis, e.g., for the CIA analyst, the tacit or active support of colleagues in the State Department and Defense Intelligence Agency who might otherwise seek to undermine one's analysis.

Secondly, it is conformity with the preconceptions of one's own most immediate supervisors. Pressures to reflect one's supervisor's views or to support departmental interests differ in different agencies. They are perhaps strongest in the Defense Department and almost certainly weakest in CIA, where a long tradition of analytical independence supports the individual analyst's freedom to express unconventional or unpopular views as long as they are well documented. Analysis biased in support of long-standing departmental interests is not the same as analysis that tells the political leadership what it wants to hear; students of bureaucratic politics are well aware that departmental interests frequently lead to intelligence assessments that diverge from what the political leadership wants to be told.

Analysts are subject to many countervailing pressures. The need for self-esteem and pride in one's work is, in most cases, at least as strong as pressures for conformity. In 28 years' service with CIA, I found that analysts generally did not hesitate to advance a novel or minority view even though it might make a few waves or cause them more work. Disagreement is, in fact, endemic to the intelligence community, and is far more prevalent than conformity. Most analysts enjoy challenging the superficial or conventional wisdom of the policy makers or other analysts, and they look for opportunities to do so as this is how they can be most useful. Analysts gain the greatest job satisfaction from presenting and defending their own views under precisely those circumstances when, according to Ransom's hypothesis, they should be catering to the preconceptions of the political leadership.

RICHARDS J. HEUER, JR.

Naval Postgraduate School

Reply

TO THE EDITOR:

Most generalized observations of political behavior fail to encompass the complexity of reality. The generalization to which Richards J. Heuer objects comes from my diligent reading of nonsecret sources over the past quarter-century. From this evidence and from conversations with intelligence professionals, I intuitively suggest the hypothesis that intelligence agencies, even if unconsciously, tend to report what they think decision makers want to hear. Heuer's experience is different from mine and he reaches a contrary conclusion. He has had the advantage, and disadvantage, of the inside view of a secret agency. I respect his observations.

Yet how does one explain the monumental intelligence failures beginning with Pearl Harbor? How does one interpret the shock in Washington at the first Soviet atomic bomb explosion in 1949; the dual surprises in the Korean War—the initial attack and later Chinese intervention; the Bay of Pigs failure; the Cuban missile surprise; the colossal failures in Vietnam, early and late; the frequent failures in forecasting Soviet strategic military power; and the more recent intelligence failures regarding Iran and Afghanistan? Surely an information pathology is at work. Admittedly, the concept of "intelligence failure" presumes a theory of intelligence that remains underdeveloped.

Failure can occur at any number of points in a complex series of steps, from the setting of requirements to the use of intelligence by decision makers. But in the examples cited, the decision makers in each instance were misinformed and in most cases were pleased to hear the intelligence estimates given them.

The inference I draw from these and other examples is that tendencies exist in bureaucratic structures that lead to misinformation or worse. I agree with Mr. Heuer that such tendencies may be less pronounced in the CIA than in department intelligence reports. Certainly such an objectivity was an important reason for creating the CIA in 1947. The practice in recent times of appointing CIA directors anew, with each new incoming presidency, however, invites a dangerous politicization of central intelligence, exacerbating the problem of objective reporting.

The record clearly indicates that major intelligence estimates have been tragically wrong about many major events over the past 30 years. Doubtless numerous variables are involved in these failures. Pressures on, and tendencies of, intelligence professionals to bring good news to political leaders appear to have been important causes of such failures.

HARRY HOWE RANSOM

Vanderbilt University

Reply to Schrodt
(Vol. 74, December 1980, p. 1048)

TO THE EDITOR:

Philip Schrodt has two complaints: one hits the target and the other misses the mark by a wide margin.

The target-hitting complaint is, of course, that one cannot derive

$$\frac{dx}{dt} = ky - ax + g \text{ from } \frac{d^2x}{dt^2} = -a.$$

My contrary claim was, I grant, worse than a fault; it was a mistake and one which I shall now put right. Replace

$$\frac{d^2x}{dt^2} = -a \text{ with } \frac{d^2x}{dt^2} = k\frac{dy}{dt} - a\frac{dx}{dt} \text{ from which}$$

$$\frac{dx}{dt} = ky - ax + g$$

can be derived using, *pace* Mr. Schrodt, freshman calculus. Even Schrodt's target-hitting arrow, however, falls short of a bull's-eye since the *whole point* of the discussion of that section (pp. 108-09) was to argue that political scientists who think of "explanation" in international politics as conforming to a highly idealized hypothetico-deductive model misconceive both the nature of explanation—which lies outside positivist science—and international studies—which lies outside positivist conceptions of it. Given the validity of the corrected notation and Schrodt's silence about my general point, I shall assume that my argument holds good. Indeed, the notion that a "theory" of international politics ought to conform to some single criterion of rationality is, on my view, a self-protective piece of petty legislation which precludes the possibility of counterexamples by stipulating that there are none. This point is not new but in light of the undeserved popularity of the H-D Model among some political scientists, it bears repeating.

As to Schrodt's second point concerning my having attributed to Richardson a deterministic *Weltanschauung*, the arrow is crooked. I never said that Richardson was a determinist. What I said was that he had committed himself, whether he realized it or not, to a version of scientific determinism, and this still seems right to me. From the fact that Richardson "sold his physics books" to learn about statistical proof from Karl Pearson it does not follow that he was not committed to a version of scientific determinism. As determinists have long argued, mere indeterminacy or randomness goes no way toward providing the possibility of free will. To have free will requires that an agent could have acted otherwise.

Let me briefly reconstruct my argument at pp. 112-13 of the article in a more cryptic form. Suppose, as the quotation from Richardson suggests, there are only two alternatives for nations—namely, *either* they follow fixed traditions mechanically *or* they stop and think. Then we have to choose between the second conjunct whose truth-conditions can never, so far as we can ever tell, be decided upon (leaving to one side the far-fetched), and the first conjunct. This is no choice at all except in a rhetorical way and that leaves the first conjunct. But what does it mean to say that nations follow fixed traditions mechanically, etc.? It appears to mean that laws of human behavior specifying necessary and sufficient conditions would suffice to determine the choices of political leaders such that they could not but act as they do. It is hard to see how Richardson could escape, *malgré lui,* a commitment to scientific determinism. Of course, I am aware that in the *Statistics of Deadly Quarrels* Richardson "discovered" that the typical pattern of such conflicts turned out to be distressingly random, but it does not follow that he did not commit himself to the search for laws governing human behavior. Support for this claim may be found in Rapoport's observation (1968, p. 516) that Richardson "hoped" that "the [sic] laws governing human violence would emerge," i.e., that sufficient antecedent conditions could be found which would give the causes of human behavior. Such laws would present a sufficient cause for an effect; the effect could not but follow; that that, in my terms, just is scientific determinism.

Despite Richardson's discovery of the purely random relationship between outbreaks of war and returns to peace, many of his followers have continued the search for the laws of human violence without even raising the question of whether Richardson's commitment to determinism is coherent. In my view it is not, but I cannot go into that here. In my article I thought it sufficient to smoke out this issue from the mathematical notation in which it has long been embedded and to call the bluff of those mathematical imperialists who think that adding a few more variables will yield determinant laws of human behavior. In general, whether I succeeded or not is unimportant, for the key point is to uncover the many and varied ways in which political scientists repeat the mistakes of philosophers and thinkers they refuse to read.

Why bother with these errant eristics? One reason for it has to do with what I take to be the intimate tie between the possibility of an essentially Cartesian conception of knowledge, glossed as logic and mathematics, and the "sad state" of moral, legal and political theory in the contemporary study of international politics. Once we see that the claims advanced on behalf of the former are quite unjustified—or, at best, as Hegel put it in arguing against Kant's Categories, one-sided, empty formalism—we shall, I hope, be better disposed to a radical shift toward the virtually uncharted region of substantive moral and political theory in international studies.

Let us, then, drop this cant about misinterpreting Richardson, Marx, Spegele, and Schrodt, and recognize the truth in Mathew Arnold's calculation that fully three-fourths of human life is occupied by moral concerns, and that international studies can and ought to carve out a significant role for itself in explaining our often tortuous moral decisions about peace and the avoidance of the horrors of war:

> Nor milder is the general lot
> Because our spirits have forgot
> In action's dizzying eddy whirled
> The something that infects the world.
>
> —Matthew Arnold, "Resignation"

ROGER D. SPEGELE

University of Nairobi

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ON BOOK REVIEWS

Comment on Campbell's Review (Vol. 74, September 1980, pp. 835-36)

TO THE EDITOR:

John C. Campbell's recent review of my *Suburban Tokyo* contains some errors, distortions, and misrepresentations that warrant the attention of *APSR* readers.

His most egregious error appears when he states that "these data are aggregated only at the city level" (p. 836). This statement is simply false, because tables in the book present data for many different levels in addition to the city level. The review implies that sub-city data do not appear owing to an oversight on the author's part. In fact, such results do not appear because they are not available: Japan's election law precludes tabulation of voting returns at the precinct level.

One distortion arises because Campbell believes that I try to account for political differences between two suburbs with a "hypothesis" concerning divergent migration patterns. Unfortunately, arguments in the book are merely generalizations based on limited evidence, not hypotheses reached through deductive analysis. Moreover, although I do stress the importance of migration patterns, I also analyze at least five other causes for the differences between the two cities, stemming from urban development, mayoral constituencies, party organization, union behavior, and neighborhood associations. Since Campbell reveals an understanding of only one of these six explanations, readers will readily appreciate why he did not find my arguments "convincing."

Misrepresentations occur owing to a mistaken understanding of the mode of discourse in history. Campbell states that I claim to write "as historian, anthropologist, sociologist, and political scientist" (p. 835). The book states that I "selectively appropriated the concepts, techniques, and methods of several disciplines" but that I write "as an historian" (p. xi). This is a significant distinction, because writing as an historian (by contrast with thinking like a physicist) imposes a style that is both analytic and ideographic. The subtleties of discourse in history are conceived to represent faithfully the "amorphous characteristics" over which Campbell despairs. In their perversity, historians think that reality itself is amorphous, or, to put it another way, we feel that man does not live by theory alone.

GARY D. ALLINSON

University of Pittsburgh

Comment on Fowler's Review (Vol. 74, December 1980, p. 1092)

TO THE EDITOR:

Linda Fowler's generally accurate review of *Parties, Interest Groups and Campaign Finance Laws,* which I edited for the American Enterprise Institute, said that the "proceedings are dominated by practitioners of various political backgrounds and ideological views," without naming the participants. I thought potential readers might want to know that although the book included some papers by practitioners, three of the four panels were based on papers by political scientists —Edwin M. Epstein, Ruth Jones, Xandra Kayden, Herbert Alexander, and Khayyam Z. Paltiel, as well as myself. The comments on the papers did come mostly from practitioners, but commentators David Adamany and George Agree are well known for their writings on the subject.

MICHAEL J. MALBIN

American Enterprise Institute

EDITORIAL NOTE

A second research frontier article appears in this issue. Dorothy Nelkin, of Cornell University, identifies dimensions of the accident at Three Mile Island that are proper subjects of social science research. David Sills of the Social Science Research Council was asked to comment on her article. A third article will appear in the September 1981 issue. The topic is the political representation of women, as analyzed by Virginia Sapiro, University of Wisconsin.

The following articles have tentatively been scheduled to appear in the June, 1981, issue:

Harvey C. Mansfield, Jr., Harvard University, "Machiavelli's Political Science"

Robert Axelrod, University of Michigan, "The Evolution of Cooperation among Egoists"

Trudi C. Miller, National Science Foundation, "Political and Mathematical Perspectives on Educational Equity"

Robert M. Stein, Rice University, "The Allocation of Federal Aid Monies: The Synthesis of Demand- and Supply-Side Explanations"

Richard L. Engstrom and Michael D. McDonald, University of New Orleans, "The Election of Blacks to City Councils' Clarifying the Impact of Electoral Arrangements on the Seats/Population Relationship"

C. Neal Tate, North Texas State University, "Personal Attribute Models of the Voting Behavior of U.S. Supreme Court Justices: Liberalism in Civil Liberties and Economics Decisions, 1946-1978"

Robert H. Dorff, North Carolina State University, and Jürg Steiner, University of North Carolina-Chapel Hill, "Political Decision Making in Face-to-Face Groups: Theory, Methods, and an Empirical Application to Switzerland"

Robert H. Salisbury and Kenneth A. Shepsle, Washington University, "Congressional Staff Turnover and the Ties-that-Bind"

Barbara Sinclair, University of California-Riverside, "The Speaker's Task Force in the Post-Reform House of Representatives"

Joseph Cooper, Rice University, and David Brady, University of Houston, "Institutional Context and Leadership Style: The House from Cannon to Rayburn"

Donald S. Collat, Yale Law School, Stanley Kelley, Jr., Princeton University, and Ronald Rogowski, Duke University and University of California-Los Angeles, "The End Game in Presidential Nominations"

James H. Kuklinski and Darrell M. West, Indiana University, "Economic Expectations and

Voting Behavior in United States House and Senate Elections"

D. Roderick Kiewiet, California Institute of Technology, "Policy-Oriented Voting in Response to Economic Issues"

Errata

The figure published in Roy Pierce's review (September 1980, pp. 871-72) of *Conflict and Consensus in France,* edited by Vincent Wright, was given incorrect source information. The source of the data on which the figure is based is *Le Nouvel Observateur,* 24 April 1978, cited in Wright's book on p. 39. We apologize for this error.

* * *

Because of an unfortunate layout error, an incorrect footnote appears in Yoav Peled's article, "Rousseau's Inhibited Radicalism: An Analysis of His Political Thought in Light of His Economic Ideas" (December, 1980). On p. 1040, footnote 5 should read as follows:

[5] A growing population is, for Rousseau, the only sure sign, and an inevitable result, of good government. *Poland,* p. 230; *Corsica,* p. 83; *Emile,* p. 432.

BOOK REVIEWS

Political Theory and Methodology

Gramsci and the State. By Christine Buci-Glucksmann; trans. by David Fernbach. (Atlantic Highlands, N.J.: Humanities Press, 1980. Pp. xiv + 470. $35.00.)

Along with Lukács and Korsch, Antonio Gramsci is considered by many a founder of Hegelian Marxism. By perceiving history as a living totality that unfolds dialectically, and modern capitalism as a momentary battleground for opposing economic classes, Hegelian Marxists emphasize human cognition and action. Workers, punished unmercifully by capitalism's exploitative conditions, will subjectively experience the revolutionary role history has allotted them, seize the productive apparatus, and create socialist justice. While the immediate cause is a worsening of the material contradiction between means and relationships of production, this entire episode is merely one fragment of an abstract universal process. Hence revolutionary action expresses the confluence of objective material factors and a subjective awareness of what it all means. The former supplies propitious revolutionary conditions; the latter initiates action. Marxists must therefore cultivate an apposite working-class consciousness, rejecting orthodox economism and recognizing subjectivity as an irreducible historical reality.

Hegelian Marxism attracts Westerners disillusioned with bourgeois rhetoric but also dismayed by the mechanical anti-subjectivism of materialist Marxism. Extraordinarily intelligent and principled, Gramsci is a revolutionary who also uninhibitedly condemns the Second International and lauds "progressive" liberal freedoms. Predictably, his popularity in the West is growing. Christine Buci-Glucksmann's *Gramsci and the State* tries to extract Gramsci's Marxist reputation from this suffocating embrace. The principal hypothesis fits Gramsci squarely in the orthodox materialist tradition. Apparent disparities between his emphasis on willful action and orthodoxy's passive materialism are resolved in Gramsci's expanded notion of the state, which simultaneously represents reflective action (praxis) and history's objective material laws (theory).

Buci-Glucksmann convincingly documents the evolution of Gramsci's "integral state" from its origins in 1918-20, to its mature exposition, after 1926, in the *Prison Notebooks*. For orthodox Marxism, the state is a political apparatus by which one class coerces another, a cultural tool of oppression automatically ceded to the dominant economic class. Gramsci's state is more dynamic. Still a political conduit for class rule, it is also the means for constituting civil society as an organic unity embracing all segments of the populace. The state's power, therefore, comes from its coercive apparatus as well as the entire complex of theoretical and practical activities used by a ruling class to rationalize its dominance and win the masses' active consent. With these "hegemonic" apparatuses—books, journals, schools, pulpits, air waves, museums, etc.—a state creates an "historic bloc," an organic unity between leaders and followers. Intellectuals perform the vital function of legitimizing culture, including extant class relationships. Hence, a proletarian "war of position" is needed, where working-class intellectuals create the subjective conditions required for the revolutionary transformation of one historical bloc to another. In brief, the emerging proletarian state reflects objective class antagonisms *and* actively, cognitively smashes elite hegemony.

This makes perfect sense from a Hegelian Marxist point of view, emphasizing the irreducible significance of the superstructure. Buci-Glucksmann, however, argues that Gramsci is a materialist. The integral state, as revolutionary praxis, dialectically expresses an objective material reality which in the last, non-immediate instance is determining. Subjectivity is saved, but not at the expense of materialism.

Buci-Glucksmann assiduously traces Gramsci's lifelong admiration for Lenin's activist spirit and revolutionary tactics. However, the controversial hypotheses—Lenin's saving materialism from an economistic death, and Gramsci's adoption of Lenin's subjectively inspired materialism—are undeveloped. The first is based on Lenin's rambling *Philosophical Notebooks*, where he inconclusively seeks to justify political activism theoretically. Buci-Glucksmann conveniently ignores Lenin's single published work on epistemology, *Materialism and Empirio-Criticism*, which is rigorously orthodox. She also quietly assumes that Lenin's revised materialism is philosophically compelling —which is problematic. Lenin's *Notebooks* may well have served not to alter materialism substantively, but to guarantee a party activist the freedom to do whatever he or she wants without violating history's objective material laws. In this case, the connection between Lenin and Stalin is, notwithstanding the author's disclaimers, irrefutable. The second issue, Gramsci's epistemological debt to Lenin, is also unproven. Although both were flexible party activists, the long-run philosophical implications of Lenin's materialism—i.e., contemporary Marxism-Leninism—vitiate Gramsci's concern for reflective praxis. Since, in addition, Gramsci openly criticized *Materialism and Empirio-Criticism,* never even read the *Philosophical Notebooks,* traced his own philosophical heritage in Hegel and Croce, and continually spiced his commentary with Hegelianisms, it is unrealistic to presume Gramsci was, philosophically, a Leninist.

Buci-Glucksmann avoids these unresolved theoretical problems by arguing that Marxism is antiphilosophical. This thesis arrogantly ignores the enormous body of significant Marxist literature devoted to facilitating rebellion by explicating Marxist philosophy, i.e., explaining to intelligent people why revolution is "correct," capitalism "oppressive," and socialism "liberating"—a task Marx himself left unfinished. It is also queer to read an Althusserian structuralist who denies the relevance of philosophy in order to save "authentic" Leninist materialism.

Gramsci and the State should be read very carefully. It lucidly outlines Gramsci's innovative theory of the state. It also dogmatically squeezes Gramsci into the author's own self-serving theoretical niche. Ironically, it mirrors the best and worst of bourgeois social science.

ROBERT A. GORMAN

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Merleau-Ponty and Marxism: From Terror to Reform. By Barry Cooper. (Buffalo, N.Y.: University of Toronto Press, 1979. Pp. xv + 223. $17.50.)

History and Human Existence: From Marx to Merleau-Ponty. By James Miller. (Berkeley: University of California Press, 1979. Pp. xi + 287. $17.50.)

The failure of Marxism as a mode of historical explanation has not detracted from its status as an existential mode. It continues to motivate and justify the principle of revolt against bourgeois life. Sartre, Malraux, and perhaps most interestingly, Merleau-Ponty, continued to share the moral imperative to revolt after they had rejected the empirical necessity of revolution. James Miller has written a book arguing that men like Merleau-Ponty are the proper heirs of the Marxist spirit, while Barry Cooper has written one both explaining Merleau-Ponty and showing the extreme tension within his thought.

James Miller writes in his introduction:

> My real reason for taking up the question of subjectivity with regard to Marxism (rather than some other social theory) lies in the irrevocable commitment of revolutionary Marxism to abolish all social orders in which human beings are oppressed and degraded. That commitment seems to me the proper place for contemporary social thought to begin—even if it ends by discarding Marxian theory (p. 9).

If one wishes to abolish oppression, then there are two reasons to abandon Marx. First, Marx seems to have based his argument for the abolition of oppression on historical necessity rather than on moral sentiment. History has thus far failed to abolish suffering and shows little inclination to do so autonomously. One might wish to compel history to do one's bidding, but one first needs a theory of agency other than that supplied by Marx. Second, one must abandon Marx because those regimes created in his name have been at least as oppressive and degrading as those which they claim to stand against.

It is thus necessary to develop a theory which is revolutionary in intent and which leaves room for voluntary action in history and concern for the individual as history's end. James Miller's book is an attempt to discover such a concern embedded in Marx, and to show how this concern culminates in contemporary varieties of existential Marxism, and in particular, in the writings of Merleau-Ponty.

Miller develops Marx's concern for autonomy by introducing the notion of "individuation." Miller distinguishes individuation from individualism. He means by individuation, "the process whereby human beings become distinctive, autonomous and self-conscious agents, each capable of purposefully reshaping the natural world and of independently evaluating moral claims" (p. 5). In a way, this is appropriate, inasmuch as Miller's reworking of Marxism seems to make intellectuals the new revolutionary subject.

Now Miller is certainly correct in seeing the creation of human autonomy as an end of Marx's project. However, his notion that the independent evaluation of moral claims is the essence of autonomy for Marx is more problematic. Marx is not Kant. First, moral evaluation is never individual. It is historical and social: morality is object, unfree. In the trans-historicity of communism, morality is abolished as is the ruthless criticism of everything existing. Both historical heteronomy and the trans-historical abolition of the moral problematic undermine Miller's argument about the nature of Marx's individuation.

Miller obviously knows this. He therefore argues that this theme of Marxism finds its fullest expression in the sort of existential Marxism of which Merleau-Ponty was perhaps the finest specimen (p. 198). In his political thought, Merleau-Ponty was archtypical of the contemporary radical intellectual. Committed to the redemption of mankind, his life was a series of disappointments about various movements of the left, without losing hope that something would turn up.

Marx's historical prophecy had proved faulty and the regimes created in his name proved inhuman. One might abandon the principle of the revolutionary redemption of mankind, but then the intellectual would be nothing more than the purveyor of useful gadgets. Existentialism from this standpoint meant an existential commitment to redemption and a revulsion at bourgeois life. Miller is indeed correct in arguing that Marxism culminates in the sort of thing Merleau-Ponty and Sartre represented, that it is necessary to develop a contingent argument for revolution in our time. But this culmination masks a failure. It undermines the scientific basis of Marxism. This changes Marxism from a universal necessity to merely one value judgment among many in a world where all values are relative. Furthermore, philosophy, which Marx sought to make identical with the proletariat—that is, with humanity—becomes once more the specialized purview of the intellectual, in the hothouse world of the Paris cafe.

Barry Cooper tries to deal with this problem by focusing on Merleau-Ponty alone. As Cooper points out, Merleau-Ponty's Marxism was a peculiarly disjointed affair. Merleau-Ponty, seeing the contingency of history, had rejected Marx's determinism. At the same time, he refused to abandon Marx altogether. Merleau-Ponty possessed what Cooper calls "a mood of hope" (p. 55). Merleau-Ponty faced the opposition between contingency and hope by trying to escape the political reality of the postwar world: the cold war between the hopeless United States and the hope-filled but brutal Soviet Union. Merleau-Ponty sought a middle course between bankrupt liberalism and brutal communism. Cooper asserts that the New Left and Eurocommunism represent that middle way (p. 168).

Both Cooper and Miller seem in sympathy with Merleau-Ponty's radicalism. Cooper, however, seems more sensitive to the troubling logic of Merleau-Ponty's formulations. Marx had refuted liberalism; at the same time, the practical consequences of Marx's thought were unacceptable. Merleau-Ponty's solution was a new political practice, committed in principle to Marx's project. However, this solution was excessively modest, if Eurocommunism is an example. Another possibility, more radical, eluded him—that the practical problem was really a philosophic one: to formulate an escape from the dialectic of modernity itself. Modernity, like Miller, is committed to abolishing all unfreedom. Each attempt has culminated in catastrophe. Modern radicalism searches for technical mistakes in its blueprints, refusing to suspect the project itself. Perhaps a truly radical suspicion ought to be raised: that domination, the rule of some over others, rather than being the quintessence of the inhumane, is the essence of humanity—that ruling and being ruled is what being human is all about.

GEORGE FRIEDMAN

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Perché la violenza?: Una interpretazione filosofica. By Sergio Cotta. (L'Aquila: L.U. Japadre Editore, 1978. Pp. 155. L. 3,500, paper.)

"It is not in the power of an individual to escape from an epoch of violence, much less to put an end to it. But to attempt to overcome it is a fundamental personal responsibility for anyone who has respect for man and thus for himself." So begins the magisterial study of violence by Sergio Cotta, professor of philosophy of law at Rome University and Italy's leading political theorist.

Many scholars write about the practical relevance of political theory, but few have done as much to illustrate that relevance than has Sergio Cotta. In numerous books and articles, he brings the insights of the classics of political theory alive in such a way that they contribute directly to understanding present-day practical problems.

A few years ago, in his book ironically entitled *L'uomo tolemaico,* Cotta advanced the thesis that it is *modern* culture which is "earth-centered" compared to medieval culture which was centered on transcendent reality. Cotta's study should be read in conjunction with Eric Voegelin's *From Enlightenment to Revolution* (Duke, 1975) and Hans Sedlmayr's *Art in Crisis: The Lost Center* (Regnery, 1958), for detailed documentation concerning the enormity of modernity's break with the past.

Much of our political and social science today demonstrates ignorance of this spiritual revolution and of its consequences. As a result, broad sectors of contemporary political science accept so narrow a paradigm of "politics" that even the phenomenon of twentieth-century totalitarianism has been incomprehensible to them. Those who follow the comfortable political science of the present day lack the analytical tools and historical knowledge even to begin to understand a political phenomenon as central to our time as terrorism. Cotta's books should be read by all political scientists so that with his help we all might begin to remedy our ignorance.

In part, Cotta's book might be described as a phenomenology of violence. He takes issue both with those writers who have defined violence too broadly and those who have defined it too narrowly. The difficulty with the first group is that if all politics and all history are seen in terms of violence (*tutto è la violenza*), then the very distinction between violent and nonviolent action becomes meaningless. What Cotta calls the "apocalyptic" theory of violence holds that all institutions of modern "repressive" society are manifestations of violence, for they "violate" the humanity of man by manipulating, indoctrinating, and regimenting him, even if the means used are "peaceful." (Ironically, the enlightenment faith that education would overcome the "violent" proclivities of "primitive" man is itself ridiculed by the view that modern education itself is a form of "violence" against the person.) For Cotta, the radical position is absurd, for it abolishes every distinction between public force and private violence as well as the distinction between violence and nonviolence. Thus, in the radical revolutionary literature even "nonviolence" is seen as a deliberate tactic for provoking violence on the part of the established authorities.

While the "gurus" (Cotta's term) of contemporary radical revolutionary thought are mostly nonviolent, peaceful people themselves, the connection between their doctrines and the actions of the Red Brigades and the Baader-Meinhof gang is a direct one. Indeed, Cotta thinks that the current widespread exaltation of violence and its equation with "morality" is an unprecedented event in the history of thought. Hitherto, even revolutionaries such as Marx, Lenin, and Mao have endorsed violence only as a grim necessity to achieve the victory of the revolution ("political power stems from the barrel of a gun"). They have viewed it coldly and calculatingly as a *means* to achieve an *end*. The new "revolutionaries," on the other hand, glorify violence as itself the end. The violent revolution is to be "permanent."

While exposing the dream-constructions" of the radical revolutionaries to the cold light of reality, Cotta refuses to fall over into another danger and define violence too narrowly as exclusively the work of "private," unauthorized groups. The distinction between legitimate "force" (by constituted public authority) and illegitimate "violence" (issuing from private groups) obscures the truth that even constitutionally established public authorities can fall into patterns of activity that violate the dignity of the person. Ritualistic cries of "law and order" may themselves contribute to a climate of violence.

What is violence, then, according to Cotta? Using the terminology of personalist philosophy, he concludes that violence is "an unregulated, nondialogic, and non-coexistential counter-activity." It is noteworthy that violence can be described only in terms of negations, for it occurs in the sphere of "opaqueness, refusal to care for the other, and arbitrary and absolute constriction."

Today's apostles of violence view identifiable individuals with contempt. The glorification of violence represents the ultimate manifestation of closure and reification. By insisting that violence be characterized only in terms of negations, many political and social scientists may be expected to attack Cotta for having abandoned the "fact-value dichotomy" and the scholar's commitment to "ethical neutrality." Cotta himself seems to be too busy following the trail of political reality to notice that he violated the taboos of today's positivistic social science, however.

Sergio Cotta brings this masterfully written volume to a conclusion in the last two chapters by considering the question posed in the title: "Why is there violence?" Here he does not mean to ask why is there violence as such, but rather why is there the "exaltation" of violence in so much of the contemporary world. It is this exaltation of violence that constitutes what is truly novel in the contemporary situation.

There is no way in a brief review to do justice to the complexity and brilliance of these final two chapters. Cotta ranges over such topics as the influence of Rousseau's voluntarism on democratic doctrine, the advent of industrial, technological society, the "activist historicism" of Marx, the overthrow of what he calls the "juridical measure" by a legal philosophy that reduces law to the expression of a particular will, and "absolute subjectivism" in twentieth-century philosphy. Taken individually, these topics are familiar to political theorists, but Cotta's philosophical framework and flair for nuance bring a new dimension and a refreshing lucidity to their discussion.

Perché la violenza? is one of the major works of political theory published in the last decade. It would be nothing short of a scandal if it is not immediately translated into English and widely discussed here in the United States.

DANTE GERMINO

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The Correspondence of John Locke: Vol. 5, **Letters 1702-2198.** Edited by E. S. de Beer. (New York: Oxford University Press, 1980. Pp. vii + 800. $89.00.)

Each successive volume of Locke's correspondence reveals fresh vistas on the activities and interests of this extraordinary genius. The nearly 500 letters reproduced in this volume were written or received between January 1694 and February 1697, a period of just over three years. It is awesome to realize that this was but a portion of Locke's total correspondence during those years, especially when one observes that some of these letters are the equivalent of short essays on complex questions of theology, philosophy and delicate affairs of state.

By 1694 Locke was settled in relative security in England; the most perilous years of political turmoil and exile abroad were behind him. His major works had either been published or were nearing publication. His fame was international and his reputation for prudence was such that he was frequently summoned to public service by King William, Lord Somers, and others in high position. Locke had written in *Some Thoughts Concerning Education* that it is "every Man's indispensable Duty, to do all the Service he can to his Country. . . ," and though his health was dangerously frail, he accepted a series of responsible and demanding posts. This portion of the *Correspondence* provides marvelously rich and detailed information on Locke's many contributions to the solution of such pressing problems as trade, colonial and foreign policy, and, above all, reform of England's debased coinage. As Locke wrote to his friend William Molyneux in 1696, "The business of our money has so near brought us to ruin, that . . . it was every body's uneasiness" (p. 594). Deeds followed words, as Locke spurred his closest friends in Parliament, Edward Clarke, Sir Walter Younge, and John Freke, members of his "College" or parliamentary political club, to secure passage of sound legislation leading to currency reform. Parliament acted favorably, and Locke wrote to Clarke assuring him that "I think the nation extremely obliged to you. . ." (p. 509). In the same letter he urged the college to initiate action on a broad front to instruct and marshal public opinion in order to insure the effectiveness of this vital legislation. Locke's letters in this regard are most instructive in revealing the spirit with which leading citizens in the social-contract society must be infused if the prosperity and security of the nation is to be preserved.

Even more fascinating in this portion of Locke's correspondence are the hitherto inadequately discussed accounts of his persistent efforts to conceal his authorship of *The Reasonableness of Christianity*. He steadfastly refused to acknowledge authorship of this controversial work to his closest and most trusted friends, even as he sought their opinion of it! His judicious friend William Molyneux never did "presume to enquire" whether Locke was the author, but he did report that "a very Learned and Ingenious Prelate said to me . . . [when] I asked him whether he had read that Book, and how he liked it; He told me very well; and that if My Friend Mr. Locke writ it, 'twas the best book he ever laboured at; But, says he, if I should be known to think so, I should have my Lawns torn from my shoulders. . ." (p. 702). These and many similar passages from this portion of Locke's correspondence may prove instructive to those who, for whatever reason, continue to experience insuperable difficulties in recognizing the issue of persecution and the art of writing.

ROBERT H. HORWITZ

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Violence and Oppression. By James C. Dick. (Athens: University of Georgia Press, 1979. Pp. x + 211. $12.00.)

Can it be moral for the oppressed to use deadly violence against their oppressors? To answer, James Dick analyzes four such episodes in American history. He concludes that in none of them—the battle of Alamance in 1771, Nat Turner's revolt in 1831, the Homestead labor violence in 1892, and the Colorado Coal War of 1914—was the violence morally justified, and, after further analysis, he concludes that such violence can seldom be justified. Finally, he gives us five criteria that must be met to justify such violence. These are the presence of serious injustice, the failure of less objectionable methods of redress, and avoiding violence that is gratuitous, directed against the innocent, or doomed to failure. Since most violence will not meet all these tests, Dick concludes that deadly violence by the oppressed is "not easily justified even under tyranny" (p. 187).

Does the analysis justify the conclusion? Probably not. The choice of cases, given Dick's assumptions, is peculiar, their interpretation questionable, the generalization drawn from them too broad, the range of possible moral justifications treated too narrow, and the analytical technique used to validate his five criteria too dubious.

James Dick uses case studies because a theoretical analysis not "steeped in the actualities of history" can only mislead (p. x). Yet his argument that to be justified violence must succeed makes his choice of cases most curious. First, he restricts his cases to those of small-scale violence involving at most a few dozen deaths, though it is quite plausible that effective violence is large-scale violence. Second, and still more curiously, he restricts himself to cases in which, by his accounting, violence failed, though he admits that it does not always fail, and in fact cites cases in which violence succeeds. How, then, can these studies of small-scale futile violence support a conclusion applying to violence in general, including large-scale successful violence, especially as success is made a moral test? Why, in view of his assumptions, did Dick not choose a more representative range of cases?

Nor is it clear that Dick interprets his cases correctly. He sees no successes, yet in the Colorado Coal War the power of a corporation that was virtually a state within a state was greatly reduced and, by Dick's accounting, a "brutal despotism" became a more or less "benevolent paternalism" (pp. 64-65). Dick asserts, though not convincingly, that the change was due to public reaction to company violence, not labor violence. But the one was both preceded and followed by the other. How does Dick know which link in the chain of violence was crucial, and how can he divorce one link from the others? Neither is it clear that a series of apparent failures, such as the Homestead violence, may not have a favorable cumulative impact on public policy.

Dick's moral analysis is too narrow. He does not give serious consideration to justifications of violence based on honor or just retribution, he does not seriously consider the possibility that tyrants may abrogate their rights, and he does not treat seriously the possibility that human dignity may in some circumstances require violence even if it fails to end oppression. (Dick dismisses honor with a quotation about dueling; retribution he tends to lump with revenge, which is dismissed; the death of a tyrant is treated as, at best, a lesser evil; and only in the case of the Warsaw uprising does he seem to accept doomed violence, though his explanation would seem to apply broadly.)

Finally, Dick tries to validate his five criteria by using John Rawls' original position analysis. But, just as Rawls' self-interested contractors, ignorant of their desert or lack of it, ignore desert as a standard of distribution, so Dick's contractors, ignorant of their status as oppressors or oppressed, ignore certain questions of honor and justice. Serious questions are not answered in this way.

WILLIAM R. MARTY

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Elitism. By G. Lowell Field and John Higley. (Boston: Routledge & Kegan Paul, 1980. Pp. xi + 135. $18.50.)

In this slim but expensive volume, G. Lowell Field and John Higley advance an unabashedly elitist position, discuss its implications and consequences, and defend it against rival postures.

The "eclipse of elitism" over the last 200 years, Field and Higley argue, is owing to the fact that, beguiled and deluded by socialists or liberal (welfare-state) principles, Western social scientists have erroneously (1) advocated generally egalitarian positions, and (2) assumed that science and technology are capable of addressing problems of resource scarcity and of assuring unlimited growth.

In setting forth their own position, Field and Higley define elites as "persons who occupy strategic positions in public and private bureaucratic organizations" (p. 20) and to whose rule "there is no alternative" (p. 70). The authors then restate "the elitist paradigm" developed in the seminal work of Vilfredo Pareto, Gaetano Mosca, and Roberto Michels by "markedly reducing [its] determinacy" (p. 19).

Thus, whereas Pareto, Mosca, and Michels had stressed the essential autonomy of elites, Field and Higley emphasize *non*-elite orientations and attitudes as serious constraints on elite action. Moreover, the authors hold, non-elite orientations toward societal issues vary with changes in the composition of labor force components as societies go through four levels of development: agricultural, lightly industrial, highly industrial, post-industrial. The direction of this change is from "prevailingly egalitarian" to "prevailingly managerial" (pp. 21-31). Although non-elite orientations influence the types of societal issues that will or will not be raised, given their "very general" nature, elites continue to have much discretion in shaping society (p. 19 and passim).

Similarly, whereas Pareto et al. had stressed the internal homogeneity and unity of elites, Field and Higley emphasize their heterogeneity and diversity. They identify four elite types, arguing that each type determines the intensity and consequences of political conflict in societies at the same developmental level, or at different levels. *Disunified elites*—characterized by the presence of distrustful cliques and factions—create political instability. *Ideological unified elites* of Communist/Fascist/Nazi varieties generate mass upheavals. *Consensual unified elites*—marked by trust, cooperation, tolerance, and moderation—make possible political democracy. *Imperfectly unified elites,* consisting of two main factions (one egalitarian, one inegalitarian), are likely to lead to political stability (pp. 35-43).

This restatement of "the elitist paradigm," Field and Higby maintain, holds "the middle ground between the classic elitist and the Marxist paradigms" (p. 19). The relations of production do have a bearing on societal conditions; so also do elites and elite conflicts.

Field and Higby proceed by (1) calling upon liberals to correct their mistakes and "recognize the essentially elitist character of their circumstances and interests" (p. 51); (2) focusing on the obligation of elitists, liberals, and others to support and promote elite consensus and unity as a means of maintaining democratic government; and (3) calling for greater freedom of action on the part of elites to handle increasingly complex world problems.

Elitism is refreshing in the unambiguous position it takes in treating a controversial topic. Some of the arguments are compelling and the book is well written. At the same time, the work is repetitious to the point of irritation and is in parts inescapably polemical. Moreover, at times the book is given to careless overexaggeration: "It is now necessary to refute the widely held assumption that values such as equality, liberty and freedom are universal and objective" (p. 3 and passim). Does a single person today accept this "assumption"? Finally, any claim to originality on the book's behalf must be tempered by a reading of Suzanne Keller's extensive analysis of "strategic elites" nearly two decades ago (*Beyond the Ruling Class: Strategic Elites in Modern Society*, Random, 1963), to which no reference is made. In fact, *Elitism* is curiously and inexplicably silent on *all* theoretical and empirical works on elites in the postwar period: with the exception of some of the authors' own writings, not a single work is cited or discussed.

MOSTAFA REJAI

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Equality, Liberty, and Perfectionism. By Vinit Haksar. (New York: Oxford University Press, 1980. Pp. 302. $24.95.)

Equality, Liberty, and Perfectionism by Vinit Haksar is a thoughtful yet disappointing work: highly priced, embarrassingly proofread and formalistic in its entry into both politics and life. Ostensibly, it is a defense of *perfectionism*, the belief that some forms of life are "intrinsically superior" to others and entitled therefore, if not to special consideration, at least to special notice. In actuality, it is a schoolmaster's demonstration that contemporary analytic and liberal theory both need and/or rely on perfectionist claims in making their case.

Vinit Haksar develops his argument in three stages. First, he criticizes attempts by John Rawls, Joel Feinberg, Stuart Hampshire, Bernard Williams, S. Benn, Phillipa Foot, H. L. A. Hart, William James and others to justify various versions of *egalitarianism* without an appeal to perfectionism on the grounds that in discussing personhood they either assume that people are (a) "wonderful creatures, compared with animals" (p. 25) or are (b) able to acquire perfectionist qualities of language, self-consciousness, autonomy, the ability to form and carry out life plans with zest, and therefore able to develop the capacity for moral sentiments and judgment.

Second, Haksar claims that perfectionist considerations must be invoked in order to work out the *liberal* implications of the egalitarian doctrine of equal respect for persons. But, he notes, the "doctrine of equal respect does not automatically commit us to a liberal society" as John Rawls and J. S. Mill would have it, nor do Rawls and Mill offer plausible explanations of why people either would (or do) choose to rank-order their values and actions. Thus, Haksar suggests, Rawls' notion that people prefer complexity to simplicity (the Aristotelian principle) and Mill's conviction that only exceptions would choose slavery over freedom meet neither real-world nor logical tests. Indeed, despite many differences between Mill and Rawls, Haksar is persuaded that the views of each are predicated on perfectionist assumptions.

Third, Haksar advances a case for perfectionism on its own merits. Using the notion of paternalism (where some people rank-order the interests of those who are not able to do so), Haksar suggests that while "some forms of life are *prima facie* more suited to human beings" (p. 295) than others, such a view does not commit one to the persecution of those who would eat their own excrement in public or otherwise engage in personally demeaning behavior. For Haksar, the perfectionist approach demands that individuals be treated as sacred ends in themselves. "What I would suggest," he concludes, "is a system of toleration but not of equal liberties for different forms of life" (p. 290).

But is it really superior and inferior "forms of life" that we need talk (philosophize) about, or rather the conditions in our shared transactional world that now threaten the human species with extinction? Haksar's mistake and the mistake of those who share his philosophical bias is that both have severed thought from its social context, substituting logic for life as the prime currency of intellectual exchange. One must wonder about the serviceability—indeed about the point—of a philosophical enterprise which seems unable to inspire wonderful creatures to act in wonderful ways or to warn them that humanity is faced with extinction through the continuing threat of thermonuclear war, environmental collapse, poverty and spreading alienation, rather than, as Haksar suggests, through people suddenly deciding not to reproduce themselves thereby bringing history to an end.

In a human and literal sense, then, Haksar's case for perfectionism is quite meaningless. It seems neither caring nor questioning enough about people's everyday lives to convey the real agony involved in anyone's or any species' premature death.

MARK READER

Arizona State University

On Justice: ΠΕΡΙ ΔΙΚΑΙΟΥ. By J. R. Lucas. (New York: Oxford University Press, 1980. Pp. vii + 270. $24.95.)

This is a rambling book, a series of reflections on what justice is and what justice is not. J. R. Lucas argues that there are certain prerequisites to justice. Most important is rationality. Justice is careful calculation, exemplified by the game theories he introduces in chapter 3. These calculations, though, must be limited by certain qualifications. Justice must take into account our needs; it must take into account our interests; it must take into account the needs and interests of others. It must also attend to deserts; it must attend to ties of family; it must attend to individualized characteristics. There are some things which the author argues justice is not. It is not equality; it is not utility; it is not Rawlsian.

One is left in the end with the many points to which justice must attend and yet with little, if any, guidance as to why, how and to what degree. We are told for example: "It is difficult to see the rationale for hostility to inherited wealth" and that "since we believe that the family is a good thing, it is surprising that we do not follow the principle through" (p. 248), but we are not told why the family is a good thing, why inherited wealth is necessary for the preservation of the family. One could argue seriously about both these issues, but the author appears to be content to accept common (and mostly conservative) prejudices as facts which need only be stated to be accepted. One sees a similar tendency when the author argues, still on the issue of taxes, that the wealthy Briton taxed at 98 percent should be given credit for his past contributions and not taxed again as he had been before. Lucas urges a view of justice which looks backwards "not merely to what people still have but, equally importantly, to what they have already paid" (p. 246). Yet, he wants to go back only so far. He does not want to go back to the question of how the individual reached the high tax bracket. Lucas does not clarify by what guidelines we are to decide how far back we are to go in our search for justice.

As the above examples should demonstrate, the book is bound by the British background of the author. The general reflections on the meaning of justice, suggested by the title and especially the Greek subtitle (presumably meant to recall the investigations of a Plato or an Aristotle) are limited by the perspective of an Englishman, and an English don at that. This tunnel vision is perhaps most obvious to an American reader in the author's dependence on a British idiom which begins the book and is used throughout to explain the meaning of justice: "to do down" or "to be done down by." I presume that the more graphic American idiom is "to do dirt." To rely on such a culture-bound idiom to get to the essential meaning of justice as applied to relationships within the market, within administrative units, between states, among taxpayers, etc., is to bring in serious limitations. This book is too unsystematic and too culture-bound to lead us very far in the search for an understanding of that elusive concept, justice, which, despite its elusiveness, we hold as a value to be pursued.

There may, however, be another way to read this book, one suggested by the discussion which appears on p. 60. There, Lucas is in the midst of explaining the relevance of game theory to the rationality on which he wishes to base justice. After presenting the mandatory Prisoners' Dilemma, he turns to the example of the Battle of the Sexes to illustrate the "logic of political situations" (p. 58). Lucas explains the model:

> He and She want to spend their holiday together, but he would prefer them both to go mountaineering in the Alps [a nice active pursuit, we might note], whereas she would rather they both spent it sunbathing by the sea [women always prefer the passive pursuits]. . . . If she can throw some hysterics and say she cannot abide the Alps . . . then he, if he is reasonable will . . . settle for the sea. . . . But equally he may see that the moment has come to take a firm masculine line and let the little woman face up to the realities of the situation, and either come along with him or go her separate way.

One could react by banging the book down, saying he can't do this in a book published in 1980, and by Oxford University Press, at that. He can't do this and be serious. Aha, one might think, he's not serious. He's doing this to provoke the reader, to show the reader where his or her sense of justice comes from. He's doing this to alert the reader to the dual meaning of his work, to the esoteric and exoteric content. For the average individual, happy to accept the customs and stereotypes of English society, ready to be urged towards a certain moderation from all extremes, be they extremes of the British tax law or the extremes of the "claim of the Third World for a fairer share of the earth's resources" (p. 255), for these individuals he argues that justice comes from reason tempered by a certain tenderness, and that most laws are really the result of reason and the attempt to apply rational judgments to specific situations. But for those not quite so ready to settle into the easy-going acceptance of society as it currently exists, these are the ones he may want to arouse; he may want to show that justice is best explained, as Aristotle pointed out (*Nichomachean Ethics* V. 1), by reference to what is unjust. (Perhaps it is this which explains the Greek subtitle.) Lucas may want to show that justice does not come from the careful calculations inherent in a game theory model, or from weighting arguments and evidence in the mythologized courtroom, free from rhetoric and appeals to emotion which he paints on pp. 36 and 80. Rather, justice comes from ire, *thumos*, a sense of what is wrong with the assumptions and biases of contemporary society. The key to Lucas' real intent may be on p. 60. Only the number of chapters does not add up to thirteen.

ARLENE W. SAXONHOUSE

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Improving Policy Analysis. Edited by Stuart S. Nagel. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. 264. $18.95, cloth; $8.95, paper.)

Literature on policy analysis abounds—one is continuously inundated with works which analyze various stages of the policy-making process, identify methodologies and approaches for doing policy analysis, and even examine the substantive nature of policy and politics. This volume adds to the proliferation of this literature by attempting to improve our ability to determine what policies will achieve goals and to determine the effects of policies (p. 7). Does it strengthen our knowledge of policy analysis? The answer is a definite yes. The 13 contributions to the volume are generally well done and, in total, provide a comprehensive survey of the state-of-the-art of policy analysis. Although it suffers from the lack of a concluding chapter which would integrate the important ideas found within, this work would be a valuable contribution to graduate readings and it is recommended for anyone interested in *doing* policy analysis.

The work is divided into four parts: (1) deductive modeling and optimizing in policy analysis; (2) the legal perspective in policy analysis; (3) the evaluation research perspective on policy analysis; and (4) implementing policy analysis research. Although each of these sections is valuable by itself, the book is much more important for several central issues raised about policy analysis. Among them are the nature of policy analysis as a profession, appropriate methodologies, and utilization.

An important question about policy analysis is its place in social science research—is it a fad or a field of study worthy of institutionalization? Although only a few authors address this question explicitly, by implication it is addressed in virtually every article. Widespread agreement emerges that policy analysis can effectively contribute to better policy making, that there is a developing set of methods and approaches for doing policy analysis, and that a rapidly growing professionalism is evident. The two most useful articles addressing this concern are by Duncan MacRae ("Policy Analysis Methods and Governmental Functions") and Arnold Meltsner ("Creating a Policy Analysis Profession"). Meltsner states that "It is not unreasonable to assume that policy analysis . . . is a more or less permanent feature of public policy making" (p. 236). One finds very little disagreement on this point throughout the volume.

On other issues, more disagreement is evident. For example, a persistent question concerns the methodology of policy analysis. In fact, some contributions approach policy analysis only as a methodology. John Brandl ("Satisficing, Feedback, and Decision Rules in Government") and Werner Hirsch ("Policy Analysis of Legal Policies") are most indicative of this perspective, which seems to value measurement precision and formal modeling to the exclusion of other elements of policy analysis. In contrast to this perspective, and more convincingly demonstrated in this volume, is a perspective which characterizes policy analysis as including "much more than procedures for gathering and statistical manipulation of data" (MacRae, p. 130). This broader conception includes attention to qualitative approaches, attention to political feasibility and administration (see Nagel, "Political Science and Public Administration as Key Elements in Policy Analysis"), and to designing information systems to meet user needs. In this regard, the contribution by Peter House and Joseph Coleman ("Realities of Public Policy Analysis") provides an excellent overview of the practical considerations of doing policy analysis research, and Jack Byrd's refreshing article ("The Humanization of Policy Models") advocates a strong dose of common sense in building policy models.

The most significant contribution of the volume is the consideration given to the use of policy analysis. While some authors avoid this issue or simplistically assume that use is related to the identification of causal relationships, most of the contributions emphasize what is done with policy analysis rather than how policy analysis is done. In this respect, particularly interesting questions are raised about the sociology of knowledge (Edward Lehman, "Policy Research: Industry or Social Movement") and about social scientists' perceptions of barriers to utilization (Karin Knorr, "The Gap Between Knowledge and Policy"). No easy conclusions can be drawn about utilization. However, the clear picture from this volume is that policy analysts can contribute to positive social change, and that the responsibility to understand and enhance the utilization process lies with the researcher.

STEVEN BALLARD

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History of the Idea of Progress. By Robert Nisbet. (New York: Basic Books, 1980. Pp. xii + 370. $16.95.)

Robert Nisbet has written not a history of the idea of progress but a comprehensive survey of Western thought about "progress," however variously understood. His book is not guided by any interpretive rationale or philosophical reference point which might serve to distinguish one level of meaning from another. Even in the epilogue, where an attempt is made to draw conclusions from the survey, Nisbet preserves a neutrality which respects all theories encountered on this guided tour of the reliquary of great books which mention or imply some notion of "progress" (from Hesiod to Teilhard de Chardin). Rather than working out a critical analysis of these ideas about progress, the author is satisfied to provide a kind of annotated bibliography or catalogue of passages which he believes will demonstrate that Western civilization has been supported throughout its career by "a rarely interrupted conviction that the very nature of knowledge . . . is to advance, to improve, to become more perfect" and that the goal of this advancement is an "ever greater perfection of human nature" (p. 5).

While never in any age "universally held by intellectuals" (p. 6), progress is always—at least until quite recently—"the dogma" toward which "the overwhelming majority of the greatest minds in Western history are oriented" (p. 8). Since Nisbet believes this orientation was a positive force in the development of civilization, he worries about the consequences of the rapid waning which "everything now suggests" the dogma of progress is experiencing "in all levels and spheres in this final part of the twentieth century" (p. 9).

Quite understandably, Nisbet encounters expressions which evoke some notion of "progress" just about everywhere in Western literature. Thus his account of the history of these expressions has a cast of hundreds, headed (in order of the attention they receive) by St. Augustine, Marx, Comte, Turgot, Condorcet, Plato, Herbert Spencer, Hegel, Darwin, Saint-Simon, Rousseau, Joachim de Fiore, J. S. Mill, Adam Smith, and Lucretius. But his litanies of names and concepts, his frequent citations and quotations (all without the documentation one expects in a work of scholarship) advance no thesis. As he admits in the foreword, the only thing Nisbet tries to do is "identify and put in proper perspective the major personages, texts, presuppositions, intellectual climates, and philosophical and ideological uses of the idea during the past twenty-five hundred years" (p. ix). Apart from any bibliographical purpose this might serve, it is not clear how the author expects us to profit from his survey (although he thanks his sponsors at the American Enterprise Institute "for their perception of the book's subject as falling within public policy research" [p. xi]). His frequent lamentations about the lack of space to develop some theme or other suggest that Nisbet himself felt frustrated by his project.

Whatever its aim may be, his history reads like a well-informed conversation one could imagine overhearing at a faculty club, where leisurely relaxation rather than serious scholarship is in progress. Among the hazards of generalization to which the book succumbs, the most serious is its failure to make the necessary critical distinctions about the different meanings of "progress" and their significance for the history of ideas. Just about any kind of positive development or process, any sense of progression in time, any theory of continuity or advancement is sufficient to count as "progress," whether it be human or divine in inspiration, temporal or spiritual in content, physical or moral in effect. Even the Aristotelian doctrine about the movement in nature from potency to act is an idea of "progress."

Of the consequences of this lack of critical precision, a few examples must suffice: Nisbet detects no essential difference between the Augustinian and Hegelian philosophies of history; he misses the crucial contrast among the American Founders between Adams and Jefferson; he treats as equally "Christian" orthodox forms of belief and such aberrations as millennialism.

At the very least, definitional responsibility would seem to call for a clarification of the difference between a sense of progressive development and the presumption of progress (progressivism) one finds in the political ideologies which are the product of modernity in the history of philosophy.

The book is divided into two large chronological parts (before and after 1750). Part 1 focuses on the classical, early Christian, Medieval, Renaissance, and early modern periods. Although Nisbet regards this part as his most original contribution to the literature on progress (in contrst to J. B. Bury's denial that any positive attitudes toward progress could be found in the premodern period), it is principally here that a lack of definitional precision creates the most problems. Plato, Aristotle, Augustine, and Dante are all made out to be partisans of temporal progress in spite of the manifest orientation toward permanence rather than change which pervades their thought. On the other extreme, Nisbet fails to discover the progressive spirit of Erasmus, Machiavelli, Bacon, and Descartes which so powerfully helped to lay the foundations of modernist progressivism.

Part 2 is more persuasive; the progressive spirit that prevailed after 1750 would be hard to miss. Here Nisbet drops the chronological format and organizes his material around two "tests" which reveal the modern orientation toward progress: the expansion of freedom and of power.

Having reached the twentieth century, Nisbet closes his survey with contrasting essays on the contemporary persistence of progress and the intellectual questioning of that idea which he finds unique to our own times. The suggestion that our century is unlike all that have preceded it in this respect is weakened by the same flaw which pervades the whole work: "progress" is seen as a general idea of virtually limitless range rather than as a spirit with precise philosophical or ideological tendency against which objections were raised in all times and places.

JOHN A. GUEGUEN

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The Laws of Plato. Translated, with Notes and an Interpretive Essay, by Thomas L. Pangle. (New York: Basic Books, 1980. Pp. xiv + 562. $26.50, cloth; $11.95, paper.)

Some years ago Leo Strauss put forth the claim that the Platonic dialogues are artistic masterpieces which cannot possibly be understood unless account is taken of their dramatic form and attention paid to all of their details, even those seemingly trivial and obscure. One implication of this claim, which Strauss' commentaries have gone a long way to substantiate, is that literal translations of the dialogues are absolutely essential. Strauss himself published no translations, but his students have, several in recent years. Of these the most recent and, indeed, the most monumental is Thomas Pangle's translation of the *Laws*.

Pangle's achievement is remarkable: He has provided as accurate a translation of the Greek as readable English prose will allow; and since the only other translation of the *Laws* based on Des Place's and Dies' edition of the Greek text (the Penguin *Laws* by Trevor Saunders, 1970) is confessedly mistranslated, Pangle's book is especially needed and welcomed.

The accompanying interpretive essay is an excellent distillation of a dialogue three times its size. The commentary is thoughtful, even profound; and it amply demonstrates the importance of reading Plato carefully and from a translation that is true to his language.

Pangle begins by noting the strangeness of the *Laws* to contemporary liberal views. The source of this strangeness seems to lie in the dialogue's preoccupation with the problem of politics and philosophy. Because liberalism treats politics as the reconciliation of competing interests, it cannot quite comprehend politics as moral education; nor can it see this education as dependent on, as well as threatened by, philosophy: to restrain material gratification and the free expression of ideas is intolerance serving the cause of totalitarianism. But liberalism today suffers from a lack of confidence, and it is at all times loath to defend itself. Nothing would better help to clarify and reinvigorate liberalism than for its proponents to study Plato's critique—because it is a qualified critique equally aware of the objections to political utopianism.

Much of Pangle's analysis centers around the question of the contribution of political order to human happiness: does Magnesia, the city of the *Laws*, offer the best life *attainable* by human beings? In light of a "Rousseauian" state of nature present in book 3 (primitive man as simple and innocent), civil society would seem able to justify itself "only on the basis of the hypothesis that man is mutilated without the pursuit of wisdom" (p. 427). Philosophy vindicates the sacrifices involved in political virtue. But few men are philosophical; and since the knowledge that is a concomitant of civilization empowers human beings to injure one another, politics must become increasingly repressive in order to control this potential for evil. The goodnes of political life is fraught with ambiguity if a sub-political life were thought to be decent (a "Hobbesian" state of nature inhabited by Cyclops is also suggested) and a trans-political life beyond the reach of most men.

Pangle explains the early books of the *Laws* as an attempt to find a suitable and effective foundation for moral virtue. What he terms the "psychology of citizenship" is rooted in various conceptions of virtue and happiness and is benefited by a number of external supports. First, there is courage understood as heroic struggle pointing to the pleasures of freedom. Spirited citizens find the rewards of political participation especially appealing and are devoted to the city on this account. In a similar manner, the founder is tied to the justice of the city by his pursuit of personal fame. A sense of shame giving rise to moderation can be an alternate basis of political virtue. This is assisted by a pious poetry that tells of just and benevolent gods. There are also the political institutions of a mixed regime which lend support of an artificial kind. But the city's grandest offerings, short of philosophy, are the intellectual and artistic pleasures of music. The aesthetic soul is a sort of intermediary between the courageous and the moderate on the one hand and the philosophic on the other.

Notwithstanding these advantages and pleasures, Pangle understands the *Laws* to say that political virtue, if it is not to become a means to an end lower than itself, must point to something higher than itself. The city cannot philosophize, but it needs philosophy in order to justify its moral education; more practically, it needs the wisdom of the philosopher to supervise the changes of law. But the persistent question is whether the city's need for philosophy is compatible with its need for piety. The *Laws* offers some resolution by using philosophy as a defender of the faith and by subjecting the impious to the "punishment" of philosophic dispute. That a place for philosophy can be found within the city is suggested by the willingness of Kleinias and Megillus to subordinate themselves to the Athenian Stranger.

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Political Theory and Political Education. Edited by Melvin Richter. (Princeton, N.J.: Princeton University Press, 1980. Pp. 203 $13.50.)

This book is an edited volume of previously unpublished papers by prominent political theorists which were first presented at an international meeting of the Conference for the Study of Political Thought. The questions addressed by the essayists are suggested by the title: how should the next generation of political theorists be trained; how should they understand their vocation; and what can political theory contribute to the education of nonacademics, such as citizens, lawyers, and officeholders?

The contributors and their essays are: Bernard Williams, "Political Philosophy and the Analytical Tradition"; Charles Taylor, "The Philosophy of the Social Sciences"; Mihailo Markovic, "Marxism as a Political Philosophy"; Allan Bloom, "The Study of Texts"; J. G. A. Pocock, "Political Ideas as Historical Events: Political Philosophers as Historical Actors"; Michael Walzer, "Political Decision-Making and Political Education"; Ronald Dworkin, "Political Theory and Legal Education"; and Sheldon Wolin, "Political Theory and Political Commentary."

Like most volumes of conference papers, the book's promise somewhat exceeds its performance. The essays, as a group, are of excellent quality but are rather sketchy. They provoke and tantalize more than they satisfy. Nevertheless, all who are seriously interested in contemporary political theory will find reading this book a rewarding experience—for a number of reasons.

First, it provides a very good overview of the contemporary rebirth of political theory. As Melvin Richter's extensive introduction makes clear, each of the contributors represents in his own work an important facet of this rebirth. Reversing the widespread impression of a couple of decades ago that political theory exhausted itself in the collection of philosophically obsolescent museum pieces, the volume demonstrates the mutually informative relationship between political theory and several contemporary intellectual disciplines and developments. These points of tangency can be expected to be increasingly significant in the next decade; and those who intend to work in political theory should be conversant with them.

Next, the essays help to exemplify some of the widely (though by no means universally) shared views of most political theorists today—for example, as to the intellectual inadequacy of positivism and the moral insufficiency of utilitarianism. At the same time, some of the more interesting and significant disagreements are also explored—for example, in the different recommendations of Bloom and Pocock for studying the classic texts.

Above all, the essays offer in their distinctive ways some very provocative intimations of ways to accomplish a major task of political theory: to provide illumination, perspective, and clarity for citizens and political leaders confronting the choices and decisions in their own lives and vocations.

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Collective Decision Making: Applications from Public Choice Theory. Edited by Clifford S. Russell. (Baltimore: Johns Hopkins University Press, 1979. Pp. xvi + 296. $24.00.)

These papers, originally presented at a Resources for the Future forum, aim to demonstrate that public choice theory can explain real political action and guide actual policy decisions. Lamentably, this aim is unmet. Of course, public choice literature elsewhere does contain outstanding accounts of real political behavior, e.g., Morris P. Fiorina's *Congress: Keystone of the Washington Establishment* (Yale, 1977), and William A. Niskanen's *Bureaucracy and Representative Government* (Aldine-Atherton, 1971). However, the contributions in this volume will, by and large, feed the fear that public choice research is "the highly theoretical plaything of a few intellectuals" (p. xvi), as Hitch puts it in his introductory remarks. The bulk of the articles fall into three basic categories: (1) nonexperimental research drawing on the self-interested, rational actor hypothesis, e.g., the papers by Portney and Sonstelie, and Mitchell; (2) laboratory experiments on decision-making rules, e.g., the papers of Plott, and Ferejohn, Forsythe, and Noll; and (3) review of policy applications of public choice, e.g., Tullock's paper. (Each of these articles is followed by comments from other participants in the forum.)

The piece by Portney and Sonstelie on "super-rationality" and school tax voting typifies too much of the empirical work in the public choice field. That is, one must strain to believe the rational (in this case "super-rational") calculations the actors are assumed to carry out in making their decisions. Here the assumption is that, in a school tax election, home owners vote to maximize the value of their property. As Russell points out in his introduction, this requires one to act "as if one knew the market's (other people's) evaluation of marginal changes in unmarketed services" (p. 11). Such an assumption is hard to swallow. Mitchell's paper tries to explain why individuals join environmental lobbies like the Sierra Club when, according to Mancur Olson's logic, they could receive the group benefits without joining. Hardin's comments imply—rightly, in my view—that Mitchell's approach is wrongheaded, for it tries to explain the exceptional case. For instance, a million Americans do belong to environmental groups, but over 200 million do not, perhaps because they are free riders à la Olson.

In the editor's opinion, the papers of Plott and of Ferejohn et al. are "the most exciting of the forum" (p. 14). I must dissent from this judgment. Plott carries out laboratory experiments intended to test alternate theories of decision making under majority rule. Ferejohn et al. explore, in the lab and in practice, how public television stations allocate their budgets to acquire programs. While these experimental efforts are quite sophisticated, they are poor examples of *applied* public choice research. With Chamberlin, I worry over their external validity. They certainly do not meet his demand that "the experimental conditions be sufficiently like the conditions in the naturally occurring environment" (p. 162). Rather, like most of the work in axiomatic social choice theory, it is heavy with unreality.

We come to the final concern of the volume, which is to show the influence of public choice theory on governmental policies. To summarize the principal conclusion on that score, let me simply quote Tullock: "If . . . we look for specific actual changes in government policy which can be directly attributed to public choice activity, I think we would find that there are none" (p. 33).

Why does this set of papers not provide convincing applications of public choice theory? One reason, I would speculate, is that many of the authors have little concern for (understanding of?) political realities. Like the reformers of the Progressive era, they believe all we need for good government is better rules. Tullock justly criticizes those reformers whose only remedy "was more careful selection of personnel and moral lectures" (p. 31). But how far is this naive remedy, at least in spirit, from the institutional design proposals of many public choice theorists? The moral tone is shared, as evidenced by Plott's declaration: "Social organization and institutions must be 'correct,' in order for the 'proper' behavior to automatically [sic] flow from the public sector" (p. 137). Further, there is boyish optimism about the ease of implementing their proposals. For instance, Ferejohn et al. remark, "Many if not all of the features of economic environments can be modeled easily and usefully in laboratory settings" (p. 188). And, Tullock opines, "If the present trends are in the direction of greater centralization, it still seems to me that it would not be very difficult to reverse them. . ." (p. 42). But it is not easy to introduce new political rules, even if they are "correct." Haefele, in his contribution, notes that Jefferson wrote a parliamentary manual for the Senate, which is very like a set of rules from contemporary social-choice theory, but very unlike the current rules of the Senate. An appreciation of Jefferson's failure to make his rules stick should temper the self-assurance of these modern-day "institutional engineers" (see p. 139).

MICHAEL S. LEWIS-BECK

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Marxism, Socialism, Freedom: Towards a General Democratic Theory of Labour-Managed Systems. By Radoslav Selucký. (New York: St. Martin's Press, 1979. Pp. xiii + 237. $22.50.)

Radoslav Selucký's provocative book deserves careful reading. He claims to have discovered, through his reexamination of Marx's concept of the market, the theoretical mistake which turned Marx's ideal society into a nightmare; to prove the impossibility of a non-market democratic theory; and to have so redefined socialism that, "stripped of its utopian features, enriched by valuable liberal principles and based on a synthesis of the market and social planning, it is possible to construct a theoretical model combining socialism, individual freedom and political democracy, as well as a democratic theory maximizing both sosocial welfare and liberty" (pp. xi-xii).

In the course of a very interesting argument, Selucký suggests that, even given the distribution of wealth according to labor output necessitated by scarcity and the social division of labor, a (socialist) command economy adds to the alienations historically associated with capitalism the further alienation of workers from their wages. Since "the demand for the desired use-values cannot be identical with the sum of the fulfilled plan targets . . . any consumer's attempt to promote his needs through effective demand is irrational. . ." (p. 39). One structural consequence of the frustration of effective demand typical of non-market, socialist systems is the emergence of a potent, ramifying black market.

History, reason, common sense, and experience have shown the market to be essential to insuring efficient production. Furthermore, not only does the market express and preserve the autonomy of the individual, but also structural links exist between the market and positive freedom. For Selucký, the preservation of the market is the necessary condition for securing in socialism five "ethically valuable liberal principles": (1) negative freedom, (2) the right of every individual to pursue his or her concept of happiness, (3) the right of every individual "to develop all his potential talents, abilities and faculties," (4) the equality of all persons, and (5) the right of the individual "to decide about the matters which have an impact upon" his or her life (pp. 173-75).

In an acute analysis of the works of Milton Friedman and C. B. Macpherson, Selucký demonstrates that (1) a (regulated) market can exist in socialism and (2) "had Macpherson realized that a market under certain circumstances could protect the negative liberty of the many *and* yet bring about some positive liberty to the many, he would certainly be less categorical as to his condemnation of the market and his praise of non-market libertarianism" (p. 151). Like Friedman, most people identify a market economy with competitive capitalism, while "socialism is identified with a non-market economy relying on coercive macroeconomic coordination" (p. 136). But such identifications are possible because those making them fail to "distinguish between the essence (market) and the appearance (competitive capitalist market), between the general (autonomous enterprise) and the particular (private enterprise)" (p. 137).

Given capitalist premises, the market causes contradictory social effects because "individual producers verify the social usefulness of their labor . . . through an intermediary, . . . only when the product has reached the market" (p. 5). Thus it tends to cause disharmony between supply and demand, it increases social inequality, "it encourages only economic activity which yields profits and is advantageous for the producers," and it leads to monopoly (p. 6). Because the market is a mechanism of production necessitated by scarcity and the social division of labor, phenomena which will survive a transition to any foreseeable socialist system, its dysfunctions can best be eliminated or mitigated by subordinating it to socialist premises by extending liberal democratic principles to the economy itself. These socialist premises are: (a) public (rather than state) ownership of the means of production following the Yugoslav innovation; (b) the establishment of an (indicative) social plan, exempting from market determination health and education needs; (c) distribution according to labor output; and (d) the extension of equality to the economic sphere. "Public ownership and social planning . . . acquire socialist properties *only* if combined with distribution according to work and equality extended to the economic sphere *provided that* principle (a) implies equal access to social capital, (b) implies social security and welfare, (c) implies absence of any exploitation, and (d) implies equal distribution of, and equal access to, economic and political control" (pp. 137-38).

One may wish that, in the course of his generally sound critique of Marx, Selucký had adverted to the works of Agnes Heller, and had concentrated on the distinction between single and complex labor and on the differences among natural, necessary and radical needs. By adding Heller's insights to his own, Selucký could have made his argument—that Marx's failure to understand the necessity for the market (following even the elimination of value from the second phase of communism) led to Lenin's statist excesses—more powerful and more subtle. More personally, I would like to know what the author might have made of Jean Baechler's arguments (1) that private property is the *sine qua non* of individual autonomy, and (2) that democracy is the source of capitalism; not the reverse, as Selucký argues.

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The Rights of Reason: A Study of Kant's Philosophy and Politics. By Susan Meld Shell. (Buffalo, N.Y.: University of Toronto Press, 1980. Pp. xi + 205. $13.50.)

The Rights of Reason is an important, original, and penetrating study of Kant's philosophy and political theory in light of his understanding of the relation between man and nature and its bearing on his philosophy of right. It is the first sustained attempt to explore these dimensions of Kant's thought; moreover, Susan Shell's approach demonstrates a degree of coherence to Kant's view that is lacking in other accounts. In successive chapters, Shell considers Kant's early writings on nature, the development of critical philosophy, the growing emphasis on morality, and his doctrine of right. This allows her to show the development of Kant's thought, but also the persistence of certain basic themes, from the early writings in which Kant viewed nature as indifferent to human ends and needs, an assault on both our dignity and security, to the last essays on man's prospects in a world of strife and injustice. Thus the basic categories—nature, reason, morality, and law, and the subtle interrelationships among them—appear throughout the work. The idea of property also looms large in this study; Shell views Kant's metaphysics, jurisprudence, and doctrine of right as substantially theories of property, answers to the questions, what can I legitimately claim as my own and on what basis? Property, for Kant, is thus not so much a source of satisfaction as a "condition of rational thought and action" (p. 187). Moreover, with the possession of any right, there is always for Kant a limitation to be accepted. Shell does not give us a comprehensive account of Kant's political philosophy, but she does provide the framework within such a task might be attempted.

Kant's moral philosophy is the usual avenue into his political theory; Shell believes, to the contrary, that we must begin with nature. Nature as opposition and man's righteous reactions to it, she argues, define the basis of Kant's thought. "Our concept of right is initially awakened by natural objects which push against the will and resist it. Right is itself a revolt against nature, a revolt in which no other creatures share" (p. 113). As children we react in anger to whatever inhibits our will; we claim rights against nature itself, and against other persons. Moreover, reason itself develops through opposition. It is through the sense of touch, for example, that we develop the notion of form, and abstraction, with its distancing, involves a repulsion of nature. Similarly, morality develops in opposition to an indifferent nature, one that frustrates and harms us. Yet only by turning that anger inward and accepting limits on one's desires can we begin to overcome nature. Moral experience, for Kant, is "inevitably jarring and disjointed. Morality draws us up against nature, . . . especially against our own nature" (p. 67). With morality and a sense of right, a fundamental distinction appears, between willed and unwilled opposition, between the opposition of persons and the forces of nature. When anger is turned outward, it will now be against our fellow beings, as indignation. Blame, however, involves an impersonal standard; with blame comes rights and a recognition of equality. "Right, properly understood, is rational indignation, anger objectified" (p. 110). Henceforth, "right and duty, self-assertion and self-limitation . . . go hand in hand" (p. 110). Property, law, and political order also spring from opposition, coming mainly from our fellows, but become aspects of a coherent doctrine of right, the purpose of which is to provide the principles necessary for reconciling competing claims. Yet, wherever persons and nature (and not just self and others) meet—reason, morality, property, or law—there are inevitable limitations that must be accepted. To be free is not necessarily to be happy; that is something we can hope for, and to labor for, but it is not ours as a matter of right. Kant would have rejected the optimistic expectations of Hegel and Marx as "dangerously mystical and misguided," but he did not glory "in deprivation" (p. 4).

Susan Shell has written a fine book, but not one free from certain difficulties. To begin with, she sets up expectations that are not fulfilled. In the introduction, Shell indicates that she will offer criticisms of Kant's conceptions of "nature and justice and their interrelation," and also that her book will shed "new and useful light on Kant's place in the history of liberal thought" (p. 5). Her ideas with respect to both are suggestive, but not developed, and occupy only two pages of the text. Also, her explication of Kant is far too compressed, even, at times, elliptical. Her comparison of Nietzsche with Kant (p. 122) rests on a confusion: she mistakenly equates revenge (*Rache*) with *ressentiment*. More seriously, she *assumes* that Kant is a contract theorist, yet her own evidence from Kant seems to argue against this (p. 168). She also states that Kant's theory of political obligation rests upon consent, but this is simply not the case. Finally, though she provides evidence of a Kant quite different from the one most of us had imagined—a Kant who has looked into the abyss, who has observed deeply the tragedy of man in nature and society, and yet retained hope—she gets no further than describing him as "melancholy," meaning one who is "righteous and embittered by injustice" (p. 179). There is, nevertheless, in the material Shell presents a Kant who is as aware as Pascal, Nietzsche, or Kafka of man's existential dilemmas and vulnerabilities. Kant does not move beyond good and evil, but he sees beyond them. To her credit, Shell points in the direction of this Kant, even though she leaves the portrait unfinished.

ROGER W. SMITH

College of William and Mary

Knowledge and Decisions. By Thomas Sowell. (New York: Basic Books, 1980). Pp. x + 422. $18.50.)

"Every false diagnosis of a condition," Thomas Sowell remarks (pp. 258-59), "is an obstacle to improvement." Unfortunately, he ignores his own good advice too often, with the result that an impressive effort to diagnose the modern condition is badly weakened by questionable assumptions, shotgun empiricism, and ideological caricature.

An economist of remarkable range, who is as well prepared to analyze civil rights law as he is anti-trust policy, Sowell writes in the tradition of neoclassical liberalism—a tradition now firmly transplanted from Manchester and Vienna to Hyde Park and Westwood. He criticizes virtually all state intervention in the economy, except in the case of "natural monopolies" (following Milton Friedman) or when third parties are adversely affected by a transaction. Like Friedrich Hayek, he identifies the central issue of modern times as the conflict between the freedom anchored in the market economy and the coercion exercised and threatened by all state authority, in its most extreme form by totalitarian regimes.

According to Sowell, to defend the "freedom of the many" (p. 289) is to go against the grain. Intellectuals have always been apologists for despotism (p. 349) and the "presumed wisdom of the few" (p. 289). Today they form a rapidly proliferating "new class" whose aim is to use the force of the state to wrest power from the business elite. By now, this is a claim familiar in the work of "neoconservatives," but Sowell tries to give it comprehensive and theoretical statement.

Sowell's premise is that decision making requires knowledge, defined broadly to include all ideas requiring authentication. Social decisions are made with the "social knowledge" conveyed in economic, political, and social transactions. This conception of knowledge blends communication, organization, and economic theory to stress the transmission of information (variously authenticated) about costs and benefits in a decisional context of systemic incentives and constraints.

The thesis is that the system of voluntary exchange among self-interested parties through the price mechanism (alias capitalism) is a more socially efficient process of decision making than that which depends on government in all its forms. Economic actors directly affected by their own decisions are held better able to calculate costs and benefits than remote politicians and bureaucrats; "surrogates" (i.e., representatives), administrators, and judges interpose their own self-interest, because their power and reward batten on the enlargement of their roles. Public policy is further distorted because collaboration develops between the regulators and the special interests who become their clients and constituents.

The general point was first made by Adam Smith, as Sowell recognizes. Here it is cast in different terms and illustrated by contemporary examples. The more remote the decision maker, the higher the "knowledge costs." Money prices, because they are "fungible," allow for versatility and adjustment, whereas government policies must be categorical rather than incremental (a notion that may surprise most students of budgeting and policy making) and require articulation "documentable to third parties" (p. 40, and passim) in such forms as legal briefs and decisions, environmental impact statements, and compliance reports. In economic transactions, consumers and investors can estimate cost-benefit ratios effectively, but as taxpayers and citizens they are in a poorer position to do so. Special interests are better able to pay the knowledge costs and to capture the benefits of political decisions for themselves while the costs are widely diffused. Whereas the market imposes severe penalties for miscalculation, political outcomes are often long-delayed, and bad outcomes are not necessarily costly to those responsible for them: politicians can avoid accountability and administrative agencies and courts are insulated from political feedback.

It is because of the trend away from reliance on the market in favor of a greater role for centralized government, Sowell asserts, that the political landscape is strewn with such wreckages of public policy as urban renewal, public housing, unemployment (because of minimum wage laws), school integration, and unwarranted subsidies to farmers, shippers, et al. All these fiascos and impositions are systemic failures, resulting from an increasingly inefficient coupling of knowledge and decision making—or, in other words, from the substitution of government for the free market.

One need not deny that American government has weaknesses contributing to failures of policy to find Sowell's analysis of the causes of and remedies for these problems incomplete and one-sided. A full critique of his collage of arguments and examples would require at the very least a lengthy summary of much that has been written about the economic and moral deficiencies of unregulated capitalism, and the need for state intervention to maintain economic stability and assure equal freedom in social relations. For present purposes, a few key points must suffice.

The notion that market transactions are voluntary and therefore not coercive conveniently ignores the embeddedness of private property and inequalities of wealth in a system of political sanctions. If such sanctions are coercive, as Sowell claims they are, then so are the property relationships they enforce. Monopolies and oligopolies, moreover, enjoy a "market power" (to set prices, influence tastes, limit options, and affect community welfare) which, contrary to Sowell (p. 118), is not merely a metaphorical form of coercion.

Nor is government coercion necessarily antithetical to freedom. In a constitutional democracy, coercion may be used only to enforce laws collectively self-imposed. The right to participate in the making of these laws and the right to be shielded against abuses in their application emanate from the same underlying commitment to freedom that allows for market transactions. The coercion exercised by totalitarian regimes is wholly different in character, because it is not authorized by laws freely adopted by the citizenry and because its use is not limited by any guarantees of individual rights. Sowell's claim that the difference between totalitarian and democratic regimes is in the degree of coercion practiced obscures this vital qualitative difference and the practical consequences flowing from it.

Many of the examples Sowell cites illustrate only half the truth at most. To insist that apart from states only trade unions "often use force, threats and harassment" (p. 175) is to indulge a bias: the same could be said of businesses involved in labor disputes. The political insulation of courts and administrative agencies is a problem in some respects, but the independence and impartiality it promotes are socially beneficial, as the framers of the Constitution anticipated. Nor is it wholly correct to claim that regulatory agencies are invariably and entirely the captives of their clients or that they are immune from criticism—witness the recent harsh attacks in Congress against the Federal Trade Commission for stepping on the toes of too many business people.

And what of the role of intellectuals in the past, and of the "new class" in the supposed conflict between the despotic state and the free market? As to the past, Sowell's tendentious history of intellectuals loses a legion of stalwart libertarians, including Cicero, Milton, Locke, the Mills, de Tocqueville, Thoreau, and Adam Smith himself, but manages to include Adolf Hitler (p. 352). With respect to the present, his indictment concentrates on the lucubrations of left-wing literati and ignores the impact of such suppliers of policy-relevant knowledge as the Keynesian technocrats (econocrats?), defense strategists, policy analysts, and engineers and natural scientists, whose work has become increasingly influential in the making of foreign and domestic policy, and who are hardly all Bolsheviks. If all academics are counted as intellectuals, a picture very different from Sowell's emerges from survey data. A 1977 poll by E. C. Ladd and S. M. Lipset (Lipset, "The New Class and the Professoriate," *Society*, Jan.-Feb. 1979, pp. 31-38) showed that 81 percent of all American faculty members favor free enterprise and 69 percent fear the expansion of the role of government. Most of the "new class," it would appear, is not exactly ready to rush to the barricades.

For old and new conservatives, then, this book will serve as a heady highbrow tract. For others, including myself, who have learned to respect Sowell's commitment to honesty and realism, it is a disappointment because its diagnosis is so faulty as to be, in his own words, "an obstacle to improvement."

SANFORD A. LAKOFF

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Naturalism and Social Science: A Post-Empiricist Philosophy of Social Science. By David Thomas. (Cambridge: Cambridge University Press, 1979. Pp. vii + 213. $32.50, cloth; $11.50, paper.)

Naturalism—the doctrine that social phenomena are in certain respects like natural phenomena and can be explained in much the same fashion—has not wanted for critics. In the late nineteenth century Dilthey and Neo-Kantians like Windelband held that the *Geisteswissenschaften* (a translator's rendering of J. S. Mill's "moral sciences" into German), having sentient subjects as "objects" of investigation, required a distinctive method, that of "understanding" or *Verstehen*. This view, which can be traced back to Vico and Herder, was later adopted and refined by neo-idealists like Collingwood and Dray, and Wittgensteinian "ordinary language" philosophers like Peter Winch.

Anti-naturalism is not without its own problems. How, for example, might we understand the actions and practices of people who appear to systematically misunderstand themselves and their situation? (This is of course the old Marxian problem of "false consciousness.") Moreover, how can we begin to explain someone's behavior "in his own terms" if the very description he supplies—that he is "exorcising demons" or "making rain"—is suspect or patently false? And, if we seek to explain behavior in terms of the agent's own reasons, aims and intentions, how can we hope to account for the unintended consequences of purposive action? Questions like these prompted Weber to attempt to synthesize naturalist and *verstehende* approaches, and his lead has since been followed to good effect by G. H. Von Wright, Jürgen Habermas, and Donald Moon (see his contribution to the *Handbook of Political Science*, eds. Polsby and Greenstein [Addison-Wesley, 1975] and his and Brian Fay's "What Would an Adequate Philosophy of Social Science Look like?" *Philosophy of the Social Sciences*, Sept. 1977).

Unfortunately, these recent attempts at synthesis get short shrift in David Thomas' *Naturalism and Social Science*, where they are dismissed or ignored altogether. Briefly, Thomas wishes to defend a "post-empiricist philosophy of social science" based upon new developments in the philosophy of the natural sciences. This new approach recognizes the importance of certain concepts and categories—e.g., "meaning"—which were virtually ignored by the older "empiricist" (or positivist) approach. But Thomas maintains that "meaning" is not the exclusive province of the agents whose behavior is being investigated; it belongs also, and more importantly, to the inquirers who, with the aid of one or another theory, conduct their investigations according to scientific criteria of significance or meaningfulness. It is not enough, he argues, to provide a culturally or contextually meaningful account of an action or practice; the social scientist's task is to render it scientifically meaningful via a transcultural theory of (say) social structure. The latter enterprise takes precedence over the former: "the observer must ultimately be prepared to impose his conceptual framework on the objects and actors under study" (p. 106). Thomas reaches this conclusion by way of a series of critiques of anti-naturalist arguments advanced by Winch and others. As he shows, even ostensible anti-naturalists like Winch require some sort of trans-contextual interpretive framework or theory. Commonsense "understanding" of "meaning" is no substitute for a theoretically grounded scientific understanding of social phenomena.

Thomas' book is rich in philosophical argumentation and in apt social-scientific illustration. Yet it is an uneven book, lucid in places and densely opaque in others (much of it still reads like the dissertation it once was). Moreover, Thomas makes things too easy for hiself by retreading, however intelligently, well-worn ground. Surely earlier interpretivists like Winch and Collingwood are easier targets than are later "hermeneutic" theorists like Hans-Georg Gadamer—which, incidentally, is why Habermas' critique of Gadamer's *Warheit und Methode* (Mohr, 1965), in his *Zur Logik der Sozialwissenchaften* (Suhrkamp, 1970) is likely to be of greater contemporary interest than Thomas' criticisms of an earlier generation of anti-naturalists. (An English translation of Habermas' critique can be found in *Understanding and Social Inquiry*, eds. Fred R. Dallmayr and Thomas A. McCarthy [Notre Dame, 1977, pp. 355-63].) But alas, Habermas is mentioned only in passing, and Gadamer not at all. (Is Britain, philosophically speaking, still an island fortress?) Still, despite its several shortcomings, *Naturalism and Social Science* repays a close and careful reading.

TERENCE BALL

University of Minnesota

John Locke: Economist and Social Scientist. By Karen Iversen Vaughn. (Chicago: University of Chicago Press, 1980). Pp. xiv + 178. $14.50.)

Despite the wealth of material published in recent years on the thought of John Locke, there has been no systematic attempt to analyze his formal economic theory and its relationship to his political thought. Indeed, many political theorists seem to be unaware that the champion of early liberal ideology was also a major contributor to the economic debates of the seventeenth century, and an important figure in the history of modern economic analysis. In *John Locke: Economist and Social Scientist*, Karen Vaughn attempts to redress this imbalance in current Lockean scholarship by providing a new perspective for understanding the nature of Lockean economic theory and its connection to Lockean political philosophy.

According to Vaughn, the foundation to Locke's economic analysis was his theory of value. "It was his tool, his one model for dealing with all economic problems" (p. 17). Significantly, this theory of value was not the famous labor theory of value that is found in *The Second Treatrise*, nor was it an early version of the supply-and-demand analysis that is the basis of modern micro-economics. On the contrary, it was, as Vaughn explains, a theory of value which was creatively derived form a comparison of the proportions between the quantities and the vents of goods bought and sold on the open market, and relied heavily on both Aristotelian doctrine and current scientific language for its formation.

The significance of this theory of value to Locke's thought was twofold, argues Vaughn. First, it provided him with an explanation of the price of commodities and a theory of money, of interest, of rent, and of foreign exchange. Second, and from Vaughn's perspective just as importantly, it provided him with a set of economic principles for objectively understanding the operations of a market economy. Indeed, Vaughn concludes that "by adopting a disinterested scientific approach to his subject," Locke was able "to arrive at a set of principles describing the behavior of essential economic variables and thereby to provide men with more reliable knowledge of how to increase their national wealth" (p. 136).

Following the inquiry into Locke's theory of value, much of Vaughn's study centers around an attempt to show how the principles found in Locke's economic writings complemented those found in his political writings and ultimately reflected a "comprehensive theory of society" which was based on an understanding of human nature and which presumed some natural order. For example, in a fasscinating account of Locke's theories of interest, of rent, and of foreign exchange, Vaughn explains how Locke's political belief in limited government neatly dovetailed with his economic belief that the government could not arbitrarily intervene in the economy without facing severe unintended consequences. This general line of argument is pursued further in her discussion of the *Two Treatises*. Vaughn goes as far as to argue that "Locke's theory of the origin and purpose of government rested in large part on his understanding of the nature and consequences of economic activity" (p. 111).

An unsatisfactory aspect to Vaughn's book is her attempt to establish the fact that John Locke was a "social scientist." While she provides a rather straightforward definition of social science as "an attempt to explain human social interaction in a systematic way, based on assumptions about the characteristics of individual human beings" (p. xii), she makes little attempt to link up the "social science" of Locke's economic and political writings to the theory of knowledge or the theory of human behavior found in Locke's *Essay Concerning Human Understanding*. Without a serious consideration of the *Essay*, any discussion of Locke as a social scientist is necessarily incomplete. This problem aside, Vaughn's book is an important and useful addition to the growing body of Lockean scholarship.

EDWARD J. HARPHAM

University of Houston

Margins for Survival: Overcoming Political Limits in Steering Technology. By Edward Wenk, Jr. (New York: Pergamon Press, 1979. Pp. xiii + 199. $26.00, cloth; $10.50, paper.)

In the past several decades developed and developing countries have been increasingly transformed into "technological societies" (Jacques Ellul) or "post-industrial societies" (Daniel Bell). Technology has become a virtual elixir for solving the problems of war, crime, poverty, energy, nutrition, and transportation, among others. New technological developments have been used by national governments both as engines of social change and for social control. Increasingly, however, observers are growing concerned about the negative influences of technology on society.

While Edward Wenk does not accept the most negative interpretations of technological development, neither is he overly optimistic. *Margins for Survival* is a personal essay on the study of the future—a future that may never occur because the current policy apparatus seems to be more attuned to short-run concerns. The book can be divided into two parts. The first contains an inventory of threats to survival, ranging from nuclear terror to the loss of freedom, as the subtitle to chapter 2 indicates. Most of these threats involve a major technology that, if developed, would yield both benefits and adverse consequences. Here Wenk turns the analysis on its head. Whereas most writers treat technology as an independent variable, studying its impact on society, Wenk is more interested in the impact of society on technological development. It is his thesis that "political limits" are more important to this process than market signals or mechanisms. Government defines "the relative responsibilities of public and private sectors, the selection of benefits and beneficiaries, and the recruitment of public resources" (p. 57). However, one of the failures of current policy making, he argues, is the failure to incorporate signals about the future into current decisions. This is because of the fact that "the swift pace of technological change no longer matches the response time of human affairs" (p. 4). The result, says Wenk, is a series of "pathologies of the short run" which prevent us from looking ahead and anticipating the second-order consequences of our decisions.

The second part of the book, comprising chapters 6 through 11, contains a prescription for institutionalizing the future in the decision-making process. The author begins this section with a list of prescriptions for studying the future which can be found in a broad cross section of the extant literature. He couples these with his own remedy —a transformation of the cultural setting in which decisions about the future are made. Although constrained by scarcities of time, personnel, and reserves, Wenk's prescription includes the incorporation of future studies in educational curricula, a new collaboration among scientists, engineers, and citizens; the enhancement of technology assessment capabilities, the integration of anticipatory analytical capabilities across government institutions, and the creation of conflict-resolving institutions.

This book makes an important contribution to the study of decision making for the future because it makes explicit the interrelationship between technology and the political process in the form of a "technology delivery system." Wenk skillfully integrates a broad range of literature on science, technology, the political process, and the future. This book should be particularly useful for policy makers because it highlights the political problems in adopting what might seem to be rather facile technological solutions to pressing national problems. However, the book is limited in its usefulness. It is primarily a personal and prescriptive statement about decision making for the future, and is so intended by the author. The absence of an analytical framework is obvious. The proffered prescriptions are too briefly sketched to be applied, and it is not always clear why they should be preferred over the present situation. The reader would be shortsighted indeed who did not examine these prescriptions with the same critical eye which the author turns on the present policy system.

ALLYN R. BROSZ

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Biopolitics: Search for a More Human Political Science. By Thomas C. Wiegele. (Boulder, Colo.: Westview Press, 1980. Pp. xiv + 159. $17.50, cloth; $8.00, paper.)

Over the last decade, an increasing number of scholars have sought to relate the biological sciences to the study of politics. Often described as "biopolitics," this trend has been paralleled in such fields as economics, sociology, anthropology, and even psychiatry.

Ten years ago, links between human behavior and biology were primarily literary: the popularizations of Konrad Lorenz (*On Aggression*, Harcourt, 1966), Robert Ardrey (*African Genesis*, Atheneum, 1961, and *The Territorial Imperative*, Dell, 1968), and Desmond Morris (*The Naked Ape*, McGraw, 1967). Today, these pioneer efforts are frequently criticized for superficiality or imprecision; scholarly research in biopolitics is slowly becoming legitimate within political science itself.

Thomas Wiegele is among those who have been active in this trend, having established the Center for Biopolitical Research at Northern Illinois University. He is therefore well placed to provide a progress report on the emerging subfield.

Biopolitics: Search for a More Human Political Science deals with an area of research rather than a specific hypothesis, theory, or problem. It does not pretend to synthesize many disciplines as they relate to a single phenomenon (like Lionel Tiger's *Optimism: The Biology of Hope*, Simon, 1979). Rather, Wiegele limits his survey to recent works by political scientists, showing how our discipline can be enriched by a biological point of view.

Critics of such an approach have frequently condemned it as "reductionist" or "determinist." Wiegele's *Biopolitics* provides both an argument, and some evidence, against this conventional misinterpretation. The argument, summarized by the book's subtitle, is that "political analysts have failed to take into account the realities of political life, *at the level at which it is lived*. At this level, biological considerations interact with emotional, rational, and psychological factors within a real human being" (p. 84, italics in original). Scholars tempted to disagree would do well to reflect on how well they think with a splitting headache.

The sheer diversity of topics covered by recent "biopolitical" research is further evidence that the field is hardly reductionist or determinist. As Wiegele puts it, "Political beings are not ethereal essences floating around in an intellectual construct called a political system. Because human nature is rational, psychological, *and* physical, analysts of political behavior must begin to conceive of the subjects of their investigations as biological beings with intellective capacities. . . . Thus it is incumbent upon us to add a biological perspective to our work; in doing so we need not engage in a blind acceptance of a biological determinism that pays no heed to human purposive activities" (p. 145).

For example, Wiegele's own research (summarized on pp. 92-95 and 116-19) shows how new approaches inspired by the life sciences can enrich traditional political science. By treating physiological stress as a vital—and measurable—factor in the behavior of political leaders, our understanding of both domestic politics and international crisis is greatly increased.

The reader may be frustrated by the lack of depth in the treatment of each of the many issues touched upon in this slim volume. But to gain a thorough background in biopolitical research, by definition, one must study specific areas in an interdisciplinary perspective. And here, Wiegele's intention is to survey *only* the work of political scientists, rather than to follow any one line of research to its completion.

Because *Biopolitics* considers only recent work, it does not stress the inclusion of biological factors in political theory from Aristotle to Rousseau, Marx and Kropotkin. As with any survey, one can complain about the omission of some authors (Henry Beck, Elliott White, Claude Phillips, and William Ophuls come to mind). Perhaps more important, Wiegele fails to indicate that the parallels between inclusive fitness theory in biology and collective goods or public choice theory, already noted by economists like Gordon Tullock and Jack Hirschleifer, cannot fail to have an effect in political science.

But such caveats only strengthen the main point of *Biopolitics*. As Wiegele says in his conclusion, "It is no longer a question of *whether* we should incorporate human life science data into our studies of human social behavior; that is already being done by the life scientists. . . . The more crucial questions are: who is best equipped to study human social behavior, the life scientist or the social scientist, and who will have a deeper appreciation of humanist values, the life scientist or the social scientist?" (p. 155).

ROGER D. MASTERS

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Power: Its Forms, Bases and Uses. By Dennis H. Wrong. (New York: Harper and Row, 1979. Pp. viii + 236. $28.50, cloth; $6.95, paper.)

When asked how he distinguished a mathematician from one merely proficient at mathematics, Norbert Weiner replied, "By the problems he chooses to work on." The same standard can be applied to social scientists concerned with political analysis, although the results are usually discouraging, especially lately. Nonetheless, by this and several other basic criteria, Dennis Wrong's study of power comes off well. It is an important work on a most important topic—perhaps the core concern of political science addressed by a well-known sociologist.

The book is accurately titled. It deals primarily with the forms (types), bases (resources), and uses (social significance) of power. It is not and does not purport to be a comprehensive discussion of all the major topics of power analysis. Largely excluded from systematic scrutiny are such central matters as the epistemo-scientific status of the concept of power, various key aspects of power relations, many features of the psychology of power (especially cognition), the measurement of power and power structures. Useful comments are made regarding some of these topics, but they are less developed than the dimensions designated in the title.

With some fanfare, Wrong builds intention into his conception of power, defining it as "the capacity of some persons to produce intended and foreseen effects on others" (p. 2). However, power is not for him the most general conception of interactor causation. He allows a residual set of "unintended influence" relations which, together with power, make up the total realm of "influence." Thus some analysts may feel that merely labeling preferences are involved. However, Wrong insists both early and late in his book that his "influence" (others' "power") is tantamount to any social interaction and therefore too general a topic to pursue fruitfully. He sees such a notion as equivalent to the subject matter of all social science. On the contrary, however, one might either demur to the charge or contend that power (Wrong's "influence") is merely one aspect of all social interaction—focusing on its causal features. One can also focus, for example, upon its economic, communication, prestige, or emotional features. In general, the question of the likely boundaries of the phenomenal realm related to power for which a relatively coherent body of concepts and theories can apparently be developed demands a more extensive and penetrating analysis than Wrong attempts. Fortunately, most of Wrong's other comments apply very well to partly intended, non-intended and anti-intended power relations.

Wrong categorizes power into four types: force, manipulation, persuasion and authority. Authority is very broadly conceived, à la Easton, as "successful ordering or forbidding" (command-obedience), regardless of legitimacy (p. 35). It is then further categorized into coercive, induced, legitimate, competent, and personal types, the last two essentially being French's and Raven's "expert" and "referent" power, respectively. In the process, Wrong makes a number of judicious comments not only about these various types of power and authority, but also about the recalcitrant problems of fit between analytic distinctions and concrete reality. Consequently, though one may find portions of this particular typology uncongenial (e.g., the confusion of hybrid types such as manipulation and authority with more fundamental types such as force or reward), one may still profit significantly from the discussion.

An outstanding feature of the book is Wrong's analysis of what he calls the "combinations and interrelations" of the forms or types of power—i.e., "regular 'tendential laws' of transition from one form to another" (p. 66). For example, he hypothesizes, with illustrations, that regular persuasion is likely to become personal or competent authority, that regular reward is likely to become seen as threat of punishment, that continuing coercion tends to become legitimized by both influencer and influencee, and that there is an ineluctable interplay between force and the threat of force. Like most of the book, this discussion is very general and essentially speculative, paying meager attention to empirical confirmation or intermediate elaboration; but it is also sophisticated, stimulating and sensible.

After the forms (types) of power, the next largest focus of attention is resources—a topic very feebly treated in the literature, with Baldwin's work (not noted) the main exception. Like his predecessors, Wrong unhappily makes no attempt to define or explicate the elusive notion of a power resource. (Doing so may reveal many resource-invoking political hypotheses as patently tautological.) He concentrates on "liquidity" ("fungibility"), on the problem of "objective interests" (well handled), and, particularly effectively, on collective as opposed to individual resources. Solidarity and organization are seen as the two most crucial collective resources and furnish the analytic basis for an interesting perspective on political mobilization.

Rather naturally, the very range and difficulty of the discussion exposes flaws. The two-edged nature of "anticipated reactions" (democratic and anti-democratic) is aptly noted, but too much is made of difficulties for power analysis posed by anticipated reactions (no reference is made to March's telling analysis). The discussion of power motivation neglects most of the psychological writing on the topic (e.g., Winter and McClelland). The treatment of the "uses" of power deals mainly with other matters or else handles such concerns as power structures or the zero-sum problem less than profoundly. The discussion strives to be multi-disciplinary but fails to do justice to the contributions to power analysis of either political science or social psychology. On the other hand, Wrong performs a very useful service for many readers by calling American attention to many of the British and Commonwealth writers on power who are frequently ignored.

The overall orientation of the book, understandably, is heavily sociological. Marx, Weber, Dahrendorf, Parsons, Mills and company are constant points of reference, sometimes helpfully, as with Marx and Weber, sometimes rather fruitlessly, as with Parsons, who was not a very effective power analyst. But if one allows for the limitations of different disciplinary perspectives, one can gain from them rich insights not forthcoming through familiar eyes. Wrong has given us a thoughtful, liberal and provocative analysis of many key features of an utterly vital phenomenon. Since Lasswell's and Kaplan's *Power and Society* (Yale, 1950), few works on power of its range and depth have appeared in any discipline. One may wish to argue with the author about many of his particular positions, but one cannot help but profit from exposure to them.

FREDERICK W. FREY

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American Politics

Friend and Foe in the U.S. Senate. By Ross K. Baker. (New York: Free Press, 1980. Pp. xiii + 301. $12.50.)

Ross Baker, former Senate staffer and currently a professor at Rutgers, has written a book on friendship and enmity in the U.S. Senate. Much of the book is devoted to the types of "friendship" he finds, based on interviews with 25 past and present senators and secondary sources dealing with a wide span of Senate history. The most common form of friendship found is that of "institutional kinship," which involves the expectations of pairs of senators that their colleagues will be restrained, diligent, empathetic, and trustworthy. Baker suggests that relationships between individual senators, based on these expectations, are bonding agents that allow the Senate to function, despite somewhat weakened general norms. Baker describes other forms of friendships between pairs of senators including "political alliance," the least intimate and personal form of friendship except for institutional kinship, "social friendship," both intramural and extramural (occurring within or also outside of the Senate, respectively), and "pure friendship," which is closest to what we most often mean by the term friendship in our own daily lives. As one moves along the typology from institutional kinship to pure friendship, we find, not surprisingly, fewer cases.

Baker examines the factors that seem to contribute to the friendships found in a Senate he sees as competitive and individualistic. Not surprisingly, shared political values are important in personal and political friendships; however, they do *not* assure friendship. For example, Baker points out the strong conflicts that sometimes occur between senators of the same state and party, as with Hatfield and Packwood of Oregon, and also shows that political and ideological compatibles are not always close. He also shows how common social ties, and especially religion, can facilitate friendship. In fact, the strongest contemporary pure friendship case he finds involves four senators who are "Evangelical Christians," Nunn, Chiles, Bartlett, and Domenici. Unfortunately, a substantial part of the author's information on these pure friendships—and, for that matter, on feuds between senators—is based on his speculation combined with secondary sources whose validity we cannot readily assess. Baker also looks at the political significance of the types of friendships he sees. He finds the Senate's institutional cohesion facilitated, especially by institutional kinship, political alliance, and intramural friendships. He suggests how intramural friendships can provide, to use Hubert Humphrey's phrase, "wiggle room." He presents one contemporary case where pure friendship actually resulted in successful persuasion, senator to senator. However, Baker is careful to point out that senators are not likely to hurt themselves or their state for a friend and are not expected to; he also suggests they shouldn't. From a political perspective, Baker argues, deeper friendships between senators have diminishing marginal returns, which many senators appear to realize. The growing politicization of a senator's social life, as evidenced in fund raisers and politically motivated receptions, also seems to limit opportunities and incentives for deeper friendships within the body as does the Senate work load and rivalry between colleagues. This is evident in the judgments of House alumni whom Baker interviewed, who found it harder to form close friendships in the "upper body" than in the House.

Baker's chapter on senatorial strife is interesting. He lists the personal traits senators most dislike (there were no surprises) and the potential sources of feuds, including jurisdictional conflict, bill or issue thievery, intervention in a colleague's election, efforts to gain the presidency, and leadership fights. Most important, however, Baker finds a lack of consistent overt gestures of malice among the feuding and suggests they are too costly and not worth the trouble. Rather, feuds are more likely to lead to a disinclination to accommodate where a senator would not be hurting himself, his state, or his positions by so doing. Baker's chapter on lonership and leadership shows how some personal, though not political loners (Borah, Taft, Knowland, and Byrd) have been selected as leaders and speculates why this occurs, but his discussion of what senators want in their leaders contains little that is new.

Baker's book is well written and will be of some interest to the devotee of Congress, but it contains little that is important *and* new. Though this is a book on friend and foe in the Senate, the humanness of that institution is not adequately captured, perhaps because senators are somewhat private in what they're willing to share and how they do so. The book is summarized quite nicely and usefully in the last chapter. However, most of what is significant in the work is already known by congressional scholars. The book reads well and is enjoyable in places. Regrettably, that can be said of too few other works we read.

SIDNEY WALDMAN

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The Pulse of Politics: Electing Presidents in the Media Age. By James David Barber. (New York: W.W. Norton, 1980. Pp. 342. $14.95.)

By the time this review was prepared, *The Pulse of Politics* had already been promoted by announcements on National Public Radio, discussed by its author on such television shows as *Good Morning America,* and commented upon in both Sunday supplements and journalism reviews. And by the time this review appears a variety of scholarly judgments will already have been pronounced. Hence a descriptive rehash of Barber's thesis may be unnecessary. Put briefly, it is that the pulse of presidential campaign politics beats in a 12-year rhythm, distinctive reverberations sounding each 4 years during the presidential campaigns. First pounds forth the politics of conflict—i.e., the presidential campaign is a battle for power (1900, 1912, 1924, 1936, 1948, 1960, 1972). But the nation tires of battle and seeks redemption; hence the next pulsation is the politics of conscience (1904, 1916, 1928, 1940, 1952, 1964, 1976). But the national spirit is not soothed, cleavages do not vanish, selfish interests do not recede. So four years later appeals for harmony and happiness echo through the campaign, the politics of conciliation (1908, 1920, 1932, 1944, 1956, 1968, 1980). Conflict, conscience, conciliation—and as Sonny and Cher once sang, "The Beat Goes On." That pulse is most readily detected in the news media. The journalist, writes Barber, is a storyteller. And every four years the news media sound a dramatic theme for each presidential campaign—first the war story, then the morality play, finally the romantic comedy.

Cyclical theories of politics have had their critics since at least the time of Polybius. Detractors note that cyclical theorists occasionally and unintentionally interpret events as fitting one cycle period when it would be just as easy to identify those events with a different period of the cycle. *The Pulse of Politics* has this tendency. Granted that Barber marshals considerable historical evidence to justify, say, labeling the 1912 campaign as conflictual, or 1928 as the politics of conscience, or 1956 as conciliation. Sometimes, however, the proof lies in redefining the personae of the campaigns or the settings of the campaign. Thus the 1900 campaign pitted not the "sweet-tempered" incumbent, William McKinley, against the Democrat's William Jennings Bryan, but McKinley's stand-in, Vice-President Theodore Roosevelt, against the silver-tongued orator. The combative Roosevelt set the tone for a politics of conflict, at least in news accounts. Apparently Teddy Roosevelt set the same tone in 1912, for the ex-professor (and four years later the moralist) Woodrow Wilson was eclipsed in the struggle of Taft versus Teddy.

But such matters of deciding just who were the principal candidates in specific campaigns are, as sports talk goes, "judgment calls." Depending upon the journalistic accounts examined and the historians making the calls, judgments will certainly vary. A greater problem emerges in redefining the setting of the campaign drama. In his critique of Barber's book, published in the *Columbia Journalism Review* (19: 78-79), Ralph Whitehead, Jr. noted only one "obvious clinker" in the 20 campaigns classified in *The Pulse of Politics.* Whitehead thinks that to be 1972, but does not say why. There is a reason and that reason calls into question the typing of the campaigns of 1968, 1976, and 1980 as well.

Barber notes that in 1972 Richard Nixon did little campaigning, opting instead to remain above the fray and letting surrogates speak for his record. With neither president nor surrogates in a combative mood, where was the politics of conflict to emerge, especially when the Democratic nominee was a highly moralistic "deacon from Dakota" (conscience) who sounded in his acceptance speech a plea for "reconciliation" with "Come Home, America" (conciliation)? As it turns out, the "story of politics as war" was dramatized not in the general election, but in the Democratic primaries. If this be so, however, can one easily dismiss the war for the 1968 Democratic nomination, for control of that party's nominating convention, and for control of that party during the general election? Were the news media really spinning yarns of conciliation when many readers/viewers thought it was conflict? And, what of 1976? News stories of caucus fights, primary battles, and struggles for control of the convention (first about Democrats, then Republicans) were surely as much The Big Story of 1976 as post-nomination themes of a "government as good as its people" or "he made us proud again."

Critics of cyclical theories point to another problem—i.e., a number of different cycles can be discovered to cover the same events. Different assumptions yield different cycles. In many respects *The Pulse of Politics* is to America's collective consciousness what *The Presidential Character* was to the consciousness of individual politicians. Freud's ego, superego, and id now meet Jung's archetypes. But there are other cyclical bases. What if some religious mythology is correct, for example, and we are bound by an endless cycle of sin, suffering, sacrifice, and salvation; how does that cycle fit the politics of 1964, 1968, 1972, and 1976, or each of the 16-year cycles preceding? Or consider Peter Kaplan's semi-tongue-in-cheek version of America's political cycles (*Esquire,* April, 1980, pp. 41-47). He suggests that we may alternate "soft" and "hard" eras of undetermined length. The late 1960s and the 1970s were "soft-line," i.e., vulnerability was the code word, self-indulgence and self-involvement the styles. The final act in the soft-line drama, thinks Kaplan, came in July, 1979, when Jimmy Carter asked Americans, "Whenever you have a chance, say something good about our country." But then came hostages in Iran, Soviets in Afghanistan, inflation, recession. Toughness, belt-tightening, and team play—the hard line—was in vogue again. A new, hard-line Carter and a hard-line Reagan contested for the presidency in 1980.

Cyclical theories, then, have their problems. In the face of them, Barber must be admired for advancing a new cyclical theory of presidential politics. It is imaginative and in many instances persuasive. There are ways, however, in which the overall case might have been even more persuasive. One wishes, for example, that Barber might have addressed directly how his vision of American political events contrasts with those of key historians such as those expressed by Richard Hofstadter in various volumes. And students of voting behavior might wonder how Barber's classification of presidential elections matches with those of *The American Voter,* V. O. Key, Jr., Gerald Pomper, or Charles Sellers. How do voters respond to what Barber calls "the interplay of Presidential campaigners and the journalists who tell their running stories?" If the cycle of conflict, conscience, and conciliation does not, as Barber says, "point out the winner," how does the cycle interact with factors which voting studies say influence electoral outcomes? Moreover, is what candidates actually do, journalists' reports, or both the key to making an election one of conflict, conscience, or conciliation? John Carey has noted (*Journal of Communication* 26: 50-57) that journalists write more about the meta-campaign than the campaign. Which stories define the pulse of the election? Finally, just as the pulse metaphor runs throughout the book, so does the dramatic motif. Yet there is a sense that dramatistic analysis in the tradition of communication scholars (Kenneth Burke, Hugh Duncan), sociologists (Georg Simmel, Peter Berger, Erving Goffman), or political scientists (Murray Edelman, Richard Merelman, James Combs) was deemed unrelated to the drama of electing presidents in the media age. That was unfortunate.

Barber believes that the cycle that is the pulse of politics can be broken and that it is a key responsibility of journalists to do it. Not only must all three stories—conflict, conscience, and conciliation—be told in each election, but also each must have greater depth: underlying facts yielding conflicting visions should be revealed, the characters of candidates seeking to guide the nation's conscience should be explored, and the nature of the coalitions flowing from conciliations should be analyzed. Should the cycle be broken, it is hard to say whether life would be softer or harder for presidential candidates. Given the current political environment (July, 1980), perhaps things are already out of synchronization. At least the active-positive incumbent might well wonder if 1980 is a campaign of conciliation.

DAN NIMMO

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Judicial Review and the National Political Process: A Functional Reconsideration of the Role of the Supreme Court. By Jesse H. Choper. (Chicago: University of Chicago Press, 1980. Pp. xviii + 494. $28.50.)

Jesse Choper has blended democratic theory, constitutional history, and political science in this brilliant "functional reconsideration" of the Supreme Court. Choper's initial chapter addresses the conundrum that has vexed constitutional scholars—reconciling the place of the Court in a democratic society. Not since Leonard Levy's essay of over a decade ago have the various tensions inherent in (and possible justifications for) the Court's role in a majoritarian political system been examined so elegantly. Moreover, drawing upon the wealth of modern political science analyses of Congress and the presidency, Choper reviews the undemocratic aspects of the other branches. He concludes that, although the other branches are slightly bent in undemocratic directions, there is a central, undeniable, and upright core of popular responsibility. The Court is neither as democratic nor subject to popular restraint.

Given the Court's undemocratic nature, Choper seeks to define its proper role. But rather than deal with the substantive results of the Court's decisions. Choper attempts to prescribe a standard of justiciability—to distinguish judicial circumstances requiring the Court's involvement from those in which the Court's judgment is unnecessary or counterproductive. Choper's prescription, in the form of four "proposals," is the result of a complex consideration of the realities of the national political process, the reversibility of the Court's action, and the likely drain upon the finite resources the Court possesses for enforcing its own decrees.

Thus, in his "Individual Rights Proposal," Choper argues that the overriding justification for the exercise of judicial review is the protection of individual liberties. The essential rationale for this position is well known—such a role for the judiciary is contemplated by history, reinforced by modern judicial opinion, and functionally justified to protect the unpopular and unrepresented minorities in a majoritarian system.

This is not the case for questions involving national infringements upon state powers, however. As Choper shows, the states are well represented in the national political arena and have many opportunities in the legislative process to impose their vetoes. Moreover, on those occasions when the Court has issued "states' rights" decisions, it has been the victim of what Hughes called "self-inflicted wounds" to its public standing. Thus Choper's "Federalism Proposal" advises that such conflicts be held nonjusticiable and left for political resolution. On the other hand, Choper does envision federal judicial oversight of state actions that might threaten the exercise of constitutionally delegated national powers. Not only does the lack of national representation in the state legislatures justify this role, but also the Court's actions are easily reversed by a Congress dissatisfied with the justices' defense of its powers.

Choper's "Separation Proposal" requires the Court to abstain from most of the battles between Congress and the president. Each branch has jealously guarded its powers and possesses the weapons necessary to preserve its prerogatives. Not only is the Court's involvement in such inter-branch disputes unnecessary, but should either branch flagrantly exceed the powers assigned to it, "there is scant reason to believe that the Court will succeed in halting the march" (p. 305).

Nevertheless, Choper does reserve for the Court the power to review attempted restrictions or enlargements of the judicial power by the other branches. Choper premises this "Judicial

Proposal" upon the "distinctive ability of the Justices" to define their power (p. 384), the Court's inability to protect its jurisdictional boundaries in the national political process, and the experience of two centuries.

The only weakness of the book rests upon a well-worn path. Although the rationale for the Court's protection of the rights of the minority is well understood, defining the minorities and the specific rights warranting protection is a "demanding and delicate" task (p. 64) that Choper only recognizes as "awesomely perplexing" but does not undertake (p. 77). Nor does he satisfactorily address the criticism that though the Court is in the best position to protect individual liberty, there is no guarantee it will assume that posture. Although Choper clearly expects the Court's substantive decisions to be made "in behalf of personal constitutional liberties," he admittedly offers no advice to the Court on "how this superintendence should be carried out" nor any guarantees to the rest of us that it will be carried out at all (p. 79).

Otherwise, the book's strengths are undeniable. Choper carves careful exceptions in each proposal to allow the Court to exercise its involvement when questions of individual liberty overlap with but stand independently of questions of federalism, separation of powers, or judicial power. And each proposal is examined rigorously and subjected to equally stringent self-criticism, tested in a variety of past and hypothetical constitutional scenarios. With his review of the modern Court's defense of individual rights against state infringement, Choper for the first time musters empirical evidence to counter the criticism that the Court has rarely served as the protector of individual liberty. And Choper's subsequent review of noncompliance with the Court's rulings suggests that the Court expends large sums of what he calls its "exhaustible institutional capital" to resolve disputes in which its intervention is unnecessary and which at the same time threaten its enforcement of individual rights decisions.

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Constitutional Government in America. Edited by Ronald K. L. Collins. (Durham, N.C.: Carolina Academic Press, 1980. Pp. xiii + 504. $24.95.)

This hefty volume consists of essays and commentaries presented at Southwestern University Law Review's first west-coast conference on constitutional law, held in September, 1977. The purpose was to commemorate the 190th anniversary of the writing of the federal Constitution. The 36 contributors include 16 law professors, 6 lawyers, 5 judges, 3 history professors, 2 professors of political science, a public official, a magazine editor, and 2 professors of things in general. Thus the views expressed in this symposium are, for the most part, those of professional legal scholars.

The volume deals with a very wide variety of subjects. Six essays are devoted to First Amendment problems, 2 with rights of privacy, 3 with criminal justice, 2 with due process, 8 with equal protection questions, 3 with access to the federal courts, 3 with federalism, one with land use, 3 with some aspects of constitutional history, and 3 constitute a brief debate on judicial review. Speaking generally, as one of the authors suggests, constitutional law has not been a very important subject until fairly recent times, and the growing attention devoted to constitutional law analysis is evidence of the ever-heightened stature of constitutional interpretation in our governmental system. The heavy emphasis on the concept of the equal protection of the law suggests its centrality in current American constitutional law. Even so, it is a fair guess that this too will pass, and that at some future time, when the essential facts of life have changed, some other legal doctrine will take center stage. For if there is any single theme in this diverse collection of essays, it is that fundamentally the Constitution has survived and served us well as the jumping-off point for so much complex and lively debate because its brevity and the generality of so many of its key provisions have supplied the element of flexibility which is the essential predicate of constitutional interpretation. However one may choose to assess various positions advanced in this book, no one can possibly miss the point that these scholars are talking about a living constitution and that constitutional interpretation is something more than an exercise in antiquarianism.

A volume of conference papers delivered by so many diverse people is bound to be something of a potpourri. Some essays are very substantial and fully annotated; others are little more than brief informal comments. Furthermore, while this purports to be a collection of essays on constitutional law, some are not really in this category except in a very loose sense, such as Donald McDonald's essay on the press or Alan F. Westin's essay on privacy. In addition, while major topics of constitutional law are ignored or touched upon only tangentially, such as the commerce power, the taxing power, interstate relations, the powers of the president, and the control of foreign relations, there is an essay on due process limitations on the impeachment power and one on the constitutional limits on land use control (zoning). Incidentally, the land-use essay, by R. Marlin Smith, is very well done, and well worth reading.

It is to be expected that in a large miscellany of this sort, one runs into observations which lead to eyebrow-raising, at least on my part. Thus, William M. Kunstler asserts that "the search and seizure protections of the fourth amendment . . . have been virtually destroyed" (p. 305). This strikes me as a very considerable exaggeration. To be sure, the Burger Court has trimmed down on the exclusionary rule, but this hardly warrants Kunstler's gloomy conclusion. In this connection, attention should be drawn to the essay on the exclusionary rule by Judge James L. Oakes, of the U.S. Court of Appeals for the Second Circuit, which presents a favorable analysis of this much-debated topic. To cite another example, Carole E. Goldberg-Ambrose asserts that "the Supreme Court has taken special pains to protect federal jurisdiction from congressional enlargement" (p. 321). This struck me as a dubious observation; if indeed the Court has taken such pains to prevent Congress from enlarging federal jurisdiction, it has certainly failed in its objective. Another example is a remark made by John P. Frank that Justice Blackmun is "very much a heavy-weight in the present scheme of things" (p. 478). With all due respect to John Frank, who is one of our leading and most learned Court-watchers, and to Justice Blackmun, who for all I know is a fine man, it never crossed my mind to describe him as an intellectual heavyweight. On the other hand, if Frank means that he is a heavyweight when compared to his current colleagues, then I must say that this is a perfectly devastating description of the quality of the sitting justices. Finally, it is inevitable, I suppose, that in a book of this sort, a great deal of effort is devoted to proving things that no longer require proof. For example, taking as his starting point the deaths of Justices Murphy and Rutledge in 1949, Irving Dilliard seeks to demonstrate that changes in the personnel of the Court have an effect on the course of its decisions (pp. 447-56). I suspect that at this late date this is a self-evident proposition which requires no further proof.

The first six papers in this book are concerned with various aspects of First Amendment freedoms. The best of the lot is a learned, well-argued essay by Steven Shiffrin on defamatory nonmedia speech. On the other hand, the equally exhaustive and fully annotated essay by C. Edwin Baker on the scope of First Amendment freedoms raises some questions in my mind. His demolition of the classic marketplace-of-ideas theory is worthy of the attention of all constitutional law scholars, but his brief analysis of religious freedom strikes what seems to me a very dubious note. Starting with the assertion that the Court wrongly decided the polygamy case, *Reynolds* v. *United States*, 98 U.S. 145 (1879), Baker then goes on to take issue with the famous remark of Justice Roberts in *Cantwell* v. *Connecticut*, 310 U.S. 296 (1940), to the effect that freedom to believe is absolute, but not the freedom to act. Then Baker asserts: "To deny conduct any protection clearly would gut the meaning of '*free exercise*' " (p. 82). A short answer to this sort of card-stacking is to point out that Roberts did not say that *no* conduct under the guise of religion is entitled to any protection. On the contrary, he said that "conduct remains subject to regulation for the protection of society." I have always assumed this to mean that where the protection of society requires it, conduct may be regulated. This seems to be a truism, that religious freedom ends where the criminal code begins. Baker says, on this point, that this is no solution "unless one can establish definite limits to the scope of criminal law" (p. 84). A short answer is that the criminal law goes as far as it goes. I must confess that I do not understand what Baker thinks he is proving with respect to religious freedom. If he means that anything goes so long as a religious tag is attached to it, then I must say that I dissent.

In addition to First Amendment freedoms, the topics that receive the heaviest emphasis are those related to federalism, the equal protection of the laws, and judicial review. In respect to federalism, Martin Shapiro says: "Within the intellectual framework of sovereignty, federalism is an absurdity, a contradiction in terms" (p. 360). That is to say, it is logically impossible to have two sovereignties involving the same people and the same territory. Of course this is true, but American federalism need not necessarily be defined in terms of two sovereignties. What makes our system federal is that the powers of the states are not mere delegations of authority by the national Congress. They are residual powers *constitutionally* reserved to the states. Without saying so in so many words, Shapiro seems to suggest that the states are largely administrative agencies of the national government, and of course however federalism is defined, it is not definable as merely a system of administrative decentralization. But the reader may take heart, for, if one keeps on reading, one will take note of Judge Charles E. Wyzanski's observation that "the greatest contribution politically made by our country has been in its preservation of federalism" (p. 490).

Most aspects of equal protection which are currently before the country are touched upon in these essays, particularly the vexing problems of affirmative action, and discrimination on the basis of sex and wealth. The brief remarks at the end of the book on judicial review add little to what has long been common knowledge on the subject.

Finally, I should like to call special attention to several absolutely first-rate essays. One is George Anastaplo's brillliant dissection of Abraham Lincolin's Emancipation Proclamation. Another is Scott H. Bice's fine essay on the law of standing, which is at present a lively subject of controversy. Still another well worth reading is the essay on administrative due process by Bernard Schwartz. Of course, several other essays in this book are works of solid scholarship. On the whole, however one may choose to cavil here and there, this is a book which will find a solid and enduring place for itself in the literature of American constitutional law.

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Energy Policy and Public Administration. Edited by Gregory A. Daneke and George K. Lagassa. (Lexington, Mass.: Lexington Books, 1980. Pp. vii + 323. $29.95.)

This book well demonstrates the pitfalls of editing a multi-authored volume. Such volumes often lack an overall coherence and reflect a situation in which individual authors (and sometimes the editors themselves) are only remotely familiar with what other contributors have written. *Energy Policy and Public Administration* is no exception; indeed, it seems to be a classic example.

One of the book's major purposes is to discuss the variables affecting the nation's energy use and to "specify realistic strategies for achieving a balanced future" (p. vii). For many of the contributors this means an advocacy of soft-path strategies emphasizing renewable energy resources at the expense of high technologies using nonrenewable resources.

Advocacy has its place, and prescription is surely in the realm of policy analysis. To be effective, however, recommendations for change ought to be consistent and certainly not contradictory. Inconsistency and contradiction serve only to confuse policy makers and to diminish the prospects for change along desired lines. The present book provides many illustrations of the latter situation. For example, one key question concerns the government's appropriate role in the energy arena. At least two authors, Walter Mead and Marc Ross, favor a market approach with very limited government intrusion. Several other authors urge much greater government involvement and many more government programs. Daneke rails against the institutional barriers that the U.S. government has created. As he puts it, the path toward the use of renewable energy resources "is cluttered with a number of institutional obstacles" (p. 20). Despite this statement, Jerry Yudelson labels as a "mythlike assumption" the belief that legal and institutional barriers prevent the development of renewable resources. Several contributors suggest the desirability of tax subsidies designed to stimulate conservation and greater use of renewable resources. In contrast, Mead stresses that past subsidies have seriously distorted the marketplace and have encouraged excessive energy consumption.

A second theme focuses on the level of government best suited for energy policy making. Daneke and some others favor decentralization and increased reliance on state and local governments. Many other contributors strongly disagree. Douglas Jones believes that decentralization has run its course; Richard Worthington sees advantages in greater centralization. In the words of Henry Harmon, "One of the seeming tragedies of the current energy crisis is that it is occurring at a time when financially strapped municipalities are least able to respond with effective . . . programs" (p. 97). In Lagassa's opinion, state control over the siting of power plants represents a "suboptimal administrative arrangement" (p. 178). Moreover, according to Hanna Cortner, state and local response to the current energy situation has not been noteworthy.

These examples of seeming contradictions are not isolated. One author claims that Congress has abdicated policy making to the Department of Energy; another observes that the department really has very little authority or responsibility. Sharp disagreement exists even about the causes and definition of the energy crisis.

In sum, if policy makers seek clarity and consistency, they are not likely to find it in this book. For others, their reading should be highly selective. They may wish to focus on the appealing chapters by Marc Ross, Sumner Myers, and Janet Johnson and Richard Sylves.

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Proportional Representation in Presidential Nominating Politics. By Paul T. David and James W. Ceasar. (Charlottesville: University Press of Virginia, 1980. Pp. xv + 298. $15.95.)

Prior to the start of the 1976 presidential nomination campaign, politicians, journalists, and political scientists alike were predicting the first brokered convention since 1952. For the Democrats, in particular, the evidence seemed to be overwhelming. First, a larger number and wider assortment of presidential contenders was expected because of the absence of an incumbent, the proliferation of presidential primaries, and the reformed delegate selection process. Second, and more importantly, the adoption of proportional systems of delegate allocation by most primary states and by all caucus states would disperse delegates widely among these many contenders, making a majority difficult to attain. As is often the case, however, the experts were proven wrong, and Jimmy Carter easily managed a first-ballot nomination. What did the experts miss? In *Proportional Representation in Presidential Nominating Politics,* Paul T. David and James W. Ceasar shed some light on this and other questions pertaining to the use of proportional representation in the delegate selection process.

The book is divided into two parts. Part 1 begins by tracing the history of proportional representation from the tumultuous 1968 convention, through the reforms of the McGovern-Fraser and Mikulski Commissions, and ends by describing the more recent recommendations of the Winograd Commission.

David and Ceasar then proceed to analyze the effects of proportional versus plurality or loophole (winner-take-all by district) systems of delegate allocation by recomputing and comparing 1976 delegate outcomes under uniform proportional and plurality plans. Two of the major loophole states, New York and Pennsylvania, are omitted from the analysis because official election returns did not indicate candidate preferences of defeated delegates.

For the Democrats, the findings confirm the expected. Proportional systems tended to fragment delegates among many contenders, giving a larger percentage of the total delegates to weaker candidates and a smaller percentage to front-runners. Plurality systems, on the other hand, tended to concentrate delegates among the leaders and to short-change weaker or fringe candidates.

Given only a two-man race, and the exclusion of New York and Pennsylvania from the analysis, the differences on the Republican side are much smaller and run counter to expectations. Surprisingly, Ford was weakest and Reagan was strongest under the actual mix of proportional and plurality primaries. Under either uniform proportional or plurality systems, Ford would have gained about 20 delegates and Reagan would have lost about 20.

The authors note that the findings for the Republicans would revert to form if New York and Pennsylvania could have been included in the analysis. With New York's and Pennsylvania's delegations included as pro-Ford instead of uncommitted, the majority candidate (Ford) would have received a major boost from a pure plurality system. Under pure proportionality, on the other hand, Reagan (the minority candidate) would have clearly benefited, since he probably would have contested both states and probably would have won a share of both states' delegations. The authors further postulate that "if Reagan had received a share as small as 24 percent in New York and Pennsylvania—and if all other things had remained the same—he would have been the nominee" (p. 56). As David and Ceasar readily admit, these are mighty big "ifs," but they do not detract from their more general conclusion that proportionality "assists" and plurality systems "penalize" the conservative minority within the Republican party.

The authors close part 1 with a discussion of the possible effects of proportional representation on delegate selection and convention decision making. They argue that while proportionality fragments support and therefore increases the likelihood of brokered conventions, the recent rules changes have also generated a countervailing tendency that serves to promote first-ballot nominations. The new rules have tended simultaneously to weaken parties and hence to decrease the number of uncommitteds, and to increase the focus on candidates and hence increase the number of available candidate delegates. As a result, candidates can now win delegates in states where previously they could not do so, and this, the authors contend, reduces the chance of multi-balloted conventions.

In addition to promoting a candidate focus, the authors also argue that proportional representation could also produce a constituency focus, and that, as a result, candidate blocs and constituency blocs could form the new bargaining units at national conventions.

Part 2 consists of case studies which examine in detail the 1976 Democratic and Republican delegate selection process in three primary states (Massachusetts, Texas, and Illinois), two caucus states (Oklahoma and Virginia), and one primary-caucus state (Connecticut). Since they were written by six different authors, they vary somewhat in quality, but each in its own way provides useful insights into different aspects of the delegate selection process or into the problems state parties face in trying to comply with national party guidelines that are sometimes inconsistent and that always conflict with state party goals.

JAMES I. LENGLE

Georgetown University

"Bigotry! Ethnic, Machine, and Sexual Politics in a Senatorial Election. By Maria J. Falco. (Westport, Conn.: Greenwood Press, 1980. Pp. xvi + 200. $18.95.)

This book is the descriptive analysis of a senatorial primary campaign in Pennsylvania and its aftermath. Three disparate phenomena or variables are analyzed: (1) the awakening of ethnic political consciousness of Italian-Americans for greater recognition in the political arena; (2) the allegedly latent fact of political bias against women; and (3) the continuing decline of city-based and state-organized political machines.

In 1964 Republican Hugh Scott ran for reelection to the Senate. The Democrats had endorsed Michael J. Musmanno, a state judge, as their senatorial nominee. Having failed to obtain their preprimary endorsement, Genevieve Blatt, Pennsylvania Secretary of Internal Affairs, contested the seat in the closed Democratic primary. There is no doubt that all three of the factors presented previously were present in varying degrees, although Maria Falco fails to support her assumptions with empirical evidence. Therefore, the question remains as to whether Blatt's defeat was a result of her sex or whether she was the target of revenge by disgruntled Italian-Americans who appear to have resented being underrepresented in statewide political offices. Whatever sexual or ethnic motivations were present, one could conclude that the party organization did not actively support its candidate because the primary wounds never healed. The strongest evidence for Blatt's defeat seems to lie here.

It may be difficult to argue that Blatt was discriminated against because she was a woman. After all, she did win the 1964 Democratic primary. Prior to that election she held the elected office of Pennsylvania Secretary of Internal Affairs from 1955-1967, defeating a number of men competing for the position. Blatt rose through the Pittsburgh Democratic organization into state politics, but one might readily concede that women were only acceptable for lower elected jobs in the organization. However, in 1964, another woman, Democrat Grace Sloan, ran for Auditor General. In the two areas where discrimination is charged, Philadelphia and Allegheny County (Pittsburgh), Sloan had a plurality of 317,059 and 158,537, respectively, while Blatt's plurality was 142,324 and 17,115, respectively. Nor does the fact that Blatt lost by 70,000 votes while President Johnson won by 1.4 million mean much in Pennsylvania. In the five presidential elections since 1952 in which a Senate seat was simultaneously selected, Pennsylvania voters split their tickets four times; and in the four gubernatorial elections since 1958 in which a Senate seat was simultaneously contested, the voters divided each time. Consequently, electing a candidate from one party to the Senate and the other to the presidency or governorship is not an unusual phenomenon in Pennsylvania.

The evidence pointing to Italian reaction to Blatt's candidacy is stronger. Senator Clark, Blatt's ally, generated the bigotry issue in his open fight with Musmanno while Hugh Scott fostered it in the general campaign. Blatt was to lose reelection to Internal Affairs in 1966 and Clark lost his Senate seat in 1968. But the available statistics in 1964 show that in every instance the black and Jewish wards repudiated Blatt to a greater degree than did the Italian ones. A number of post-election interviews revealed that the bigotry issue had an effect but did not in itself lose the election for her.

The most important contribution of the case study is the activity of the political machine in a factional primary battle. Blatt bucked the machine endorsement and Clark had a reputation of mere toleration for the organization. Blatt ran a weak campaign with no overall campaign manager. It would not be the first or even the last time that a party organization geared up for one candidate only to be disappointed with another. The lengthy process of court deliberations, the inadequacies of local election officials to deal with the problem, the design of the ballot itself which was expertly shown to be used in the past to benefit certain local candidates militated against Blatt's election. More importantly, as Falco suggests, the demise of the Philadelphia and Pittsburgh Democratic machines has resulted in growing numbers of independents, reform and anti-organization candidates. The American political party is more enfeebled today than in 1964. What this case study demonstrates is that state and local parties face a transition period as social forces intermix to form new types of political organizations.

Frank T. Colon

Lehigh University

The Irony of Vietnam: The System Worked. By Leslie H. Gelb, with Richard K. Betts. (Washington, D.C.: The Brookings Institution, 1979. Pp. viii + 387. $14.95, cloth; $5.95, paper.)

The Vietnamese War was America's longest war; it was also America's greatest foreign policy failure. For more than 15 years, political analysts have sought to explain why and how the U.S. became involved in that conflict and to draw lessons from the experience. And given the awful and awesome consequences of the war and of the communist revolutions that followed it in Indochina, they will continue to do so for years to come.

In *The Irony of Vietnam: The System Worked,* Leslie Gelb and Richard Betts have written the most comprehensive and most definitive account yet of the forces that put America in Vietnam and kept it there until 1975. Successive chapters of the book present (a) the range of existing and competing explanations for the U.S. involvement; (b) detailed accounts of U.S. decision making on Vietnam from the Roosevelt administration to the Johnson administration; (c) analyses of U.S. policy makers' goals, stakes, and calculations of the means for winning the war; (d) analyses of U.S. policy makers' perceptions, hopes, and "strategy of perseverance"; and (e) a conclusion on the lessons of Vietnam. In making their case, the authors include references to virtually every major scholarly interpretation of the U.S. involvement in Vietnam published in the last generation.

The thesis of the book is that the explanation for U.S. involvement in the war lies in the doctrine of containment and in the American political consensus which supported that doctrine. American presidents believed in the containment doctrine; the commitment to maintain a non-communist Vietnam was merely the natural and logical consequence. And in adhering to this containment logic, the presidents also adhered to the political consensus in the nation. With regard to the means by which to maintain a non-communist Vietnam, the political consensus from 1950 to 1965 was to use the "minimum necessary," i.e., growing quantities of military assistance and military advisors; after 1965 keeping a political consensus meant using the "maximum feasible," i.e., massive but still limited military operations. In brief, both the purpose of the war and the way it was waged, both the goals and the means, were the product of the political consensus on the containment doctrine.

For most political scientists, the special quality, indeed the great glory of the American political system as designed by the Founding Fathers two centuries ago is its central tendency toward consensus policies, toward bargaining and compromise in which no one wins everything and no one loses everything. For Gelb and Betts, the U.S. policy in Vietnam was from first to last a product of consensus; it is in this sense that "the system worked."

This thesis stands in opposition to several other explanations for the U.S. involvement which the authors discuss. They especially seek to refute explanations which focus on "bureaucratic politics" and "the slippery slope," which see successive presidents being led by bureaucratic misinformation into an "inadvertent descent into unforeseen quicksand" (pp. 16-25). Here, their analysis is similar to, but more complicated than one put forward by Daniel Ellsberg in an essay, "The Quagmire Myth and the Stalemate Machine" in his *Papers on the War.*

In summary, this is a substantial and serious book—subtle and perceptive in its analysis, wise and comprehensive in its understanding, and prudent and sensible in its conclusions. No political scientist interpreting the Vietnamese War can ignore it.

There are some anomalies in the book's thesis, however. There were times when the containment doctrine was applied with considerable ambiguity. For example, U.S. policy makers after 1948 saw a unified although communist Yugoslavia under Tito as a means to contain the greatest communist power, the Soviet Union, in Southeast Europe. By itself, the containment doctrine does not explain why these same policy makers did not see a similar potential for a unified although communist Vietnam under Ho Chi Minh as a means to contain the great communist powers, the Soviet Union and China, in Southeast Asia (or indeed why it took them so long to see the potential for China itself to contain the Soviet Union). Conversely, the containment doctrine by itself could have led President Eisenhower to undertake military intervention in northern Vietnam (Tonkin) in 1954 to prevent its fall to the communists.

In the chapters giving detailed accounts of U.S. decision making on Vietnam, the authors give evidence that political considerations led policy makers to interpret containment to mean different things at different times. This suggests that the central variable was really the political consensus in any particular period. But here there are further anomalies. Although the authors' basic model has the president almost totally circumscribed, indeed contained, by the political consensus, the book presents a good deal of evidence for each administration from Truman to Johnson that the president and his advisors actively shaped that same political consensus in regard to Vietnam, either by acts of commission (e.g., public addresses) or omission (e.g., not putting crucial information before the public). This was most obviously and decisively the case in 1964, when Lyndon Johnson and his advisors repeatedly deceived the Congress, the press, and the public. The authors seem to recognize the importance of these actions for their thesis, and they argue that Johnson himself had not yet fully decided upon a war. But it is clear that a different presentation of the issue at that time by the administration would have produced a different political consensus. In 1964, there was a political consensus to attempt to maintain a non-communist Vietnam with means such as military assistance and advisors; there was certainly no consensus in the public to go to war in Vietnam.

Where, then, was the consensus to go to war in Vietnam? Among the foreign policy elite of the day, sometimes known as "the Eastern Establishment," and particularly among those figures, then wonderfully and ludicrously known as "Wise Men," that David Halberstam wrote about in his book *The Best and the Brightest*. It was *their* system that really worked.

In their conclusion on the lessons of Vietnam, the authors observe that the nature of the American political system, like that of any political system, is to make errors in foreign policy from time to time. The appropriate task is not to design a system that will never make errors, but to improve the American one so that it can quickly recognize and correct them. Here their prescriptions follow logically from their analysis. Americans in the future should avoid congealing analyses into doctrines, such as that of containment; the avoidance of doctrines would permit more intelligent and prudent examination of the realities in particular circumstances. Americans should also avoid congealing politics into consensus; there should be more opportunity for dissent in the Executive and in Congress. In particular, Congress could be more quick and more active in turning around a bad policy. These prescriptions—pragmatism instead of doctrine, dissent instead of consensus, checks and balances instead of executive centralization—amount to making the most distinctive features of the American political system even more distinctive and pronounced, or rather to restoring the system to what it was before World War II. It is an irony of Vietnam indeed that, as a result of the war, the system now works even more the way it was originally designed to do.

JAMES R. KURTH

Swarthmore College

Truman's Crises: A Political Biography of Harry S. Truman. By Harold F. Gosnell. (Westport, Conn.: Greenwood Press, 1980. Pp. xv + 656. $35.00.)

The subtitle of this long book is more descriptive of most of its content than its lead title. It is a copiously researched and largely factual account of the rise and the presidency of Harry S. Truman. It is also and perhaps necessarily a pretty thorough history of events and developments which affected or were confronted by Truman before and during his presidency. Harold Gosnell was for many years a professor of political science at the University of Chicago. He has also held a number of administrative and research positions in the federal government, and has become a prolific writer on public affairs. His primary interests have been politics, political leadership, and voting behavior. These concerns are reflected in the emphases of this book on Truman's politics and his political decisions rather than on his administrative experience and his personal life.

In his preface, Gosnell undertakes to answer his own rhetorical question: why another book on Truman? He points to two diametrically opposed views among the writers about Truman. One regards him as one of the truly great presidents—"decisive, intelligent, modest, discreet, farsighted. . ."; the other considers him "indecisive, dumb, inept, cocky, indiscreet, vulgar, second rate. . . ." Gosnell's declared aim "is to search for the truth, which, [he hopes] to show, lies somewhere between the extremes."

The book proceeds with a mainly chronological description of Truman's growth and maturation. Its first of three parts recounts his life from his birth in 1884 to his succession to the presidency in 1945. It tells of his family background, his early education, his experience in farming, his business experience and ultimate failure, his captaincy in World War I, his beginnings in politics in Missouri which started in 1922, his local appointments with the support of the Pendergast organization which culminated in his nomination and election to the U.S. Senate in 1934. This part of the book includes an account of his chairmanship of the Senate's Special Committee to Investigate the Defense Program during World War II, better known as the Truman Committee. This was the principal source of his national recognition that led to the presidency.

The book's second part, appropriately entitled the "Accidental President," deals with the almost unbelievable problems confronting a new president who had not been familiarized with the basic information and policies of the administration in the midst of a major war. Truman's growing confidence and optimism, his knowledge and appreciation of history, and his combativeness during the years of 1945 to 1948, remain among his most impressive characteristics.

The third part of this book concerns the "President in His Own Right," the term which expresses his surprising reelection in 1948. It deals with his actions with respect to NATO, China, McCarthyism, a variety of domestic social programs, his prospective successor—Eisenhowr or Stevenson—and ultimately to the rather unpleasant transition to Eisenhower.

The closing chapter (number 43) reverts to the question suggested by its title and raised in the preface with particular reference to Truman's decisions. In this chapter Gosnell also seeks, through a variety of quite inadequate measures, to evaluate one president against another. Comparisons of this kind are always speculative; few presidents can be compared with others because the circumstances and the challenges they face so differ. In his closing chapter, Gosnell describes Truman's decisions in three basic categories: pre-presidential, foreign relations, and domestic decisions. Although his assessment is not altogether clear, his account indicates that, in his opinion, Truman was decisive and wise: a "near-great president."

This book is the product of a tremendous amount of research. It is in the main accurate and complete. Those who, like this reviewer, remember the Truman years, will read it with nostalgia and will learn some things they did not know or have forgotten. It is a first-class reference work. But it is long and impersonal reading. And one concludes it without feeling that one knows much more about Harry S. Truman as a man than one knew before.

FREDERICK C. MOSHER

University of Virginia

Machine Politics in Transition: Party and Community in Chicago. By Thomas M.. Guterbock. (Chicago: University of Chicago Press, 1980. Pp. xxii + 324. $20.00.)

Sociologist Thomas Guterbock addresses some large and significant questions in his study of contemporary machine politics in Chicago. Rather than providing simple description, Guterbock attempts to determine the factors which account for effective organizational persistence. In particular, he explores the linkage between the service activities of a ward club and voting support for the party's candidates.

Squarely in the tradition of community-based research, Guterbock combines survey methods with participant-observation in an intensive case study of one ward on Chicago's North Side. The ward, not specifically identified, is characteristic of most wards in Chicago—the Democratic party is the only game in town. The ward is quite heterogeneous, possessing sizeable numbers of white upper- and middle-class residents, a diverse collection of low-income ethnic and social groups, and a stable white working-class population.

Guterbock lived in the ward from 1972 to 1975; for half of that period, he was active in the regular Democratic organization as an unpaid assistant to the Democratic alderman. As such, he had access to the alderman's files from which he systematically drew a client sample of 111 names. A systematic sample of 170 names were drawn from the ward's registered voter list. For most of the analyses these two samples were combined, producing a total of 160 completed interviews. Appendices usefully contain the full interview schedule, item marginals, plus descriptions of the sampling, interviewing, and multivariate analysis procedures employed.

Originally, Guterbock hypothesized that the organization derived continuing voter support through certain kinds of social exchange mechanisms. Material incentives would elicit party loyalty from low-income residents. Other urban immigrants, not yet completely assimilated into the community, would provide support in exchange for personal and affective ties to the party workers living in the community. However, Guterbock found that the data did not support these hypotheses. Material exchange was useful only in understanding the organization's internal workings: party work insured a public job. The ward club was seen as a complex compliance structure which used various techniques to secure its workers' compliance, many of them being "venal" and basically "undemocratic" in nature. Organizational maintenance required, in Guterbock's terms, this "system of defended venality," with the public being carefully shielded from the essential nature of the organization.

To explain the continued support of the citizenry, Guterbock develops a "community attachment" model which assumes a distinctly different basis for external support. Internally, the Chicago patronage party is seen as "a tightly organized exchange system involving politicians of diverse origins who have self-interested motivations in common" (p. 226). In terms of the external environment,

> the party uses service activities to show that it is responsive to persons who feel the responsibilities of local citizenship. By their substantive and symbolic actions in local public life, the patronage politicians persuade the voters that they are concerned about the local community and that they are acting to advance its interests (p. 288).

Unfortunately, the survey instrument was not adequate to test fully this model which emerged during the course of the study.

The study has other limitations. The sample size is quite small. Combining two discrete samples for data analytic purposes is questionable. Little effective use is made of the qualitative data derived from participant-observation. Finally, the representativeness of this particular ward and organization must be questioned, and thus the generalizability of the study's findings.

Nevertheless, as an exploratory study this is a worthy effort. Guterbock tested some widely supported notions and found them wanting. As a consequence, he developed an alternative model for which he finds some confirmation. He provides a plausible explanation for the continuing viability of a political organization in a changing environment, a model worthy of receiving systematic and more refined testing in other studies. This is Guterbock's chief contribution in *Machine Politics in Transition* along with providing other young researchers with an example of careful, systematic, and self-conscious social inquiry.

RICHARD M. JOHNSON

University of Illinois, Chicago Circle

The Rewards of Public Service: Compensating Top Federal Officials. Edited by Robert W. Hartman and Arnold R. Weber. (Washington, D.C.: The Brookings Institution, 1980. Pp. xi + 238. $11.95, cloth; $4.95, paper.)

The head of the Social Security Administration, who will oversee 77,000 employees and annual outlays of $136 billion this year, receives an annual salary of $52,750. The Aetna Life and Casualty Company has half that number of employees and assets of $30.2 billion, yet it is the largest diversified financial company in America. It paid its chief executive—in salary and bonuses alone—$541,000 last year.

For a variety of reasons, specialists in public affairs have begun to worry that the total compensation system for top federal executives is insufficient to attract and hold expert public managers in their jobs. The obstacle to reform resides in the fact that executive salaries at present are pegged to the salaries that congressmen and senators vote for themselves—in fact, for the new Senior Executive Service, they are pegged some $10,000 below the remuneration of lawmakers.

Against this background, the Brookings Institution in 1978 assembled a conference of academics and practitioners to examine one of the few issues in public administration that is automatically volatile—the pay of top federal officials. The resulting book consists of seven papers by eight of the participants.

Without much difficulty, the participants demonstrate that the pay issue is "politically awkward" (p. 87). As Louis Fisher reveals in his history of pay adjustments in Congress, "A vote on a pay raise is uncomfortable and was meant to be uncomfortable" (p. 50). This lesson is reinforced by the failure of the decade-long effort by Congress, beginning in 1967, to depoliticize the process by assigning the compensation issue to an independent nine-member quadrennial commission whose "impartial" recommendations, funneled through the president, would become law if Congress did nothing.

Informed of the past, the conferees set out in search of solutions for the future. The solutions they propose are professional and managerial—in two senses. First, the conference was dominated by college-educated professionals and managers—the only subgroup of the U.S. population, as Roger Davidson points out in a paper that begins with a summary of public attitudes on the subject, that are receptive "to more generous compensation for government managers" (p. 71). Not surprisingly, the major recommendation of the book is that executive and judicial salaries be disconnected from congressional pay—for the purpose, one assumes, of letting executive and judicial pay rise more rapidly.

Once removed from congressional hesitation, the determination of executive and judicial salaries would become, in the eyes of the conferees, a management problem. "In its pure form, the issue of the proper compensation . . . is a subset of the general problem of wage and salary determination" (p. 5), subject to the precepts of public administration and "the economic theory of occupational choice" (p. 101).

Public administrators prescribe different salary levels for different levels of responsibility. Congress, however, has acted otherwise. By being more generous toward low- and mid-level bureaucrats than toward itself and, by linkage, top executives, it has created a salary squeeze, most acute for members of the Senior Executive Service. Their total salary differentiation is being compressed into a few thousand dollars by the swelling magma of five million civilian and military personnel pushing up against the dome of congressional pay.

The attempt to think about total executive compensation (present, future, material, and psychic) in terms of the economics of supply and demand, comparability, net advantage, lifetime income and market differentiation might have some strange consequences. There is no economic rationale, for example, for paying the Secretary of Defense, who manages an institution three times the size of America's largest corporation (the telephone company) the same salary as the Secretary of Housing and Urban Development, whose organization employs the same number of persons as Anheuser Busch.

It is still possible, of course, to fill top executive posts by appealing to the candidate's sense of public service (and the promise of future financial payoffs in the private sector). Edward Lazear and Sherwin Rosen, in their sensitive paper on the economic aspects of compensation, show how this tends "to produce a government of the young and inexperienced . . . with high turnover and short tenure in office" (p. 112).

Some readers may find the book unnecessarily naive. It is hard to imagine our lawmakers embracing a proposal to depoliticize executive and judicial pay and let those salaries slip past their own. Conversely, it may be just as naive to believe that Congress can continue to present government executives with compensation packages that are perceived as substandard and contrary to the economics of the situation, and still expect high levels of performance.

HOWARD E. MCCURDY

American University

The Truman White House: The Administration of the Presidency, 1945-1953. Edited by Francis H. Heller. (Lawrence: The Regents Press of Kansas, 1980. Pp. xxiii + 247. $15.00.)

Three years ago the Harry S. Truman Library Institute for National and International Affairs assembled in Kansas City 22 persons who had been active participants in Truman's presidency. Their two days of discussions and the several short papers or statements written for the conference or contributed from earlier accounts by participants have been edited into this book by Francis H. Heller, who also had helped coordinate the conference.

Such a conference and its record are a valuable supplement to the static research materials housed at the presidential libraries. The papers of each administration in the respective collections are the basic contemporary materials, and the oral histories are excellent commentaries and explanations soon after the fact by participants and observers. This conference by administration participants with the added perspective of time and other presidencies has provided a different kind of material. The resulting book is an approximation of the "Heavenly Discourse" which has allowed the former staffers to discuss and argue about salient features of the Truman administration in the light of the succeeding Eisenhower, Kennedy, Johnson, Nixon, and Ford administrations. In fact, all of the participants had been given copies of Stephen Hess' book, *Organizing the Presidency* (Brookings, 1976) and were asked to react to 15 questions (not provided in the Heller book, unfortunately).

Editor Heller warns the readers that "Historical perspective is often dangerously clouded by hindsight; it is easy to say today that this or that should have been done differently" (p. xxiii). Nearly a quarter-century of intervening years has also clouded details in some of the participants' accounts as well. Quite naturally, all of the commentators, whether political appointees or careerists, tended to be very laudatory of the Truman presidency and President Truman as well. It is not an unbiased account.

Each of the four discussion sessions in the two-day conference was scheduled to cover a specific topic: the cabinet, the early period of the White House staff, the latter period of the White House staff, and the Executive Office Agencies.

In the papers and reported discussions, there was a broad consensus that the Truman administration was not top-heavy with White House staff, nor was the staff on top of the agencies, as appears to be the case in some succeeding administrations. There are repeated statements that White House staff did not come between cabinet officers and the president.

There was no agreement on whether Truman did have a real chief of staff, whether John Steelman, who carried the unique title, "Assistant to the President," or Clark Clifford or Charles F. Murphy were ever de facto chiefs of staff in the contemporary notion. Certainly, all agreed that even John Steelman was not a chief of staff in the Sherman Adams or Bob Haldeman sense. The morning staff sessions presided over by the president, who handed out assignments and sought staff views, gave access to the president even to the junior staffers and gave them direct knowledge of what the president wanted. Morale was high, and the president and staff had a high level of trust and confidence. The conference participants, 24 or more years after their involvement, discount Stephen Hess' assertion that the staff inter-position between cabinet and president began in the Truman era. Charles F. Murphy said about the idea of White House staff trying to control operations in the departments, "I think it is fair to say that during the Truman administration there was none of that. In the first place, the president regarded it as highly improper; and in the second place, I do not think the staff had any such ambitions or desires" (p. 64). There were more likely to be oversight duties by White House staff with regard to some of the smaller, non-cabinet department committees and agencies.

All of the participants seemed to agree that President Truman had a keen sense of history, a sharp comprehension of constitutional prerogatives of the respective branches, legislative and executive, which should not be violated, as well as a right of the departments not to be supervised heavily. Truman gave direct access to cabinet members and all key staff. Matthew Connelly, his appointments secretary, controlled the timing but not the ultimate access to the president via phone calls or personal meetings.

It is unfortunate that ill health or conflicting obligations kept away the other 18 invited participants such as Clark Clifford and James E. Webb, who would have contributed to the content and style of the discussion. Undoubtedly, the participants in the conference and the session moderators concentrated upon aspects of the Truman White House, one would not have been so likely to have paid attention to before Watergate. There is a great deal of duplication in this book of earlier oral histories and other accounts, but the advantage of a post-Watergate view outweighs the inevitable historical fallacies that occur so long after the fact. What the Truman Institute and Francis Heller have accomplished in this publication should encourage the other libraries and their supporting foundations to attempt similar ventures. Calling together veterans of past administrations to reflect systematically upon their past roles and activities can be useful. Directed discussion, interpretation, introspection, and disagreement among participants can add useful insights for scholars of the presidency. Enlightening footnotes to history have been created by the conference and have been edited into a usable format, more accessible than stray interviews by individual scholars which may never make it into print or into the Truman archives.

W. LANDIS JONES

President's Commission on
White House Fellowships

Why Policies Succeed or Fail: Sage Yearbooks in Politics and Public Policy, Vol. 8. Edited by Helen M. Ingram and Dean E. Mann. (Beverly Hills, Calif.: Sage, 1980. Pp. 312. $20.00, cloth; $9.95, paper.)

The Policy Studies Organization's 1980 Yearbook raises a crucial question—why politics succeed or fail. The question is the essence of public policy analysis as that area of teaching and research has developed in the political science discipline in the past decade.

Except for the series editor's introduction and opening and concluding chapters by the editors of the volume, the eighth yearbook in the organization's series leaves the impression that political scientists, by and large, are not yet ready to answer with systematic research the conceptual issues raised in the volume. Most of the articles seem not to have been written for the yearbook's theme and the selection of articles fails to round out the conceptual concerns of the editors.

As a result, the reader gets an unsatisfactory answer to the question of policy success and failure. In the introduction, Helen M. Ingram and Dean E. Mann speculate on some of the critical determinants in policy failure: intractability of the environment, nature and extent of available resources, and operant political constraints. There is a serious oversight, however, of two major areas that previous research has demonstrated as critical factors in successful and unsuccessful policy making. Little attention is given by the editors or the contributors to citizen participation and clientelism which emerged as a critical determinant of welfare and poverty policy failure. Nor does the yearbook address alternative policy delivery, which economists and public choice theorists raise as a potential for the design of successful solutions from previous policy failure.

In one of the more important contributions, Ingram and R. Kenneth Godwin pick up on the recent theme of single-issue politics to develop interesting insights into the role of political institutions in policy failure. The other articles are too esoteric and unfocused on the yearbook's theme to contribute much to a theory of policy success or failure or to alert the inquiring reader and aspiring researcher to critical variables which affect policy outcomes.

In addition to policies, one might wonder why yearbooks fail or succeed. The Policy Studies Organization, with the assistance of Sage Publications, has provided a useful forum for the publication of policy-oriented research in the discipline. Through the yearbook series, policy researchers have found an outlet for sharing their research interests and findings with others, with shorter than typical time lags. A lot of ground has been covered: what is policy; how policy is formulated, implemented and evaluated; urban dimensions of policy; and how policy is made in a federal system. Editors of future yearbooks might consider commissioning research which addresses the topic. Otherwise the Policy Studies Organization may need to put off asking such crucial questions as why policies are or are not successful, until the answers are known.

J. OLIVER WILLIAMS

North Carolina State University

Public Entrepreneurship: Toward a Theory of Bureaucratic Political Power: The Organizational Lives of Hyman Rickover, J. Edgar Hoover, and Robert Moses. By Eugene Lewis. (Bloomington: Indiana University Press, 1980. Pp. x + 274. $22.50.)

This is a fascinating, original, and seminal book that deals with the impact of personal character and style in public administration. Eugene Lewis reviews the careers of Hyman Rickover, J. Edgar Hoover, and Robert Moses to illustrate his thesis that public organizations may be shaped more by the personalities and orientations of individuals within them, particularly those at the top, than by formal organizational characteristics or even informal organization.

Lewis uses the examples of Rickover, Hoover, and Moses to develop a theory of public entrepreneurship. The public entrepreneur is a "person who creates or profoundly elaborates a public organization so as to alter greatly the existing pattern of allocation of scarce public resources. Such persons rise and succeed in organizational and political milieus which contain contradictory mixes of values received from the past. Public entrepreneurs characteristically exploit such contradictions" (p. 9).

Like elected politicians, public entrepreneurs seek power and status within and without government. Because of the vast resources of the bureaucracy, public entrepreneurs often have a broader influence upon public policy than their elected counterparts. Public entrepreneurs dictate their values to the other branches of the government, and are only intermittently controlled by Congress and the president. The potential power of the leaders of public organizations stems from their tenure and their ability to draw upon the resources of the organization to develop expertise that may become virtually unchallengable. Congress did not dictate, for example, to Admiral Rickover when it came to matters of nuclear technology concerning the atomic submarine. When J.. Edgar Hoover testified before congressional committees, he was treated with deference that was accorded to virtually no other government official. Rickover and Hoover had tight control over their respective organizations, and had become the most skillful manipulators of individuals and rules in the federal bureaucracy. They had become virtually a law unto themselves concerning matters under the jurisdiction of their agencies.

Lewis stresses the fact that in the final analysis public entrepreneurs are supreme politicians who do not have to stand for election. The three men who are the focus of this book "had the vision and the opportunities to make that leap from the confines of formal rule-bound bureaucracy into the political arena, without ever having to divest themselves of their grasp on organizational mandates and other resources. They were, in effect, a new kind of politician, one successfully adapted to the pressures of 20th-century society" (p. 239).

Lewis concludes that the power of public entrepreneurs presents a gloomy picture to those who believe in the ideals of representative democracy. These top level bureaucratic-politicians "reduce the significance of the traditional political system and thereby alter the face of democratic government and politics" (p. 241).

This book lays to rest once and for all the common notion that the administrative branch of the government is stagnant, and composed of faceless bureaucrats who are generally without ambition and incapable of innovation. The book is must reading for all scholars and students of political science and public administration.

PETER WOLL

Brandeis University

Mr. Justice Black: Absolutist on the Court. By James J. Magee. (Charlottesville: University of Virginia Press, 1980. Pp. xv + 214. $22.50.)

Shortly after he announced for the first time that freedom of speech is absolute, Hugo Black received a letter from a critic who said, in effect, that he had been wanting to express his opinion of him for a long time but had been afraid of the libel laws. "Now, I am at last free under your interpretation of the First Amendment to express my precise opinion of you. Mr. Justice, you are a sonofabitch" (Hugo Black, Jr., *My Father*, Random House, 1975, p. 184). The source of this anecdote is Black himself and he nowhere suggests that his critic incorrectly interpreted his rights under Black's absolutist thesis.

James Magee's book on Black is a detailed analysis of this thesis—how it developed, the reasons for its development, the criticisms directed against it, and its practical and theoretical ramifications. The author treats Black's constitutional jurisprudence from 1937 to 1971 with particular reference to the First Amendment. That jurisprudence is said to be based on three factors: absolutism, literalism, and Black's reading of history. Literalism refers to Black's belief that the words of the Bill of Rights are to be taken literally—that they mean what they say. Absolutism describes the thesis that all constitutional limitations on governmental power are to be strictly enforced. History is used by Black to reinforce absolutism/literalism and to justify Black's constitutional interpretations.

While conceding that Black's absolutist interpretations of the First Amendment are possibly justifiable, Magee makes and supports a number of claims that tend to dilute, if not destroy, such possibilities. Thus, during the period in which Black subscribed to the absolutist/literalist thesis, he (1) made exceptions to the demands of the thesis where he insisted none should be made and failed to explain why, (2) promoted the myth that the breadth of freedom of expression in the First Amendment is self evident and self defined in the words of the Amendment, and (3) ascribed an historical meaning to freedom of expression "strikingly short" of that justified by a fair reading of historical materials (p. 50).

On the subject of change—the author argues convincingly that Black was a balancer of interests in the forties and became an absolutist subsequent to 1951. He gives considerable attention to the alleged change in Black's behavior to which many observers have alluded in the period 1964-1971. Magee is decidedly unhappy with some of the explanations offered by these commentators. He is particularly disdainful of Glendon Schubert's 11-page treatment of Black in his *Constitutional Polity* (Boston, 1970). Agreeing with Jeffrey Morris that Schubert accuses Black of becoming senile (*Journal of Politics* 34: 296), Magee finds Schubert "willing to explore any hypothesis under the sun before he attaches any significance to Justice Black's determination to adhere to the strictures and doctrines that he synthesized into a judicial philosophy" (p. 149). Noting at one point that Schubert's fragmentary and "nearly non-existent evidence . . . refutes rather than supports his conclusion" (p. 149), the author suggests that "Schubert's unsupported conjectures flow from an undisguised dislike of the results that Justice Black derived in the late 1960s, from his literalist construction of the Constitution" (p. 150). The alternative explanation clearly preferred by Magee is that Black's libertarian attitudes waned in the period 1964-71 and that this, coupled with his allegiance to the demands of his judicial philosophy, accounted for any behavioral change that might have occurred.

While I am inclined to agree with the author's conclusions regarding the 1964-71 period (my own research is supportive here) and most of his other general lines of argument, Magee's treatment of Black's critics struck me as a little incautious. I suspect that some of these critics would more than quibble with the characterizations of their criticisms as well as with the context in which their comments were presented for analysis. And the author's comments on other observers of Black are not always supported by probative evidence. For exampled, while Schubert may or may not have "conjectured" that Black became senile in the period 1964-71, the author's evidence for the proposition that such conjecture was a product of Schubert's dislike for the substance of Black's decisions is unconvincing. In any event, the hard treatment of Black's critics pales somewhat when it is noted that Magee and Black's protagonists eventually reach the same general conclusion —i.e., that the absolutist thesis cannot stand close analysis.

In general, Magee has provided us with an excellent overview of Black's jurisprudence. The book is well crafted. The scholarship is solid. Most of the arguments presented are well supported and, on the whole, convincing. One can say this without assessing the importance of the absolutist thesis for American jurisprudence. In one sense, if the absolutist thesis is taken literally, it is indefensible. If it is not taken literally, the theoretical significance attributed to it by the literature is unwarranted. Though Magee clearly shows the thesis to be one which attracted the attention of large numbers of scholars, the significance of its impact on constitutional law and jurisprudence holds a potential for further development.

S. SIDNEY ULMER

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Politics As Communication. By Robert G. Meadow. (Norwood, N.J.: Ablex Publishing Corporation, 1980. Pp. xvi + 269. $24.95.)

With this book, Robert Meadow accomplishes three important tasks. First and foremost, he analyzes political processes and institutions from the perspective of communication, showing how changes in communication variables alter the shape of politics. This differs from the traditional approach of treating communication as a dependent variable. Secondly, Meadow reviews selected major works in the literature about political communication and points out their theoretical and analytical shortcomings. Finally, he maps out areas for research and provides a model to guide scholars from a variety of disciplines in their future explorations of the field of political communication.

Politics As Communication is divided into four parts. Part 1, entitled "An Overview of Politics as Communication," sets the stage with succinct definitions of basic terms and brief, critical reviews of past works in the field. Six major theories of political communication are described, along with their models they have spawned, including a model composed by the author which attempts to synthesize major features of prior models. The discussion is enriched by a series of well-executed diagrams.

Part 2, "Communication Perspectives on Political Concepts," examines how communication in its various forms affects eight major political processes. Here Meadow focuses on conflict, integration, development, participation, power, influence, authority, and political socialization. Each concept is probed from a variety of perspectives. For example, in discussing the role of communication in political conflicts, Meadow outlines how communication may avert or initiate conflict, how it attracts or repels a variety of participants to the fray, and how it is crucial in terminating conflict.

In Meadow's analysis of power, he shows how it involves not only the ability to control communication channels to send and suppress messages, but also requires that aspirants to power place themselves at the strategic hub of communication.

Meadow's chapter on political participation appropriately includes protest movements, as well as more routine forms of participation, on the grounds that protests of all types are a major form of communication for those lacking linguistic skills and financial and organizational resources. While political participation is viewed broadly, political socialization is treated from a limited perspective. Meadow focuses on childhood socialization, largely omitting the more important questions of the role of communication in adult socialization and re-socialization.

In part 3, "Communication in Political Institutions," Meadow examines the impact of internal and external communication on the functions performed by major governmental and private-sector institutions, such as legislatures, political parties, and mass media organizations. He calls attention to a variety of communication problems that plague such institutions. Examples are communication overload, which requires delegating much information screening to people at the bottom of the decision-making hierarchy, and the problem of facilitating the entry of change-oriented information into the system. Normally, organizational communication systems filter out information that endangers the status quo of the organization.

Part 4, on "Communication as a Political Issue," is a grab-bag of tenuously linked topics. The section begins with a general discussion of language and its meanings in various political contexts. The focus then turns to case studies of French-language policy in Canada and Spanish-language policy in the United States. A chapter on popular culture analyzes the pro- or anti-regime political messages that non-elitist movies, television shows, comics, artistic performances, and the like, convey to the general public.

The final two chapters of part 4 deal with constraints and regulations of the mass media information flow and with the uses of communication by government for public information, public relations, and propaganda. Besides formal government regulation of the mass media, Meadow discusses informal constraints springing from the need to make money from mass communication, to comply with demands for privacy and to restrain news reports threatening national security. The book ends with a comprehensive bibliography that should be very helpful to readers who wish to deepen their understanding of the many topics that have been sketched out for them.

As this summary of *Politics As Communication* indicates, the book covers a vast stretch of intellectual territory. It pays the price for such breadth in lack of depth, including a failure to convincingly establish the superiority of the communication approach over other approaches to political analysis. Nearly every one of the chapters could—and eventually should—be expanded into a book-length study because the communications perspective on politics has become increasingly useful and important.

The second half of the twentieth century, more than any previous period, is an era of communications dominance. It has become easier than ever before to gather politically relevant information, store it, retrieve it, and disseminate it to large numbers of people in written, oral, and visual form. More and more people are participating in the communication process, both as senders and receivers of messages. Communication therefore has become an increasingly pervasive and powerful political force. It lends itself more readily to human control than most other variables in the game of politics. The world's political leaders and major publics are fully aware of the power of communication. Understanding how it works and how it can be used to advantage has become paramount. By mapping out the field in original and stimulating ways, *Politics As Communication* makes a major contribution to filling this need.

DORIS A. GRABER

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National Interests and Presidential Leadership: The Setting of Priorities. By Donald E. Nuechterlein. (Boulder, Colo.: Westview Press, 1978. Pp. xvi + 246. $18.00, cloth; $8.00, paper.)

Although published in 1978, Donald Nuechterlein's book is remarkably pertinent in this election year as it considers the roles of the president and the Congress in defining the national interest of the United States. In reviewing a number of foreign policy problems which faced President Jimmy Carter at the end of 1977, it used a method of analyzing the national interest by which it predicted many of the difficulties facing the President now.

This work would be useful if only for such contributions. But its greater use results from Nuechterlein's effort to explore the concept of national interest more systematically and to promote consideration of national interest in terms of value/cast analysis. Although the idea of the national interest has been explored extensively before, this book provides an orderly process by which the policy maker and scholar may define and evaluate the national interest in specific cases.

Nuechterlein defines the national interest as "the perceived needs and desires of one sovereign state in relation to the sovereign states comprising its external environment" (p. 3). Although this definition excludes elements of the external environment other than sovereign states, seems to reflect the organic theory of the state, and seems to minimize the impact of domestic politics on the national interest, Nuechterlein indicates in the analysis which follows that he has not completely overlooked such considerations.

He, first, categorizes four basic interests: *defense interests, economic interests, world order interests* (distribution of power), and *ideological interests* (protection and promotion of the values of the nation). No priority is established among these types of interests, since each case will require its own order. Such interests are not mutually exclusive, and any case will involve an interrelated mix of the four. Second, he classifies the level of involvement or *intensity* of interest. Some interests involve *survival issues* which carry a threat to the state so serious as to warrant, possibly, an unlimited response. Some are *vital issues* which might call for a conventional force response. Some are *major issues* which are usually settled by diplomacy, and others are *peripheral issues* which are those involving private citizens or economic enterprises but which do not threaten the state as a whole. From these eight considerations, Nuechterlein forms a matrix to analyze a state's national interest in a specific case (p. 11) and then applies the matrix to different states involved in the case (p. 13). He then refines the meaning of *interests* and *intensity* and provides a table (p. 20) which summarizes the various values and costs which must be considered in any determination of policy to satisfy a perceived interest.

Nuechterlein applies his method of analysis to four cases in which presidents of the United States opted for military action abroad. He considers the perceptions of national interest of Wilson in regard to World War I, of Roosevelt in World War II, of Truman at the time of the Korean police action, and of Johnson in regard to Vietnam. He applies it to Nixon's decision to attack Cambodia. And, he considers the then unresolved cases of the national interest in regard to the Panama Canal Treaties, to the effort to separate Quebec from Canada, and to the question of racial policy in the Republic of South Africa. In each case he uses his method of analysis to evaluate the interests of all of the parties involved. Almost incidentally, he has included a chapter on the process of policy making in which he provides a good review of the ways in which presidents have used the National Security Council since its inception and concludes that the National Security Council system had developed under Nixon, Ford, and Carter. But this is hardly a book on the process of policy making. Nuechterlein's analysis concludes that, although presidents exercise the ultimate operational decision of what national interest is in any specific situation, they accept grave political risks if they fail to consult Congress in any matter involving *survival* or *vital* interests.

Refining the traditional concept of the national interest, Nuechterlein applies both historiography and systematic analysis to define the concept more precisely as an operational analytical guide to policy. The work will be useful to anyone wishing a more complete knowledge of the complexities included in the idea of national interest.

WALTER S. SCHOENBERGER

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Privacy, Law, and Public Policy. By David M. O'Brien. (New York: Praeger Publishers, 1979.. Pp. xiv + 262. $23.95.)

Sir Henry Maine, the British historian, once wrote that "the movement of progressive societies has hitherto been a movement from [law based on] Status to [law based on] Contract." In feudal times individual rights were grounded in one's status in an elaborate legal and social hierarchy. With the advent of capitalism a new theoretical basis for liberty had to be found in concepts of property and contract. In the late twentieth century social changes as profound as those which destroyed feudalism have begun to separate power from property, compelling philosophers and lawyers to seek a new basis for liberty in concepts of personhood, dignity, and autonomy. Nothing better illustrates this incipient revolution in the jurisprudence of liberty than the Warren Court's treatment of the burgeoning rights of privacy which are the subject of this book.

Unfortunately, the Burger Court is resisting this trend. The evidence is unmistakable, and in drawing it together David O'Brien has made a valuable contribution to our understanding of what Justice Brandeis once called "the most comprehensive of rights and the right most valued by civilized men."

The book begins with a useful distinction between privacy as a "*condition* of limited access" to the "life experiences and engagements" of indivuals (pp. 16, 20), the *law* of privacy (and liberty) which imposes duties of non-interference on potential privacy invaders, and the *rights* of privacy which enable the victims of some of the more serious privacy invasions to obtain compensation and, perhaps, to deter future violations. O'Brien rightly criticizes the tendency of some theorists to justify the condition of privacy and laws contributing to that condition in terms of a single value, such as information control. He also rejects the idea that privacy simply denotes seclusion or withdrawal, that rights to privacy are necessarily chosen or voluntary, or that acknowledgment of privacy interests necessarily requires the recognition of legal rights.

In his second chapter, O'Brien demonstrates that expectations of a Fourth Amendment more firmly grounded in concepts of personhood have not been fulfilled. Without abandoning the Warren Court's rhetoric about protecting persons, the current majority has converted " 'expectations of privacy into expectations of ownership, possession, or occupancy' " (p. 71, quoting 83 Yale L.J. at 1461).

The Burger Court's exposition of the Fifth Amendment right not to be a witness against oneself also remains mired in the utilitarian reasoning and proprietary concepts of the Property Age. In chapter 3 the author expertly dissects the three most common justifications for that right and shows how the Burger Court has tended to ground the right mainly on the narrow utilitarian need to maintain a responsible accusatorial system. Unfortunately, by separating privacy from respect for dignity and conscience, O'Brien ends up conceding that privacy can provide no more than an "ancillary justification" for the Fifth Amendment privilege. Mind-freedom, the right to be left alone in one's religious life, and the right to limit access to one's political and religious beliefs certainly were part of the common law right to be free from all forms of compulsory self-accusation, and cannot be read out of the privacy debate by a few court decisions interpreting the privilege in the context of ordinary criminal cases.

Limited conceptions of what constitute threats to privacy and principles relevant to the law of privacy also mar the remaining chapters of the book. Chapter 4, entitled "Preferred Freedoms and the Rights of Conscience," is not so much about rights of conscience as it is about the tort law of privacy as a shield against persons claiming First Amendment rights and associational privacy in the face of government investigations. Surprisingly, little is said about political surveillance, perhaps the greatest threat to the privacy of beliefs and associations in our time.

Chapter 5 advances the ungenerous theory that the contraception and abortion cases secure liberty, not privacy, as if there did not exist a substantial body of judicial opinions and scholarly commentary asserting a right of personal autonomy over certain fundamental life choices, including decisions to beget children, engage in homosexual activity, refuse medical treatment, or even choose the conditions and time of death. Chapter 15 of Laurence Tribe's *American Constitutional Law* is more helpful precisely because it does not treat this debate as foreclosed by a few Supreme Court decisions.

O'Brien's analysis of the new statutory rights of informational privacy (or fair information practices) is limited in scope to a dissection of the Freedom of Information and Privacy Acts. Unsuccessful attempts to constitutionalize a right of informational control are discussed at scattered points elsewhere in the book and no attempt is made to make conceptual sense out of the principles which underlie a variety of bills and regulations to govern personal data in bureaucratic hands—principles which, like the common law of privacy, may someday receive constitutional recognition.

Despite these limitations, *Privacy, Law, and Public Policy* is a good book—useful, provocative, well documented, and well written. It is worth the outrageous price Praeger has charged to print it.

CHRISTOPHER H. PYLE

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Women's Organizations' Use of the Courts. By Karen O'Connor. (Lexington, Mass.: Lexington Books, 1980. Pp. xi + 157. $18.95.)

This is a timely work. The importance of women's rights, changing patterns of litigation, and concern with single-issue interest group behavior combine to make Karen O'Connor's analysis of court lobbying both fascinating and pertinent. Focusing on "associations that have engaged in women's rights adjudication before the United States Supreme Court" (since 1869), O'Connor traces the development and treatment of women's rights before the nation's highest tribunal and analyzes the factors that shape the course of litigation (p. 9).

Arguing that interest groups use a variety of litigation-related strategies to suit myriad purposes, O'Connor initially considers litigation as a type of political action and then goes on to review the development of women's rights organizations before finally analyzing the use of the courts by these particular groups. The research summarized builds on history and traditional case analysis, though the author does report assessments from interviews conducted with attorneys involved in major court cases. Of particular interest in *Women's Organizations' Use of the Courts* is O'Connor's analysis of alternative litigation strategies, her comparison of women's rights litigation with that of the civil rights movement, and her scrutiny of factors related to adjudication success.

Taking decided aim with Nathan Hakman's argument "that the available evidence does not support a theory of judicial lobbying or planned litigation" (pp. 15-16), O'Connor identified three distinct types of strategy in women's rights cases: "litigation for publicity, direct sponsorship of cases for desired policy outcomes, and participation as friend of the court" (p. 141). Though this variety of lobbying tactics has long been recognized in judicial process, O'Connor's typological outline, her insightful description of sponsoring organizations, and her political critique of their court agendas constitute a significant contribution.

Another important feature of *Women's Organizations' Use of the Courts* is the consistent and appropriate reference to Vose's study of the NAACP. O'Connor draws the reader's attention to the litigation strategy made famous by the NAACP and analyzed by Clement Vose. O'Connor, though, takes pains to emphasize that women's rights litigation is quite different from the NAACP effort. The variety of organizations involved in women's rights, the sometimes contrasting political agendas (a good illustration being the comparative assessment of the National Woman Suffrage Association and the National Consumers' League), and the presence or absence of any of the factors associated with litigation success (e.g., financial resources) make the difference predictable if not inevitable.

O'Connor's analysis of factors ranging from status of attorney to issue focus critically related to court success reveals no surprises. The author's attention to the idea of litigation control in general and to the lobbying of the National Consumer's League in particular, however, illustrate the point effectively. The National Consumers' League focused on the improvement of working conditions for women. NCL members were enthusiastic in their support of protective legislation though this issue focus left the organization in a defensive litigation position. O'Connor outlines the ensuing strategy that NCL adopted and emphasizes the critical role that Brandeis played. Positing litigation control as the only condition of his involvement in NCL litigation, Brandeis worked successfully as NCL counsel. The limited success of Frankfurter and Acheson after him (neither of whom stressed litigation control as forcefully as Brandeis) sharply illustrates O'Connor's analysis of factors related to litigation success.

The lasting merit of O'Connor's book may lie in the questions that stem from the analysis. How have the changing patterns of litigation and recent alterations in judicial response to demands on courts (e.g., restrictions on class action suits) interacted with interest-group strategy in adjudication? Does the rise of issue-focused interest groups combine with the correspondingly exponential increase in issue complexity to make limited and rather dispersed success the only possible goal in lobbying? How do judges and courts respond to "controlled" litigation and how do these factors interact with judicial attitudes and perspectives in decision making?

These questions illustrate the heuristic contributions of O'Connor's work. Combined with the detailed case study of women's organizations and the careful analysis of women's rights cases before the U.S. Supreme Court, this makes *Women's Organizations' Use of the Courts* an important contribution to the literatures of judicial process, interest groups, and women's rights.

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The Impact of Reapportionment. By Timothy G. O'Rourke. (New Brunswick, N.J.: Transaction Books, 1980. Pp. xiv + 213. $14.95.)

The ink had scarcely dried on the Supreme Court's 1964 reapportionment decisions when several political scientists attempted to measure the impact of malapportionment prior to judicial entry into the political thicket. Employing a variety of statistical methods comparing state districting patterns with policy outcomes, these studies found little correlation between malapportionment and public policies unfavorable to urban areas or "liberal" causes. The implication, seized on by critics of the Warren Court's one-person-one-vote decisions, was that the reapportionment revolution had been waged to no avail because it would not produce the results widely anticipated. While these "skeptics" generally stated their conclusions with a high degree of certitude, one of them, Thomas Dye, had the prudence in 1965 to attach this caveat: "Quantification necessitates a simplification of what may be a very complex question. The consequences of reapportionment may be so subtle and diverse that they defy quantitative measurement. Perhaps the consequences in each state will vary so much that interstate comparisons are inappropriate" (*Journal of Politics* 27: 600).

In 1971, William Bicker (in Nelson Polsby, ed., *Reapportionment in the 1970s,* California) subjected these impact studies to a searching and penetrating analysis, concluding that they were flawed by a variety of methodological problems, and did not constitute tests of either the arguments by "reformers" or of the potential impact of the Court's reapportionment decisions. Bicker declared a mistrial. Moreover, subsequent studies began to find some effects of changing representative patterns, suggesting that institutional transformations may require time to trigger results.

Judiciously taking account of this extensive literature, particularly Bicker's insightful critique, Timothy G. O'Rourke has designed a sophisticated conceptual framework in *The Impact of Reapportionment.* This timely volume classifies potential effects into four categories. The first level embraces representative changes that result from any redistricting and that affect legislator-constituency relations, potential legislative turnover, etc. Second-level effects comprise the impact of reapportionment on group and partisan balance in the legislature, including urban/rural representation. Third-level effects include the impact of redistricting on legislative procedure, committee organization, and patterns of legislative conflict. Finally, fourth-level effects refer to the potential connection between reapportionment and policy outcomes.

O'Rourke then convincingly argues:

> In order for reapportionment to bring about measurable policy change, a series of developments rather than the single event of redistricting must transpire. First, reapportionment must lead to abnormally high turnover or, at the very least, enough district modification to alter legislative representation in a detectable fashion. To the degree that policy reflects group demands, redistricting must produce sufficient modification of group representation in the legislature (for example, the distribution of urban-rural or partisan interests) to make policy change likely or possible. If these first two conditions are met, measurable policy change would still require surmounting a variety of institutional barriers. Within the legislature, for example, stability in committee chairmanships and party leadership positions might mitigate major policy innovation despite substantially altered membership. In addition, legislative decision making must function within the context of previous decisions or laws and budgetary restraints which can limit policy innovation. It should be readily apparent that fourth-level effects, being several steps removed from the immediate impact of reapportionment, are likely to result only if reapportionment has produced sweeping changes in patterns of representation (p. 5).

O'Rourke then devotes a separate chapter to each of the four levels by analyzing patterns before and after reapportionment in the period 1962-1974 in six "sample" states: Oregon, South Dakota, Tennessee, Kansas, New Jersey, and Delaware. Why these particular states? The author does not really address this question squarely, except for a brief reference to differing or comparable patterns of urban/rural representation or party competition. Oregon, usually considered the best-apportioned state in 1962, appears to serve as a "control" case—i.e., since there was little actual malapportionment, no impact of consequence should follow reapportionment. Since O'Rourke had postulated (with supporting evidence from some studies) that the impact of reapportionment would be most severe in one-party states, it would have seemed appropriate for him to include in his sample a state such as Georgia, or Florida, or Alabama, where before 1962 urban areas were grossly under-represented in both legislative houses. The author does cite case studies in all three states revealing substantial impact of reapportionment. Still, O'Rourke's analysis, combining the depth of the case-study approach with the comparative elements of quantitative analysis, could have benefited from the inclusion of such a traditional one-party state. Augmenting the book's statistical data is a well-designed questionnaire sent to the 674 legislators of the six sample states in 1973, with a 43 percent response rate.

His investigations confirmed O'Rourke's initial expectations. First- and second-level effects were most immediate and discernible, while institutional patterns, established rules and customs, revenue limitations and past policy commitments tended to render third- and fourth-level effects more diffuse. Changes were least noticeable in Oregon and South Dakota, which had been well or fairly well apportioned prior to 1962. Impact appeared greater in states where changes in district patterns were most sweeping, replacing artificial non-metropolitan legislative majorities. But effects were also mitigated in states with strong two-party competition. Reapportionment helped increase the number of black legislators in Tennessee, Kansas, and New Jersey, and stimulated two-party competition for legislative control in Tennessee and Kansas. As for fourth-level (policy) effects, O'Rourke concludes: "The evidence presented suggests that reapportionment has affected selected policies in all sample states, but that the overall influence of reapportionment on policy has been rather limited and immeasureable" (p. 151). Among fourth-level effects were: more favorable distribution of state expenditures to central cities in most sample states; generally more "liberal" policies and possibly a more favorable climate for labor interests in all six states.

Prior to the 1960s a key indictment against malapportionment by "reformers" was that it enhanced divided party control in many states by virtually guaranteeing one party dominance of either or both legislative houses through the very pattern of districting plus partisan geography (perhaps most aptly captured by Governor Alfred E. Smith's lament in the 1920s that the New York legislature was "constitutionally Republican"). While there seems to be strong evidence that judicially prompted reapportionments have changed this condition in several states, O'Rourke does not develop this aspect well and even blurs it (see pp. 68, 157). In the case of New Jersey, he concedes that reapportionment "opened the possibility for Democratic control of the Senate" (p. 52), but then considers this to be offset by suburban Republican gains in the Assembly. But this approach tends to confuse the issue, which was not meaningful party competition but rather what the late V. O. Key, Jr. termed "the rigged rules of the game" (*American State Politics*, Knopf, 1956, p. 70).

On the whole, however, Timothy O'Rourke has made a most valuable contribution to our understanding of a complex issue. *The Impact of Reapportionment* reminds us that government decision making is intricate and full of nuances, not reducible to a simple stimulus/response mechanism.

GORDON E. BAKER

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Politics in Public Service Advertising on Television. By David L. Paletz, Roberta E. Pearson, and Donald L. Willis. (New York: Praeger, 1977. Pp. xi + 123. $14.95.)

Last spring quarter one of my students, an advertising major of reformist bent, agreed to do a term paper on the Advertising Council. Apparently not a diligent researcher, she found a number of references, and some of the self-promoting publications of the council. Under pressure to produce more, she interviewed some principals of major San Francisco advertising firms, and in the end produced a B-minus sort of thing.

What she didn't know, but should have known, was that David Paletz et al. had done the job three years ago. I didn't know it either, of course, and would have been still ignorant of this monograph had not the *APSR* book editor two months later asked for a rapid review of "a little volume that deserves professional notice."

Indeed it does deserve professional notice. Not that there's anything startling, especially insightful, or even surprising about *Politics in Public Service Advertising*. It isn't a book you can't put down—it has, at time, even a pedantic quality inducing yawns.

Its strength is that it describes clearly and in just the right amount of detail the fig-leaf of the advertising profession, the Advertising Council. Founded in 1942 jointly by the national government and business interests, named then the War Advertising Council, its mission was home-front propaganda. Since dropping "War" from its title in 1946 it has been promoting non-controversial public virtue for governmental agencies and for the more antiseptic private tax-exempt groups.

Among the associations for which the Advertising Council provides free or at-cost TV spots is Keep America Beautiful. Their ads are foursquare against individual litterers, but omit all reference to industrial pollution of waterways and to petroleum-origin air pollution. Major supporters of Keep America Beautiful are Pepsi, American Can, and United States Brewers Association; KAB "has actively opposed legislation to require . . . returnable beer and soft drink containers" (p. 17).

On the other side, council screening rules—calling exclusively for national-scope, non-commercial-sales, nonpartisan, non-controversial and non-sectarian messages that "do not attack others"—keep out groups like Freedom From Hunger and Zero Population Growth. Especially illustrative is the effort of the Council on Children, Media and Merchandising to get aired their public service announcements on snack foods and the relationship between sugar consumption and dental problems. CCMM had the impertinence to ask the three TV networks to show their spots "twice each Saturday morning and once each weekday between 4:30 and 6:30 p.m." The networks declined the offer in favor of AMA and American Dairy Association spots which did not "take a negative approach."

The evidence of this research demonstrates that television's public virtue is bland, non-partisan, non-political, and that it appeals for "solutions" to problems by individual altruism within a consensual society. The fact that controversy is inevitable in public life, that governmental action is often necessary, and that private enterprise may be inimical to the public good—such blasphemies are never admitted by the Advertising Council. The Participation urged by the council is of three kinds: (1) solicitation ("Give to the college of your choice"), (2) recruitment ("Join the Marines"), and (3) personal admonitions ("Fasten your seat belt").

Palentz and his collaborators seem to think that public service announcements should be controversial and truly politicized, and that messages should be aired from groups other than those promoting patriotism and apple pie. They also assert that such announcements are effective—that though they appear not to change people's attitudes or behavior, they do "canalize predispositions and reinforce existing behavior." A late chapter in the book, "Including the Excluded," reports some free-lance and left-liberal successes in penetrating the public service announcement world without the Advertising Council's help—to the benefit of organized gays in California, post-Vietnam amnesty for war resisters, the United Farm Workers, and some anti-growth and small-is-beautiful groups. The authors' implications are clouded: if public service announcements don't change attitudes, why bother promoting gays, draft-dodgers, stoop-laborers, and progress-doubters?

Despite its stereotypical left-lib cliches, this is a useful piece of documentation on some hitherto unexplored features of television as business.

BERNARD HENNESSY

California State University, Hayward

Community Power and Political Theory: A Further Look at Problems of Evidence and Inference, Rev. ed. By Nelson W. Polsby. (New Haven: Yale University Press, 1980. Pp. xxvi + 245. $22.50, cloth; $5.95, paper.)

If you liked the first edition, you'll love the second. Readers expecting a conciliatory, centrist, mellowed synthesis of a major intellectual controversy will be disappointed. Those anticipating a challenging, argumentative, feisty rendition will be more than satisfied. For now Nelson Polsby, having dissected the stratificationists in his first outing, strikes back at the critics of the pluralist approach in general and the New Haven study in particular in this much-expanded new edition.

Although the new book has thematic integrity throughout, the old and new parts do constitute different segments in the history of the study of community power. To help set the additional installment in place, I will quote at some length from a review I wrote of the *first* edition in 1964 (*Journal of Politics* 26: 939-40).

"Polsby's basic objective is to explore (and explode) what he calls the 'stratification theory' of community power. Five key propositions of this theory are posited. . . . The author questions the validity of these propositions by examining in detail the major studies of community power which seem to have been guided by stratification theory. He goes on to 'test' the theory by laying it against the New Haven data. . . . Essays on why the theory is so prevalent, the pluralist alternative to studying power, and what is required for a theory of community power round out the book.

"The principal merit of this *tour de force* is that it juxtaposes the theoretical underpinning of the two research approaches (stratification and pluralist) and their attendant empirical results. It is difficult, in view of his thorough documentation, to reject Polsby's contention that the stratification writers have failed to match theory and results. His argument that the pluralists have been more successful in this regard is less well documented, though still persuasive."

I went on to hazard that "the book will occasion some controversy"(!) and concluded that "this is a work of extreme relevance to those interested in the study of community power, the relationship of theory to research, and the sociology of knowledge." I am reasonably content to stand by this appraisal, though I was not sufficiently prescient to anticipate the staying power of the book.

Why did the initial work succeed so well? The answers are important because they bear on an evaluation of this lengthy addendum. First, Polsby is a master at distilling and clarifying. He has the touch, the instinct for divining and setting in place contrsting points of view. Second, he is thorough in his scholarship; he cannot be accused of being selective in his source materials (though some will quarrel with him on his *interpretation* of these materials). Third, he marshals an argument exceedingly well. This is the reason the book has so often been the focal point of the anti-pluralists. Fourth, Polsby writes in a clear, often pungent fashion. His prose is laced with asides and irreverencies that can only delight the reader.

The virtues of the first effort are present in full force in the augmentation, which consists of a reflective preface and six new chapters. Polsby first takes stock of his earlier contribution by concluding, through quoting their own work, that the stratificationists have basically accepted his critique of their failure to have demonstrated empirically the presence of cohesive ruling elites in American communities. He then goes on to address several specific charges brought against pluralists and the New Haven study. The questions and answers can be put as follows. (1) Do pluralists, as well as stratificationists, fall victim to their own theoretical and methodological orientations? No, and assertions to the contrary remain asertions until backed by evidence. (2) To what extent were the original findings and interpretations of the widely cited New Haven study in error? Specific charges, claims, and misstatements about weaknesses in the New Haven study can be refuted by textual references (shades of inerrancy here). (3) Is the second face of power, that of nondecision making and the mobilization of bias, hidden from the misdirected tools of pluralist researchers? No, whereas the first face persists largely without blemish, the second face, though occasionally glimpsed, remains largely one of imagery and conjecture. (4) Do pluralists err in not distinguishing more clearly between the manifested preferences and the real interests of political actors and do they not overlook the problems of enlightened choices and false consciousness? Well, the subject of interests and preferences is troublesome, indeed, but pluralist researchers have not ignored actors' interests to the extent of painting a false picture of community power.

Of these new chapters the one dealing with the mobilization of bias strikes me as the most powerful and generally most useful one to a variety of readers. This theme is perhaps the most tantalizing and intuitively appealing critique made against the pluralists. Yet point by point Polsby neutralizes the criticisms, though ultimately conceding that if non-decisions could be studied successfully this might enhance our understanding of community power. Much of this chapter is vintage Polsby, his rapier in full play.

Just as I was beginning to compose this review I received a notice that Floyd Hunter's new book on Atlanta was imminent. Having read the manuscript version, I can assure the world that community stratification theory is alive, though whether it is also well is more problematic. So the eighties should not lack for a little excitement. And the reason is not a consuming interest in local politics per se. I agree with Polsby that "as a source of cumulative knowledge about the governance of local communities, the community power literature is disappointing. As a smallish arena for the ventilation of some of the central metamethodological issues of social research, this literature has some didactic value" (p. xvii). With this fresh installment, he has done his part to keep the action alive in that arena.

M. Kent Jennings

University of Michigan

Urban Policy and the Exterior City: Federal, State and Corporate Impacts upon Major Cities. By H. V. Savitch. (New York: Pergamon Press, 1979. Pp. xv + 359. $35.00.)

Books which describe the decay of major American cities, catalog the urban programs which are intended as responses to that decay, and cite the failures of states and nations to develop an effective urban policy are not unusual. However, the approach taken in *Urban Policy and the Exterior City* is unusual. H. V. Savitch argues that urban decay is a problem of the "exterior" city and that forces external to the city, i.e., national authority, middle governments, and private power, have created the circumstances which have led to its destruction.

The key to Savitch's argument is the concept of privatism, by which he means "the larger corporate structure in American capitalism and the ability of its individual weight . . . to make a difference in the size and composition of cities" (p. 3). National authority, e.g., presidents and their administrations, and middle governments can be classified either as reinforcing this privatism or as adopting "meliorist" policies which attempt to intervene on behalf of the cities. Savitch discusses recent national administrations and the states in these terms and concludes that, to varying degrees, Kennedy, Johnson, California, Maryland, Massachusetts, and New York are "meliorist" while Nixon, Ford, Louisiana, Missouri, and Ohio are "reinforcing."

There is considerable virtue in challenging the conventional wisdom which focuses on the political and administrative failures of city governments as contributors to urban decay. However, Savitch's attempts to create clear, dichotomous classifications are not fully successful. At the outset, sunbelt cities are excluded from the exterior city category because they have not yet experienced urban decay (p. 1). There are serious logical problems with such an exclusion. Privatism and intervening national and middle government policies clearly have an impact on cities which are classified as "most advantaged" as well as on those that are "disadvantaged" (p. 37). The impact is simply different. The exterior city concept is a useful one, but it is a serious mistake to apply it only to the decaying, disadvantaged city. One can no more understand Houston by examining only "interior" factors than one can understand Cleveland in this way. Privatism, often reinforced by national, state, and local policies, is tremendously advantageous to some cities (if physical and economic growth is always an advantage) and disastrous to others. The model of the exterior city and the assumptions underlying it all are ill defined.

The classification of national and state policies as "reinforcing" or "meliorist" is overly simple. Savitch finds himself uncomfortable with his own categories in that he sees Kennedy as less meliorist than Johnson (p. 111) and finds several states to be "mixed or ambiguous" as to type (p. 247), to cite but two examples. The labels probably can best be seen as two points on a continuum. By implication, there is something beyond meliorism which would deal more effectively with privatism (or even eliminate it), and it is possible to go beyond reinforcement. The criteria used by Savitch to place administrations and states on the continuum are not clear. In the case of states, they are tied to state aid to selected cities and to differences between state aid for welfare and state aid for highways (p. 246). However, because the analysis is limited to 10 states with exterior cities, our reference points are muddled. Are Arizona or Texas (not included in the analysis) more reinforcing of privatism than Ohio? Is Ohio closer to New York on the scale than it is to Arizona? Is Ohio, then, really reinforcing rather than meliorist? The data are incomplete, and we really have no answers.

Urban Policy and the Exterior City is a fundamentally discouraging book. It begins with the assertion that "what corporations and the shakers of the economy do determines what Washington and middle governments will do" (p. 12); rejects many recent national efforts as "policies [which] may meliorate the festering wounds of the city and the difficulties of chronic unemployment, but . . . do nothing basic to solve these problems" (p. 6); and rejects "the commonly held belief that Washington has already done what it could for cities and is not to blame for their failure" (p. 6). By implication, we can conclude that Washington should do more, that "more" does not mean additional meliorative policies, and that the determinate relationship between privatism and public policy should be broken. Savitch does not take the next step and offer such a prescription. If his first assertion is correct and corporate decisions do determine national, state, and local governmental policy, then the future is indeed bleak, given his analysis. If the relationship is not wholly deterministic, then Washington and the states have some hope of reversing the tide.

There is evidence that Savitch has overstated his case and that governments can and do affect private economic decisions in a significant manner. State and federal environmental protection regulations influence corporate decisions with respect to plant locations and the existence of federal minima guarantees that corporations are restrained to some degree from playing one state against another. While federal and state efforts have been modest, they do provide evidence that Savitch's "determines" is too strong a word and that there is some hope for those who view government intervention in the allocation of private resources among urban areas as a major part of the solution to problems of urban decay.

Savitch's analysis is too simplistic. It is crucial to recognize the role of private, corporate decisions in urban policy, but the web of interrelationships is considerably more complex than the one which he suggests.

JOHN H. BAKER

Southern Illinois University, Carbondale

The Future of U.S. Politics in an Age of Economic Limits. By Bruce M. Shefrin. (Boulder, Colo.: Westview Press, 1980. Pp. xii + 251. $24.50.)

This book considers future U.S. political and economic systems based upon a slow-growth or no-growth American economy. Bruce Shefrin argues that the existing U.S. political system—consensus politics—reduces political conflict mainly through the lower and middle classes' expectation of progress based on their belief in economic growth, i.e., greater opportunity and prosperity. He indicates that consensus politics has limited redistributive policies, has moderated class consciousness, and has maintained economic inequality among classes.

Shefrin asks: if consensus politics is weakened by a decline in economic growth, then what changes will occur in American political attitudes and behavior and the conflict-resolving mechanisms of the political system? He suggests that lower levels of economic growth will lead to an increased perception of economic inequality and more intensive types of political and economic conflicts than are found under consensus politics. A limitation in this book is that Shefrin does not indicate the process through which these attitudes will change, nor does he show that these types of attitudes changed during earlier periods of U.S. economic decline.

Shefrin accepts the likelihood that the U.S. economy will have limited growth in the near future. His position is consistent with the views of D. Meadows et al. *Limits to Growth* (Universe, 1972). Shefrin does not examine either the criticisms of Meadows' analysis or the revised economic model in M. Mesarovic and E. Pestel, *Mankind at the Turning Point: The Second Report to the Club of Rome* (Dutton, 1974). These omissions reflect shortcomings in Shefrin's analysis of physical limits to growth—raw materials and industrial effluents—and his assumptions about a future based upon limited growth.

Shefrin offers a speculative analysis of four scenarios in a future of limited growth. Each scenario is based upon a different ideological foundation—liberalism, neo-Marxism, corporate-conservatism, and traditional conservatism. He argues that there are different combinations of political and economic systems. These new structures depend upon a serious challenge to consensus politics leading to social awareness of slow-growth or no-growth economy. Shefrin's emphasis on the ideological polarization underlying political and economic systems adds an important element to limits-to-growth analysis. However, further conceptualization of political change is needed to explain how the new awareness will occur under the alternative political and economic scenarios.

The study of changes in a society's political behavior and attitudes during periods of economic instability or economic decline is an important question for political scientists. Shefrin raises thoughtful questions about the relationships between attitudes toward the economy and political behavior. His study leaves room for further conceptual and empirical analysis of those questions and about future U.S. politics.

STEVEN PURO

St. Louis University

The Failure of the Franklin National Bank: Challenge to the International Banking System. By Joan Edelman Spero. (New York: Columbia University Press, 1980. Pp. xi + 235. $14.95.)

Joan Spero has painstakingly documented the most dramatic episode in recent U.S. banking history: the collapse of the Franklin National Bank. Franklin held $5 billion in assets and was the twentieth largest U.S. bank before it began its slide. The failure of a bank as large as Franklin would be a serious matter any time, but was especially so in 1974, in the context of a worldwide recession, high inflation, uncertain prospects for petrodollar recycling, large fluctuations in exchange rates, and announcements of other banking problems, including closure of Bankhaus Herstatt in Germany while efforts were under way to salvage Franklin.

Spero's thesis is that: (1) banks, especially U.S. banks, rapidly expanded their international operations after 1960, inevitably attracting some weak, ill-prepared participants like Franklin; (2) international banking involves different, possibly larger risks, particularly hazardous to newcomers; (3) the international banking system is highly interdependent because it relies so heavily upon interbank transactions, including loans, deposits and foreign exchange trading; (4) the system is fragile because a deterioration at one bank can leave others vulnerable as well; and (5) the absence of an international lender of last resort, together with gaps in bank supervision, suggest that the system may be unable to prevent or manage future crises. Improvements were made after 1974, but Spero believes stronger bank supervision and clearer demarcation of responsibilities may be needed.

Spero's account is excellent history—important reading for those interested in money and banking or in international economic relations. The policy implications could be expanded, however.

The United States has developed one of the most extensive and restrictive bank regulatory and supervisory systems in the world. U.S. banks are not "national"—their operations in the U.S. are generally restricted by law to a single state, sometimes to a single location. U.S. banks are barred from many lines of business in which foreign banks engage. U.S. banks are subject to frequent audits and examinations by federal and state authorities.

One of the few growth opportunities for U.S. banks lies in international banking. Franklin took the plunge and failed, but there is little to support the view that Franklin failed because of its international activities. Spero notes that Franklin was weak on virtually every count: "weak management, a bad domestic loan portfolio, poor investments, heavy reliance on very short-term loans" (p. 11). Excessive foreign exchange speculation, illegally concealed, helped bring down the bank, but it was undertaken in a futile effort to make up losses on domestic, not international, operations.

Surprisingly, the extensive American system of bank supervision and regulation failed to prevent Franklin's collapse, and it took the three federal agencies concerned (the Federal Reserve Board, FDIC, and Comptroller of the Currency) five months to resolve the crisis. International cooperation, on the other hand, was smooth and effective in the Franklin case. Perhaps improvements in the international banking system should begin at home?

ROBERT W. RUSSELL

U.S. Senate Committee on Banking, Housing and Urban Affairs

Rolling Thunder: Understanding Policy and Program Failure. By James Clay Thompson. (Chapel Hill: University of North Carolina Press, 1980. Pp. xv + 199. $14.00, cloth; $6.50, paper.)

"Rolling Thunder" was the code word for the sustained bombing campaign carried out by the United States against North Vietnam between February 1965, and March 1968. The campaign began as a reprisal in connection with the Pleiku incident. Shortly it became a long-range program with a multiplicity of objectives, to include forcing the North Vietnamese into serious negotiations. It was ended by LBJ in his "abdication" speech of March 31, 1968, in the aftermath of the Tet Offensive.

There seems little doubt that the campaign was a strategic failure, and Thompson is quite clear on this. He uses the campaign as a case study of the workings of the American national security apparatus. In particular, he attempts to determine why it took so long to discover that Rolling Thunder was not accomplishing its objectives and why the foreign/defense bureaucracy failed to adapt to the realities of that failure.

Thompson's approach is to begin with three descriptive chapters on the origins and failure of Rolling Thunder. This is followed by a chapter giving his interpretation of the evolution of organizational theory, especially that of decision making. This chapter, by the way, would be better placed as an appendix. The final chapter focuses on decision masking in the area of foreign and defense policy with a view toward interpreting the course and outcome of the Rolling Thunder campaign previously described.

Rolling Thunder was enormously expensive in material and human resources; the subject is therefore an important one. The standard air force explanation for its failure is that air power was applied incrementally as contrasted to being applied massively on such critical targets as Hanoi and Haiphong. Thus, this explanation goes, when Nixon finally applied massive air power late in the war, i.e., the Christmas bombing of 1972, the North Vietnamese agreed to the accords of January 1973. What this explanation overlooks is that the January 1973, agreement is, with minor differences, the same one which the U.S. could have received the previous fall. Certainly the North Vietnamese were happy to get the heavy bombardment over, as was the U.S. military, for the costs in aircraft and personnel were heavy. The question is, did the Christmas bombing really have any political utility in terms of the final settlement? Many, myself included, would say no.

In this context what is the value of Thompson's work? Certainly it is an excellent and concise description and analysis of the bombing campaign and on that level a very worthwhile effort. The author goes beyond that, however, and tries to determine why the U.S. national security apparatus failed to adapt to the realities of the environment, which clearly was not being influenced by the massive tonnage being delivered on North Vietnam. His answer, though not highly original, is interesting. He concludes that Rolling Thunder was shaped more by "the internal needs of the [U.S. national security] bureaucracy, than to the problem at hand." Further, says Thompson, the program resembled more an "anarchy" than a coherent military program designed to achieve foreign policy objectives.

While this work is probably not the final word on the strategic failure of the American bombing campaign in North Vietnam, it is more insightful than the simplistic post hoc explanations of the U.S. Air Force. Overall, the case seems to be another example of foreign/defense policy decision making being influenced more by process than rational analysis.

DOUGLAS KINNARD

University of Vermont

Policy Implementation in the Federal System: National Goals and Local Implementors. By Carl E. Van Horn. (Lexington, Mass.: Lexington Books, D.C. Heath, 1979. Pp. xiv + 178. $18.95.)

The legislative successes of the Nixon administration's New Federalism proposals raised several questions regarding the potential change in intergovernmental relations. How are national goals reconciled with often conflicting local priorities? What happens when the implementing rules are changed from nationally controlled categorical programs to more loosely structured block grants? How does the political process change and who benefits from such changes? Carl Van Horn combines two areas of study for political scientists, public policy and intergovernmental relations, to address these questions. Taking the General Revenue Sharing program (GRS), the Comprehensive Employment and Training Act (CETA), and the Community Development Block Grant (CDBG), Van Horn develops and applies a model of intergovernmental policy implementation to analyze what transpires in the implementation process of these three important cornerstones of the New Federalism.

Van Horn's model identifies three variable clusters as a means to analyze the programs. The first, policy standards and resources, examines the law (its priorities, procedures and prohibitions, and its contradictions and ambiguities) and its resources (funding availability and timing, and contribution to recipient communities' budgets). The second, the national policy environment, explores the influence of national organizations, and national-level administrators and elected officials who attempt to influence policy implementation. The third, the local policy environment, investigates the attitudes and influence of various local actors, the social, economic and fiscal conditions of those areas and the characteristics of the agency responsible for carrying out the law.

Van Horn's book confirms the intergovernmental changes brought about by the implementation of the New Federalism policies. In both the CETA and CDBG programs, locally elected officials became more involved in implementation decisions than they were under the more centralized period of the categorical programs. Conversely, GRS produced no significant changes from normal (pre-GRS) decision-making practices at the local level. While the New Federalism programs initially reduced the influence of national administrative agencies in the design and implementation of the laws, these agencies have become more aggressive in their roles through more assertive enforcement of procedural requirements and providing direction in the substance of local programs. This is evidenced by a greater mandate for monitoring and evaluations in the reauthorization of the programs. In the use of funds under these programs, spending in the CETA program seemed to be the exception, in that minimal program reform was achieved with the changes from the categorical to the block grants. GRS funds were spent primarily on capital projects with a minimum amount allocated to social service programs. In contrast to patterns established with slum clearance, urban renewal, and social services offered with Model Cities programs, communities spent CDBG bunds increasingly on smaller, short-term projects directed toward public works and housing conservation and rehabilitation. The beneficiaries of the three programs were as varied as the programs themselves. As GRS was not designed to benefit low-income people, few such direct benefits were so distributed. The inquiry into benefits attributable to the CETA and CDBG programs produced mixed results. Combining the summaries of analysis from each program, Van Horn offers the generalization that some amount of flexibility or lack of legislative clarity may be necessary for effective implementation—though to insure redistributive outcomes, it appears necessary to establish clear national goals.

This book is an interesting mix of reporting the findings from several studies (particularly on GRS) and Van Horn's own work, yet it is not without certain weaknesses. At times, the reading of the book slows, perhaps because of the difficulty in integrating those studies through a new and differently focused lens. While CETA and CDBG programs provide an opportunity to compare the consolidated grants with their predecessor categorical counterparts, there is some difficulty in including General Revenue Sharing. GRS is a significant component of the New Federalism, yet it does not have a nationally based program as a predecessor for comparison. The book claims a readership of those interested in the study of public policy, intergovernmental relations, urban politics and/or implementation and certainly such a wide audience will find at least parts of the book directly related to their interests. However, as Van Horn warns at the outset, this book does not provide specific details about each program. Consultation with the well-referenced case studies is appropriate. The author also acknowledges the major weakness of the model: that the variable clusters are not as comprehensive as they might be. He also restricts his investigation by looking only at selective parts of each program (among them, enforcement of non-discrimination provisions of GRS and the distribution of public service jobs in CETA). Nonetheless, the rigor employed in the analysis helps explain the complexities of program implementation and the changes which have occurred through the New Federalism efforts of the early seventies. For political scientists interested in a systematic overview of those efforts, this book provides a very good opportunity to assess the factors influencing the implementation of different programs by employing a useful model. While that model is not as comprehensive as it could be, the effort is commendable as the book attempts to find the seldom-reached middle ground between the various frameworks for studying public policy and the plethora of case studies which abound.

DOUGLAS H. SHUMAVON

Miami University

Unloving Care: The Nursing Home Tragedy. By Bruce C. Vladeck. (New York: Basic Books, 1980. Pp. xiii + 305. $13.95.)

In the introduction to this Twentieth Century Fund Study, the author is described as a "young and untested scholar interested in public policy issues . . . who had a grasp of the complexity of his subject and cared passionately about the shortcomings he discovered" (p. ix). Bruce Vladeck is a health planner for the New Jersey Department of Health in addition to teaching public health and political science at Columbia. He is qualified to deal with this complex, dreary, and increasingly important facet of public policy. Although the text could have been better organized, Vladeck reveals a clear understanding of the goal of nursing home care: "High quality nursing home care consists of the maintenance of a clean and pleasant environment, in which the food is good, . . . there is plenty to do, assistance is readily given with dressing and bathing, people are nice to each other and respect each other's privacy and personal dignity, and good medical and nursing services are provided to those who need them" (pp. 148-49).

He also reminds us that nursing home care in the United States is a public problem: 57 percent of nursing home income comes from the public purse and almost 90 percent of that amount comes via Medicaid, the federal welfare program for the aged and medically indigent.

Vladeck provides just enough statistics to demonstrate the intractability of the nursing home problem. Although only one in ten citizens over 75 lives in a nursing home, the average age of the institutionalized elderly is over 80. More than half cannot walk unassisted; 30 percent are incontinent. White females over 80 constitute the modal group of nursing home residents.

Vladeck's description of the history of nursing homes and government policy initiatives constitute the middle and longest portion of the book. Vladeck shows a sophisticated understanding of the political process as it related to hospitals and nursing homes in the 1970s.

I found the prescriptive portion of *Unloving Care* disappointing, however, perhaps because I expected too much. Vladeck's claim that private needs can be met only by public action (p. 266) isn't persuasive, particularly with regard to problems of the aging which are treated largely outside the public domain by the American middle class and even more so within the black and brown ethnic communities. The author recommends federally mandated reductions in the number of nursing home beds with the residual to be reserved for short stays for the approximately 150,000 aged who are terminally ill and those needing interim care, plus the 600,000 who require round-the-clock custodial care (p. 223). Standards would be maintained by a complex set of oversight procedures including inspection, peer review, and public scrutiny.

Certainly the current policy schema of incentives and reimbursement by level of care hasn't produced quality nursing care, particularly in homes populated largely by Medicaid recipients. But, implementation of Vladeck's policy seems unlikely to produce more loving care than the policies currently in force; I believe that increased regulation would promote consolidation of nursing homes and would lead inexorably to the depersonalization of services to aged residents which Vladeck criticizes.

Sadly, Vladeck did not attend carefully enough to some of the newer approaches to care for the aged or to the ways government might encourage these new approaches. He discusses home care only briefly (pp. 215-16) and mentions the hospice movement not at all. He fails to examine the ways in which some local governments are combining federal programs, such as senior luncheon activities and the subsidized housing program, to provide creative and humane options for the ambulant elderly.

As for the aged and infirm, careful reading of *Unloving Care* leads me to the unappealing conclusion that there is little help for the lonely old lady trapped in a filthy and understaffed nursing home with nowhere else to go.

GAYLE AVANT

Baylor University

Legislated Learning: The Bureaucratization of the American Classroom. By Arthur E. Wise. (Berkeley: University of California Press, 1979. Pp. xvi + 219. $10.95.)

Criticism of the extraordinary growth of government regulation of education has been an insistent theme of academic professionals in recent years. *Legislated Learning* is an analysis of this problem that makes an important contribution to the literature. Part of the book's significance is Arthur Wise's particular background. He is a former officer of the National Institute for Education and is currently a senior social scientist with the Rand Corporation. More relevant, it was his legal analysis in *Rich Schools, Poor Schools* (Chicago, 1969) that intellectually launched the modern movement to equalize public school finance. Although the U.S. Supreme Court in a 5-4 decision (*San Antonio* v. *Rodriguez*) rejected Wise's constitutional argument, many state courts accepted his thesis. As these courts set about reforming their state's educational fiscal systems, they inadvertently had the effect of strengthening state dominance of local school boards and increasing educational bureaucracy in general. This occurred most dramatically in New Jersey where the state Supreme Court in six different opinions (called *Robinson* v. *Cahill* I through VI) moved from a concern with equalizing school finance to a complete revision of the goals and structure of the educational system. The court frequently cited *Rich Schools, Poor Schools* with approval and in *Legislated Learning* Wise devotes a chapter to detailing the policy evolution in these decisions.

Given his background, then, it comes as a bit of a surprise that Wise's new book is a critique of too much bureaucratic and judicial regulation. He, of course, does not reject all regulation. He tries instead to make a distinction between regulation to achieve equality which he believes is necessary, and regulation to insure efficiency and effectiveness—most of which he thinks is misguided. The distinction, however, as he concedes, is increasingly difficult to sustain.

There is a drive for accountability apart from any concern for equity. Wise identifies some 11 different management concepts or tools that have been applied to education, ranging from competency-based education to performance contracting. By 1974, there were at least 73 laws enacted by states mandating educational "accountability."

Increasingly, however, the search for accountability and the search for equity have intertwined. As the goal of equal *opportunity* in the sixties evolved into a demand for equal *results* in the seventies, legislatures and courts discovered that the new form of equality could not be achieved without regulating all of the contributors to the efficiency/effectiveness of the entire school system. Nor is Wise's distinction entirely plausible that equity regulation is political and therefore within the knowledge of courts and legislatures, while intervention for efficiency/effectiveness is technical and beyond the capacity of courts and legislatures. Equalitarian regulation in the form of affirmative action plans, challenges to academic testing and employment selection or judicial supervision of school integration often have substantial technical components. It is not apparent that courts and legislatures know how to create equality any more than they know how to create efficiency/effectiveness.

But, if Wise's criticism of the source of regulation is a bit partial (perhaps for liberal ideological reasons), his analysis of the consequences of the growing school bureaucratization is very persuasive. He describes a number of trends, increased litigation, assertive legislatures, ambitious teacher organizations and powerful single-issue groups, which contribute to forcing educational policy to higher and higher levels of the political process. Since these levels can only deal with schools, teachers and students as abstractions, the result is "hyperrationalization" (Wise's favorite term) of educational policy.

"Hyperrationalization" is the attempt to establish rules and procedures for all elements of the educational process and to specify all goals in ways that have measurable outcomes. This may make schools less arbitrary and educational results more predictable; but it reduces teacher discretion and incentive, the variety and competition among schools, and leads to a reductionist view of the purposes of education. Being the easiest data to gather, credit hours, student-teacher ratios, standardized test scores come to dominate discussions of educational policy. Students are seen as products subject to cost and quality control rather than as full human beings.

What is the likely future of this development? Are there alternatives? In his final chapter, Wise suggests four possibilities. The most likely future development is incremental bureaucratic centralization, but the other possibilities are "planned bureaucratic centralization, responsible local control, the rationalistic solution and the rational solution." Wise notes that some might conclude that it would be better to plan bureaucratic centralization than to have it occur incrementally. He suggests that centralized educational systems elsewhere in the world and large-city school systems in the United States be studied as a prerequisite for such a step, but Wise is not enthusiastic. Nor does he think local control will work, given the continuing need to assure equality. The rationalistic solution which begins with the premise that education needs improved policy tools, principally productivity-improving devices, does not have a record of success, according to Wise. That leaves "the rational solution," which Wise defines as maintaining regulation for equality but recognizing that we do not know enough to regulate for increased productivity. Wise prefers this approach and he brushes aside vouchers and school decentralization in a few paragraphs. But the rational solution concept is never really developed and it is hardly a "solution" in any event. In the meantime, regulation of education by many agencies having many motives will continue to grow. Wise has given us a powerful statement of the costs of that trend, but sees no solution.

GEORGE R. LANOUE

University of Maryland, Baltimore County

Who Votes? By Raymond E. Wolfinger and Steven J. Rosenstone. (New Haven: Yale University Press, 1980. Pp. x + 158. $15.00, cloth; $3.95, paper.)

This study examines the impact of registration procedures and 17 demographic variables on the probability of voting in presidential and congressional elections. The Census Bureau's Current Population Surveys for November, 1972 and 1974, were the main sources of data, and probit estimation was the principal mode of analysis. The authors report their findings with unusual care and clarity.

The huge census samples (of over 90,000 respondents) permit exploration of the demography of voting and nonvoting in rich detail. The authors nicely disentangle the separate effects of education, income, and occupation and, holding other factors constant, show also that:

- the probability of voting increases with age up to and through the seventies;
- housewives are as likely to vote as working women;
- marriage increases the probability of voting, with the effect weakest among the young;
- the turnout of governmental employees in "patronage" states is significantly higher than their turnout elsewhere;
- Chicanos are as likely to vote as other voters, Puerto Ricans are not.

Other findings of interest abound.

The census samples are also very helpful in isolating the effects of various registration procedures and are well exploited to that end. One has greater confidence in the study's positive findings in this regard, however, than in some negative ones. Laws authorizing neighborhood registration *may* make no difference, but their apparent insignificance could also reflect inability to identify variations in the implementation of such laws. Similarly, while *provision* for the use of deputy registrars does not increase voting, their actual use may, as the authors note.

The census surveys make no effort to determine whether respondents feel obligated to vote, prefer a candidate, think voting accomplishes something, find the campaign interesting, see the race as close, or have partisan affiliations. The absence of these and other psychological variables from the authors' data limits not only their ability to explain some of the variance in turnout but also their ability to trace the connections between demographic variables and rates of voting. For instance, while they show convincingly that education has a far more powerful and pervasive relationship to voting than income or occupation, they cannot, given their data, say why. Their speculations on the matter are sensible, but speculation is not explanation.

The study concludes with this statement: "For the present, it appears that advocates of both sides of major controversial policy issues are represented among voters in proportion to their numbers in the population" (p. 114). This finding is well supported, and the analysis that produced it is an obviously good move in assessing the consequences of differential turnout among population groups. The finding is sure to suggest to some that differential turnout is of no importance, however, though the authors do not draw that conclusion. Properly. Unless one can show that full-turnout elections would have no impact on the strategies of candidates, one cannot be sure that the issues now raised are the ones they would raise in the absence of group differences in turnout.

In the tasks it sets itself, I do not expect soon to see a book that surpasses the achievements of this one.

STANLEY KELLEY, JR.

Princeton University

Comparative Politics

Religion and Politics in Contemporary Iran: Clergy-State Relations in the Pahlavi Period. By Shahrough Akhavi. (Albany: State University of New York Press, 1980. Pp. xix + 225. $29.00, cloth; $9.95, paper.)

Shahrough Akhavi has produced a book on a subject as tricky as it is timely, a book which is a triumph of intelligence and common sense, and the best guide that now exists to the rôle of the *ulama* (clergy) in twentieth-century Iranian politics. Although the style is sometimes dense, and reading it is occasionally hard going, especially in the introductory chapter, the later chapters, appendices, and notes repay close attention, as they include a great amount of information coupled with sensible and convincing interpretations. The book covers briefly the leading role of the *ulama* in the rebellious and revolutionary movements of 1890-1911; the diminution of their power under Reza Shah (1925-41) who stripped them of many legal, educational, political, and economic prerogatives; and especially their revived political role after 1941. The religious opposition of the Mossadeq period, represented chiefly by Ayatollah Kashani and the terrorist Feda'iyan-e Islam, are well covered, and there are facts not easily obtainable about the activities and writings of recent oppositional *ulama,* including Ayatollahs Khomeini, Taleqani, and Shari'atmadari. Also covered is the lay (not theologically educated) religious opposition. Particularly interesting to me is the discussion of anticlerical implications of some of the talks of the most influential man in this opposition, Ali Shari'ati, which were echoed by the cool reception given Shari'ati before his death in 1977 by all the leading ayatollahs. It is only since his death that many of the latter have tried to appropriate him. Throughout, the most important Persian and Western writings have been consulted by Akhavi.

Akhavi's book includes discussions of the economic and political sides of growing clerical discontent; depriving the *ulama* of positions meant depriving them of wealth and power. He notes that the Pahlavi rulers never gave significant attention to even the justified grievances of the *ulama* or their allies in the bazaar economy, underestimating the importance of both in their forced march to Westernization.

Although small errors may be found, such as the author's following Algar's account of Khomeini's deportation, which has anachronisms including dating it in 1963 instead of 1964, or writing *fiq* for the standard *fiqh,* Akhavi's account is generally reliable and, because it was largely done before materials on his subject were widely available, it is a remarkable and highly useful achievement.

NIKKI R. KEDDIE

University of California, Los Angeles

The Politics of Economic Decline: Economic Management and Political Behaviour in Britain since 1964. By James E. Alt. (New York: Cambridge University Press, 1979. Pp. viii + 296. $24.95.)

This study of the intricate relationships in contemporary Britain among personal well-being, the state of the economy, and political outlook is by far the most thoughtful and exhaustive investigation on the subject. James Alt offers a useful explanation for why these troubled economic times have produced a politics characterized by cynicism and conservatism rather than a politics of activism and radicalism: that individuals have become more realistic about the condition of the economy and more pessimistic about the limits of government. Alt begins his investigation by acknowledging his debt to Butler's and Stokes' work of a decade ago. He does, however, reject Stokes' distinction between valence and positional issues, and argues that in recent years at least, every issue contains both valence and positional components. For example, everyone is opposed to unemployment, the valence component; but if full employment is linked in the individual's understanding to continued high inflation, then the positional component of the issue becomes apparent.

This interweaving of positional and valence dimensions is important to understanding Alt's central argument that Britons have grown in economic sophistication as economic troubles persist. Britons, Alt finds, clearly distinguish between their own individual economic prospects and their appraisal of the national economy. They tend accurately to assess national economic performance and increasingly to see the interconnectedness of economic issues. Their skepticism has grown over the abilities of successive British governments to improve either the voter's personal well-being or the condition of the nation's economy.

Alt's findings concerning the welfare state's future in a time of diminishing economic expectations will generate some controversy. Alt interprets support for the welfare state, particularly in those areas that extend benefits to relatively small subsets of the population, as altruism born of prosperity. An example is support for state aid to the unemployed, which for many Britons is an altruistic act, since few favoring state aid can personally expect to benefit from it. As personal economic conditions deteriorate, altruism diminishes, presumably because people increasingly notice and resent that their tax outlay is being spent to benefit others. Thus bad times, paradoxically, may hurt the Labour party because of its commitment to welfare-state expansion. Working-class Labour supporters experiencing bad times become more critical of increased welfare expenditures, leaving Labour's middle class as the only major group advocating increased welfare expenditure in a time of economic decline.

The limits to altruism argument is fascinating but requires further testing. Alt relies on British-sponsored SSRC surveys from 1964 to 1974 and commercial polls from that period, with a few drawn from the post-1974 period. A fuller exploration of the 1974-79 Labour government would prove useful. The real testing ground for Alt's thesis may well prove to be the Conservative government elected in 1979, after the book was written. The Conservative commitment to a sustained reduction in expenditure may place the welfare issue in sharper focus. Alt's examination of continued and deepening economic decline could have been further tested by offering more in the way of regional analysis that the book does. Alt only briefly mentions Scotland and rarely contrasts the much more economically troubled north—whose economy has been in decline for most of the century—with the fairly prosperous south fas a means of assessing the long-term effect of economic regional decline.

Greater attention to regional analysis or extending the study to future governments is needed to see if the altruistic-non-altruistic dichotomy is sufficiently supple to interpret attitudes in times of sustained deterioration and substantial challenge to the welfare state. For example, on the question of attitudes towards welfare-state expenditure, voters may support aid to specific groups on prudential rather than altruistic grounds either as a means of responding to group discontent or because they see for themselves some future need for the aid in question. Distinguishing between altruistic and nonaltruistic attitudes may prove to be as elusive as distinguishing between valence and positional issues.

These concerns, however, do not detract from what is a needed and valuable contribution to understanding political attitudes in an age of lowered expectations.

JOHN G. FRANCIS

University of Utah

Political Action: Mass Participation in Five Western Democracies. By Samuel H. Barnes, Max Kaase and Associates. (Beverly Hills, Calif.: Sage Publications, 1979. Pp. 609. $25.00.)

The longest-running epic since *Gone With the Wind* has at last appeared in print! Written over a period of 10 years by a collective made up of Europeans and Americans, it forms one of the most extensive and sustained pieces of comparative research yet produced in political science. It is indeed resolutely comparative, concentrating on individual uniformities across five countries (U.S., U.K., the Netherlands, Austria and West Germany) rather than documenting national differences. The focus is on unconventional and direct political tactics—not necessarily violent—such as petitioning and demonstrating. These are viewed as a concomitant of post-materialism in the five countries, the argument being that not only must education and extended leisure increase participation, but also changing values and outlooks, particularly of the middle class, will alter the content of political participation. It will decreasingly take the form of broad socio-political movements for system change, and entail instead "an increase in limited, issue-bassed . . . ad hoc group actions" (p. 49). To an ever-greater extent, these will express themselves directly rather than through parties and pressure groups.

This is an interesting thesis, touched off by the student protests of the late sixties, but not necessarily less valid because of that. How does it stand in light of the analysis presented in this book? The authors would be the first to admit that their answers cannot be conclusive. Limited by time and resources to basing themselves on single cross-sectional surveys for each country, they can only extrapolate tendencies backwards and forwards in order to estimate changes over time. These limitations also prevented them from uncovering much evidence on overt protest behavior (a point which in itself rather detracts from the assertion that such behavior is widespread).

As a result, their dependent variable is not, as the book-title misleadingly implies, political action, but attitudes towards political action: in particular, whether particular forms of direct action are approved or disapproved, and whether respondents think they might ever undertake them in the future (having not done so in the past).

Approval of political action is viewed as affecting actual behavior not only by creating individual dispositions to undertake protest but also by producing a general climate of tolerance towards its emergence. The assertion is again plausible but untestable in the context of the book. Curiously, the country in which the "license for protest" is strongest is the Netherlands. In spite of the tendency for Amsterdam squatters to take to the streets, the Dutch are not noticeably more prone to protest than Americans, Germans or the British. Their replies here remain anomalies not resolved by discussion.

The analysis in general rests too heavily on the assumption that measuring attitudes toward protest is measuring protest behavior itself. Rather than concentrating on explaining such attitudes through other attitudes, the authors might have paid more attention to the question of just how far they do trigger action in the countries studied.

This said, many of the findings on attitudes are of interest. Approval of protest is certainly more evident among the educated young than among other groups—perhaps a pointer to the future? Moreover, approval of direct action is, if anything, stimulated by a heavy engagement in conventional political activity—there is no antithesis between the two. This finding certainly undermines any assumption that protesters are uniformly opposed to all aspects of the existing political system. For many people, direct action is an alternative to other forms of participation to which they may resort when pushed. The various types of action, from lawful demonstrations to occupations and property damage, are clearly arrayed by all respondents in order of increasing severity and risk of violence (to the extent that they form clear and well-validated Guttman scales). Engagement in protest action seems to be generally seen as an escalation of tactics in response to ever more intolerable political alternatives, and this holds without regard to country. Governments be warned!

The careful validation and use of cumulative scaling, as well as the analysis of ideologies, illustrates the methodological care and rigor of the analysis which forms a great strength of the book. It is worthwhile recommending to students as an example of field research in which the classic methods of index construction are applied as originally intended and operational problems are discussed with clarity and honesty.

The major thesis is that (protest) action has increased, is increasing and ought to increase more. What evidence is there for the first two points? Here we come to the problem that bedevils survey research: the lack of comparable data over an extended, or even in this case a limited, time period. Apart from comparing the reported inclinations of parents and children, and age-cohorts, all the authors can do is to point to the indubitable upsurge in protest in the late 1960s and argue for its continuation since. This reasoning implies, however, that protest activity has always been at the low level of the 1950s and 1960s (though, arguably, in Britain, the Suez issue and the Campaign for Nuclear Disarmament did create a comparable earlier level of participation). However, harking back to the demonstrations and street-fighting of the interwar period, it is by no means obvious that present direct action has reached a higher level. It could be argued that it has tailed off in the seventies compared with the previous decade.

Much of the argument for an increase in protest behavior rests on the equally problematic assumption that increasing segments of the population have shifted from a concern with order and economic security to a concern with equality and freedom of speech. Such "post-materialist" values, it is argued, will prompt an increasing recourse to direct action. Since equality and freedom are traditional concerns of the left, particularly of its middle-class supporters, the same point could have made more economically by citing the increasing strength of socialist and radical alternatives over the postwar period in the countries concerned—more than enough in itself to account for any change in political tactics.

These slight critical points by no means invalidate the argument that protest activity is an established feature of political participation to which activists will resort if they feel the conventional channels are blocked, and to which nonactivists can be roused if the stimulus is sufficiently strong. If direct action has indeed been employed in most mass democracies under a variety of guises since their establishment, there is an even stronger case to be made for its recognition as part of the general political repertory available for citizens as a means by which to influence their governments. This book has not established all its points. It has, however, forcibly drawn the attention of political scientists to a neglected but vital form of democratic political activity. The further comparative investigation promised by the authors should shed more light upon this, especially if they can now concentrate on actual protest behavior rather than attitudes which are only inconclusively related to it.

IAN BUDGE

University of Essex

Micropolitics in Contemporary China: A Technical Unit During and After the Cultural Revolution. By Marc J.. Blecher and Gordon White. (White Plains, N.Y.: M.E. Sharpe, 1980. Pp. xiii + 135. $17.50.)

This impressive little study is a product of the authors' exploitation of a unique interview opportunity in Hong Kong. In the modern China literature, it augments a shelf of very useful case studies of organizational microcosms—an urban neighborhood or a commune here, a ministry or a county administration there—that invite limited testing of broader interpretations. Marc Blecher and Gordon White actually improve upon the genre by entertaining a few leading hypotheses about the Cultural Revolution period themselves.

The result is more or less to confirm the main conclusions of Hong Yung Lee's *The Politics of China's Cultural Revolution* (Berkeley, 1978). A loose "rebel faction" allied in this unit in 1968 most of whose members and activists were people of middling or bad class background, old friends, non-party members, technicians (as opposed to workers or administrators) and particularly technicians in positions of responsibility. These "rebels" expressed through their style of political participation an acute sense of discrimination, and consequent frustration, under the party dictatorship of the mid-1960s. They acted in the desperate self-interested hope that the system might be changed.

By 1973, the last year of available data for this unit, their hopes had been dashed. The pre-Cultural Revolution party organization had essentially reconstituted itself, and everyone had grown "tired of politics." The later "Gang of Four" and "Four Modernizations" phases, unfortunately, could not be included.

Preceding their account of the Cultural Revolution, Blecher and White describe the unit in detail, including its personnel structure and operations, as well as the general political issues that had arisen there. These details are of more than usual interest because the case study shelf at present contains no other work about a technical body. The unit was located in an interior province and was responsible for some sort of field surveys. Almost half of its 240 employees were technicians. It was responsible ultimately to a ministry in Beijing (Peking).

Description and hypothesis testing both rest on a data base of 25 items of information about each and every employee of the unit, a treasure trove unheard of in the modern China field. The authors are most careful in presenting their data with appropriate cross-tab statistics.

The offbeat source of this wonderful archive might give some readers pause. It is a single informant, "Mr. Ji," a former employee of the unit. With "his encyclopedic memory" he was able to recall the data in 100 hours of interviews lasting over seven months. Blecher and White are sensitive to the obvious problems, and they make a persuasive case for Mr. Ji's remarkable mental powers. As a clincher they told him late on that they had lost their notes on factional membership and asked him to review each person's faction.

"To our amazement, he did so on the spot without a single mistake" (p. 7). I am persuaded.

GORDON BENNETT

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World Leaders: Heads of Government in the Postwar Period. By Jean Blondel. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. 282. $17.50, cloth; $7.95, paper.)

Jean Blondel's latest work is unfortunately neither thoughtfully conceived nor carefully executed. It aspires in a modest volume to analyze the structure, background, career pattern, and tenure of the top leaders of some 138 nations from 1945 to 1975. While this would seem the stuff from which cross-sectional number-crunching follows, Blondel's analysis, dotted with bivariate cross-tabulations, nowhere tests for strength of relationship or statistical significance. This abstinence might well portend, from a senior scholar, more informed and substantial insight as a welcome relief from purely statistical prowess.

But this is not to be. Blondel makes a number of implicit (i.e., undefended) assumptions about his data, all of which are objectionable. He assumes that each of his 138 nations are of equal weighting. Population size or economic strength are not considered as variables for analysis. So we get findings like this: "Only six countries outside these two areas [Latin American and sub-Saharan Africa] are ruled by a president: the United States, Algeria, Cyprus, the Philippines, Indonesia, and the Maldive Republic" (p. 57).

Blondel assumes that each nation is sovereign, with equal capacity to choose its own top leaders. The really interesting questions are where, when, and in what manner this assumption approaches credibility. Nowhere does the author take the opportunity to raise the question of outside intervention and its effects on leadership choice. One entire chapter, devoted to "routes to leadership," nowhere mentions the role of foreign intervention. In a world of sovereign, semi-sovereign, and non-sovereign systems, this lumping together is a remarkable failure in concept.

Consistent with the above, Blondel asumes that formal governments actually govern, that top titleholders are in fact the top leaders. "All countries have a government, and all governments have one or more persons at their head. Everywhere, leaders of government are viewed as essential elements of the political and social life of their country" (p. 37). For the most part, the study confuses officeholding with power. While recognizing the power of party leaders in the communist systems vis-à-vis government officeholders, Blondel will not deal with the de facto power of IMF officials in Zaire, or the U.S. Ambassador in Saigon (San Salvador), the Soviet Ambassador in Prague (Kabul), or a United Fruit executive in Guatemala. One theme developed by Blondel is that of dual leadership, especially in the communist systems, yet he is unable to extend this theme beyond his concept of the nation-state, to an area where it might break new ground.

The main text is filled with one and two-line analyses of entire nations over extended periods. Some seem accurate enough; some, such as the coronation of Jean Bokassa in the Central African Empire as an example of the potential for new monarchies, seem poorly grounded (p. 57); all are unsatisfying. As I have argued elsewhere, it would be preferable to select a smaller number of cases to provide for greater depth and a closer fit to key assumptions.

This study is intended as the first of a three-volume series, yet there is little effort to resolve relevant methodological issues. The author attaches considerable importance to making a dichotomous distinction between "regular" and "irregular" succession at the top (Ch. 3). Yet, aside from stipulating that "regular" transfers of top office meet the letter of existing law, Blondel tries to deal with the problem of a "climate of pressure" as follows: "All that can be said is that, where there is a broad consensus that the pressures are 'acceptable,' the resulting transfer is 'regular' " (p. 84). *How* broad a consensus, and acceptable *to whom*, are not defined; again, external pressures are not mentioned. As Blondel is aware, reliable information on "acceptability" is not generally available, and the author gives no indication as to how a determination of "regularity" was made for this study.

Some of Blondel's findings, on the identification of dual leadership in the communist systems, on the social background of top officeholders, and on routes to top title, have been better established and refined in works by John Hodgson, Robert Putnam, Ralph Miliband, and Klaus von Beyme, to name only a few. For whatever reasons, the author takes no notice of these works. Other findings, that long-term officeholders are more likely to enter office at an earlier age (p. 226), and are more likely to exit through death or retirement (p. 205) are not enlightening.

Finally, the volume suffers from annoying technical inadequacies. Several tables (pp. 90, 92 and 122) have percentages placed immediately next to frequencies with which they are *not* associated. Table 6 (p. 107) gives column totals for frequencies, but calculates percentages across rows. There is, on the other hand, no table of the nations included in the data set; the tiny bibliography is not useful.

JOHN D. NAGLE

Syracuse University

Planning and Budgeting in Poor Countries. By Naomi Caiden and Aaron Wildavsky. (New Brunswick, N.J.: Transaction Press, 1980. Pp. xviii + 371. $7.95, paper.)

In the six years since it first appeared in 1974, this book has become a minor classic among studies of public administration in developing countries. It does not deal with dramatic issues of ideology, or political conflict, or development strategy, or even the North-South confrontation. Instead, it focuses on the humdrum of Third-World public administration, the processes by which financial resources are allocated by governments, and specifically, the relationship between macroeconomic planning and annual budgeting. What accounts for the success of this book, and for the publisher's confidence that a paperback edition will sell in the 1980s?

The argument is straightforward: macro-economic, multi-sectoral, "comprehensive" planning has been a disastrous failure everywhere it has been tried. Because it requires accurate data, abundant economic resources, and the power of planners to impose their priorities and designs on regimes that are inherently pluralistic, macroeconomic planning is especially unsuited to countries characterized by poverty, uncertainty, and instability. Whenever planners attempt to step down from the world of abstract calculation and confront the realities of their societies, they become embroiled in the normal processes of decision making and their activities become indistinguishable from others attempting to cope with uncertainty, limited resources, competing goals, and conflicting demands. Instead of imposing predictability on society, the latter succeeds in absorbing the planners. The solution is to discard the wasteful, futile, and pretentious efforts at comprehensive planning and to rely instead for governmental resource allocation on the more modest process of budgeting, a hitherto neglected subject in the literature on development administration. Close to the shifting and often unpredictable realities of economic life and to the power centers of governments, the budget authorities, strengthened by modest, incremental improvements, provide the most promising avenue for strengthening management in the governments of poor countries.

This book appears to have been conceived in the mid-1960s at a time when macro-economic planning was the dominant fad among developmentalists and economic rationality was heralded as the social technology that would insure the achievement of modernization and the maintenance of the affluent society. The U.S. government was requiring that all recipients of U.S. foreign aid produce national multi-sectoral econometric plans; economics was becoming the master science for public policy; and PPBS was being installed in all the departments of the U.S. government. By the time this book appeared in 1974, the bubble had burst; PPBS had been abandoned in the federal agencies, and "new directions" had been instituted in U.S. foreign aid programs and in the World Bank. These new directions de-emphasized macro-economic growth, large-scale capital flows, and financial investment—the forte of the planners—and emphasized rural development, income distribution, popular participation —subjects that were unfamiliar to devotees of "takeoff" and "the big push" and for which economist-planners enjoyed no comparative intellectual advantage. The intellectual debunking of comprehensive national planning had been well under way before this book helped to administer the coup de grace, and not without a substantial element of overkill. The authors are inclined to accept at face value the testimony of planners about their limitations, failures and frustrations, while dismissing the benefits of longer time horizons, economic rationality, inter-sectoral consistency and similar useful perspectives which planners have brought to development policy in many governments.

To students of development administration, the uniquely useful and fascinating contribution of this book is its behavioral treatment of public budgeting. Budgeting is a subject with a rich history of normative and prescriptive writing oriented to tighter control and increased efficiency in the use of public funds. Comprehensive budgeting, program budgeting, performance budgeting are among the normative canons that have filled the literature on financial administration and provided the rationale for a significant flow of international technical assistance. But though every observer and practitioner has long known the practical tricks of the trade, which depart markedly from the prescriptive literature, they have been of little interest to scholars. Few of these practices escape the pages of this book—from the calculated underestimation of revenues, arbitrary ceilings, incremental authorizations, political bargaining, and releases based on available cash flows on the part of central budget authorities, to the padding of estimates, concealing of resources, special earmarking of funds, and deliberate underestimation of multi-year capital requirements by operating departments. From the perspectives of the parties concerned, the authors demonstrate that these behaviors are rational and are exacerbated by the uncertainties and the desperate shortages of funds that chronically confront the governments of poor countries:

> The pervasive influence of poverty permeates government budgeting in poor countries. Faced with multiple uncertainties they resort to tactics which give them room to maneuver. They favor short-term over long-term commitments, a generalized conservatism over exactly programmed outlays, political bargaining over economic analysis, and figures which can later be moved over hard-and-fast allocations. They desperately seek redundancy. The elaborate and often exasperating games which result are thoroughly grounded in the needs of spenders and guardians to create and defend a surplus when both are poor. The effect, as with so many other facts of life in low-income countries, appears to be perpetuation in a vicious cycle of misery, muddle, and mendacity. The conclusion drawn is that, as it is now organized, government financial administration is powerless to help the economies of the poor nations (p. 165).

As with so much behavioral writing, this book is far more convincing in description, analysis, and criticism than in prescription. Caiden and Wildavsky argue that macro-economic planning should be abandoned; emphasis should shift to improved budgeting, to "converting obstacles to opportunities," as they bravely put it. But when they attempt to be concrete, their proposals prove to be either very modest or impracticable. Improving staffing, cost-accounting, and financial reporting systems to increase the learning and error-correcting capabilities of governments are unexceptionable proposals dating back at least to Point IV in 1949. The proposal to reduce uncertainty about levels of revenue by converting foreign aid from project assistance to general budget support appears to be innocent of the extensive literature on the subject, including the failure of program assistance to influence the economic and fiscal behavior and the priorities of recipient governments, and the inability of donor agencies for their own political reasons to allot funds in that way. "Continuous budgeting" to facilitate adaptation to "emergent problems" recognizes that budgets are continuously being modified in response to contingencies both on the revenue and the expenditure side; it is therefore presented as a substitute for the formalism and the rigidities of annual budgeting. Nowhere is there an estimate of the institutional implications of this proposal—a process of financial allocations and presumably tax assessments dominated by finance and departmental bureaucrats from which political authorities would be excluded even more than at present. The concrete proposals for reform are disappointingly meager and unpersuasive and, not surprisingly, have had no influence on policy, despite the wide readership this book has gained.

In the continuing tension in the social sciences between the "rationalists" and the "incrementalists," this book comes down powerfully on the side of muddling through. But muddling through is not, according to Caiden and Wildavsky, the absence of rationality, but rather rejection of the abstract rationality of planners in favor of the pragmatic rationality of the numerous actors in government pursuing plural goals in an environment dominated by uncertainty and chronic resource scarcity. By imputing a higher rationality to the current actors, by celebrating their pragmatic coping and problem-solving behavior, and by their evident hostility to social engineering, Caiden and Wildavsky come close to the conclusion that, given the circumstances, whatever is, is rational and right. This attitude is compatible with the present mood in the social sciences which is skeptical about government intervention and reform, and with the powerful drift to neo-conservatism in public affairs. The success of this book has been due, in my opinion, to its devastating attack on the arrogant and over-intellectualized rationality of comprehensive economic planning as a form of large-scale social manipulation; to its refreshing description and explanation of budgetary behavior in the turbulent and resource-constrained conditions of poor countries; and to its skepticism and about a positive role for government in reforming existing institutions which enhance economic efficiency or improving living conditions in developing countries. The message of this attack on planning is reliance on the operation of pluralistic political and bureaucratic markets to formulate public policy and manage public affairs; there is no concession to the insights of the planners that the desperate poverty of these countries might call for a more dirigist use of public authority, even though the risks might be high. There is little in this book that suggests how public affairs in poor countries might be better managed and even less about substantive priorities and the organizational means of achieving them.

MILTON J. ESMAN

Cornell University

The Social Democratic Image of Society. By Francis G. Castles. (Boston: Routledge & Kegan Paul, 1978. Pp. xiv + 162. $15.50.)

Francis Castles has provided a perceptive analysis of Scandinavian Social Democracy in this short comparative survey of party origins and policy achievements in Sweden, Norway, and Denmark. Country specialists and scholars who use different measures of social welfare will no doubt fret about various omissions and interpretations of empirical data; but overall Castles' achievement is an impressive *tour d'horizon* of an important set of case studies of long-term Social Democratic efforts to humanize industrial-capitalist society.

The stated purpose of the volume is "to explain the political dominance of the Scandinavian Social Democratic parties and to demonstrate the consequences of that dominance" (p. x). To that end Castles describes, in his first chapter, contrasting patterns of Social Democratic and union development in the three Scandinavian countries, the parties' strategies for maintaining their status as the dominant political force in Scandinavia, and the emergence of "new problems and new parties" during the turbulent 1960s and 1970s. In his second chapter Castles addresses himself to the most complex and controversial theme of his study: namely, whether or not Social Democracy in Scandinavia "has made a fundamental improvement in the conditions of the working class. . ." (p. 50), and, if so, according to what criteria, and for what reasons. The concluding chapter (part 2 of the book, paradoxically labeled "Origins"), surveys sources of Social Democratic electoral support, class-group relations, and the weakness of the Scandinavian right.

Castles' study is not based on field research of his own, nor does it purport to present any substantive new findings. Instead, Castles' strength lies in his discerning summary and critique of contrasting perspectives on the Scandinavian experience. These range from what Castles describes as the "reformist interpretation," which largely celebrates Social Democratic policy achievements since the early 1930s and holds that they are replicable elsewhere, to the "radical interpretation," which "denies the Social Democrats any genuine achievement and presumably regards their continued historical success as a basically unimportant historical anomaly" (p. xii). Castles himself insists that "both interpretations are fundamentally incorrect," suggesting instead that the Scandinavian experience is singular and that generalizations from it are therefore "inappropriate."

To buttress this argument, Castles devotes most of the first and third chapters to a descriptive analysis of distinctive features of the Scandinavian Social Democratic movements. These include historical attitudes of tolerance on the part of traditional bureaucratic-conservative elites, which facilitated the peaceful emergence of democratic socialist unions and parties during the latter part of the nineteenth century; the close institutional and programmatic links between the parties and the national federations of labor in contemporary Scandinavia; and above all the Social Democrats' striking record of policy success (particularly in Sweden) in extending social services and facilitating a largely sustained pattern of economic growth during the past four decades. Underlying the latter achievement, Castles notes, is the fact that the three Social Democratic parties have consistently retained their character as working-class parties. By retaining the ideological trust of most industrial workers, they have thus maintained their status as the dominant party on the left throughout Scandinavia—thereby escaping the paralyzing division of the socialist movement in various other European countries while simultaneously achieving the integration of the working class within the larger social fabric on the basis of institutionalized power. It is this feature of Scandinavian society and politics, Castles contends, that accounts for the maintenance of relative harmony on their labor markets—and not, as other observers have claimed, the prior existence of consensual norms and institutionalized procedures for resolving conflict.

An important further indicator of the "insularity" of Scandinavian Social Democracy is the effect that Social Democratic policies have had on "conditions of the working class." In the lengthiest and most problematic section of his book, Castles addresses himself to this fundamental issue by seeking to evaluate empirically the consequences of welfare reform measures in an explicit comparative framework. He begins with a brief summary of prevailing radical criticisms of reformist Social Democratic parties (notably Frank Parkin's *Class Inequality and Political Order*, Praeger, 1971), and readily concedes that the Scandinavian Social Democrats have in fact failed to modify the basic "reward structure" of capitalism or to achieve social equality. But they have made a meaningful difference in the way people live, as demonstrated by their capacity to effect welfare state reforms.

Castles utilizes a composite "measure of welfare state provisions" to assess the social and economic performance of the 25 countries that comprise the Organization for Economic Cooperation and Development (OECD). His measure consists of four social indicators: (1) the current revenue of the general government as a percentage of the Gross Domestic Product (GDP); (2) total public spending on education as a percentage of the Gross National Product (GNP); (3) infant mortality; and (4) per capita GDP. Various problems are immediately apparent in this choice of indicators. Castles himself notes that they offer only an "approximation of the reality of welfare state provisions" (p. 66). Moreover, their choice lacks any compelling theoretical justification and is patently subjective (though by no means arbitrary). Nonetheless, Castles' effort to devise an applicable empirical measure of welfare state performance is a noteworthy improvement over the more superficial assessments of the Scandinavian experience advanced by both the reformist and radical interpretations.

In applying his index of welfare provisions, Castles finds that Sweden and Norway rank first and second, respectively, and Denmark ranks fourth in a rank ordering of the OECD nations. (The Netherlands ranks third, Canada ranks fifth, and the United States tenth.) Interpreting these and related descriptive statistical data, Castles concludes that "the Scandinavian Social Democratic claim that 'we're far ahead compared to other countries' is justified. With the single exception of the Netherlands, no other advanced countries manifest anything like the same consistently high levels of welfare effort and output" (p. 73). This achievement, in Castles' view, is demonstrably a product of political choice—specifically, the Social Democratic commitment to cultivate welfare—and not a generalized consequence of levels of economic development.

As is the case with any macro assessment of complex social phenomena, Castles' study can be faulted on various grounds. For example, he does not fully capture the complexity of the Danish party system nor adequately probe the recent volatility of the Scandinavian electorate. Nor does he utilize relevant documentary material to explain sufficiently the potentially explosive issue of wage-earner funds—a concept advanced since the mid-1970s by union leaders in Denmark and Sweden as a means to extend labor's influence vis-à-vis private capital. To have done so would have enhanced the book's indisputable status as a thoughtful and provocative assessment of the distinctiveness of Scandinavian Social Democracy. Whether the resulting debate on the prospects of economic democracy, as a step beyond the welfare state, will mean that the Scandinavian experience can remain insular is, of course, another question.

M. DONALD HANCOCK

Vanderbilt University

Supermadre: Women in Politics in Latin America. By Elsa M. Chaney. (Austin: University of Texas Press, 1980. Pp. 210. $14.95.)

This work is the first book-length study by a single author published in the United States about women in Latin American government. The core of the study is an analysis of women leaders in Peru and Chile, based primarily on 167 interviews with female politicians and officials in 1967. Elsa Chaney sets this analysis in a broader context by briefly reviewing the image of women in Latin American culture, the history of the women's movement in Peru and Chile, and more recent trends in women's participation in public life in the two countries. The different facets of the study are bound together by a single theme: women in Latin America, to a greater extent than elsewhere, are restricted in their professional lives to stereotyped maternal roles.

As one of the very few studies of women in Latin American politics, this book is a pioneering work. The selection of Peru and Chile was an informed decision, allowing Chaney to make generalizations to Latin America as a whole because Peru was considered one of the most "traditional" South American nations and Chile one of the most "modern." (One intriguing finding of the study is that, contrary to the conventional images, the attitudes of the Peruvian and Chilean women leaders were similar.) The many revealing quotations from women leaders indicate that Chaney was a skilled interviewer. The combination of the rich interview material with background overviews of women in Latin American culture and history in one brief book will make the work particularly useful for many readers.

Yet, in part because of the pioneering nature of the study, it is flawed both methodologically and conceptually. Chaney cannot present a methodologically rigorous case for her main argument because the necessary comparative and time-series data are not readily available. To show that women in Latin America are more strictly bound by maternal stereotypes than women elsewhere requires comparable data for women outside Latin America, but such data are absent. For some control group data, interviews with Peruvian and Chilean male leaders might have been conducted; comparison between the men and women would have been particularly interesting with respect to the leaders' priorities for national development and their views of their own careers. Chaney's argument would also have been strengthened by the inclusion of more current data. Completed in 1967, Chaney's interviews predate the emergence of the women's movement even in the United States. Chaney suggests, but does not demonstrate, that Latin American women's attitudes have remained stable over 13 years.

Although these methodological deficiencies were probably beyond Chaney's control, some methodological improvements were perhaps feasible. We need more information on the representativeness of the 167 interviewees, especially for the Peruvian sample. Chaney also tells us little about her selection of 12 women in each country for extended informal conversations and observation of them at work. One indication of Chaney's inattention to methodological detail is that she reports on page 107 that she observed "most" of the selected women officials at work but on page

131 that she observed "each one."

Even more important to Chaney's argument is a careful conceptualization of the maternal stereotype and explicit specification of the attitudes and tasks within this classification. Chaney acknowledges the significance of this issue in the introduction and conclusion of the book. She points out that conceptualization is difficult, especially because some analysts, in particular Latin American ones, may perceive the designation of certain abilities and tasks as uniquely "feminine" to be positive rather than negative. Yet, in the body of the text, Chaney does not acknowledge this analytical issue. Nor does she advance a working classification of attitudes and tasks under the "feminine" rubric. At times Chaney seems to stretch the definition of the feminine role so that the attitudes and activities of the women leaders fit into it. For example, in Chaney's analysis of interviewees' statements as to what, upon their hypothetical election to the presidency, their administration's three main goals would be, Chaney classifies the responses "more jobs" and "more, cheaper foods" as " 'housewifely' adjustments" (p. 146). This designation might interest Peru's recently elected president, Fernando Belaúnde Terry, who made the promise of a million new jobs a cornerstone of his campaign.

Despite these flaws, this book is a useful starting point for the analysis of women in Latin American politics. Its data will provide a helpful baseline for studies in the 1980s. Its argument is provocative and will influence future work.

CYNTHIA MCCLINTOCK

George Washington University

The Soviet Union Since Stalin. Edited by Stephen F. Cohen, Alexander Rabinowitch, and Robert Sharlet. (Bloomington: Indiana University Press, 1980. Pp. viii + 342. $22.50, cloth; $7.50, paper.)

Readers who have grown dubious about the usefulness of multi-authored studies may be pleasantly surprised in reading this volume. The general theme of developments in the Soviet Union since Stalin is focused enough to give the book internal coherence and yet open-ended enough to provide latitude for the individual authors. Specialists on the various topics covered in the book are unlikely to find much that is new here—indeed, several of the contributions have been previously published. But taken as a whole, the volume provides a broad analysis that transcends narrow specializations and will therefore be useful and informative to most persons interested in the Soviet Union.

Political scientists are likely to find the set of essays on "Society and Culture" the most enlightening, particularly George Gibian's sensitive analysis of "New Aspects of Soviet Russian Literature." Gibian not only provides a concise review of recent Russian literature, but he also analyzes the political dimensions of Soviet culture, which has no close analogue in the West.

The volume is divided into four parts: Politics, The Economy, Society and Culture, and Foreign Policy. The essay likely to have the most long-term importance opens the book. This is Stephen F. Cohen's "The Friends and Foes of Change: Reformism and Conservatism in the Soviet Union." Previously published as the lead article in the June, 1979 *Slavic Review* (and published here without the four short commentaries on the piece by other specialists and Cohen's reply to them), Cohen's essay attempts to provide a fresh look at the major forces that have determined the direction of Soviet policy in recent years. The most important mainstream attitudes he labels "reformism and conservatism," and he concludes that the Soviet Union under Brezhnev has become "one of the most conservative countries in the world." This short description does not capture the subtlety and thoroughness of Cohen's interesting analysis. Particularly noteworthy is Cohen's point concerning the problem ideology poses for the dominant conservative position in the present leadership: that even in dogmatized form, Marxist-Leninist ideology is based on the idea of change, and thus it serves as a useful tool for the reformist persuasion in the present leadership.

Probably second in importance is the last substantive essay in the book, that by Charles Gati on "The Stalinist Legacy in Soviet Foreign Policy." Gati argues, in effect, that the basic nature and approach of Stalinist foreign policy is still used by his successors. This is no mere piece of cold-war rhetoric, but a well-reasoned, thoroughly argued statement (which fits in well with the theme enunciated by Cohen concerning the essentially conservative nature of the present leadership). It is also a controversial position to take, and the overall value of the book is enhanced by the inclusion of three short "responses" to Gati's essay, by Roger E. Kanet and John D. Robertson ("The Soviet Union and Eastern Europe"), Alvin Z. Rubinstein ("The Soviet Union and the Third World"), and William Zimmerman ("The Soviet Union and the West"). Zimmerman's statement, which emphasizes the distinctions between Stalin's foreign policy and that of his successors (and which prompted Gati to write an afterword to further dispute Zimmerman's position), does a particularly good job of sketching the contours of the controversy.

Between the Cohen and Gati essays are 11 other pieces. "The Stalin Question," by Soviet dissident Roy A. Medvedev (adapted from Medvedev's 1979 book *On Stalin and Stalinism* and the only piece not connected with the conference at which these papers were originally presented) provides interesting details by a semi-insider on de-Stalinization and the attempted rehabilitation of Stalin in 1969. George Breslauer's "Khrushchev Reconsidered" provides a useful supplement to Cohen. Breslauer shows how the Brezhnev-Kosygin regime has been a "conservative reaction" to a number of Khrushchev initiatives, which has resulted in, among other things, "the abandonment of populist efforts to redefine the status of political authority." His conclusions should give pause to those who argue that popular participation has been significantly enhanced under Brezhnev. Essays by Erik P. Hoffmann and Robert Sharlet complete the section on politics and provide different perspectives on policy making.

Three essays in the second section of the book give an overview of current problems in the Soviet economy. Probably of most interest to readers will be Robert N. Taafe's "Soviet Regional Development." Among other things, the author gives considerable attention to problems growing out of the fact that much of Soviet energy resources are found in the eastern part of the country, while energy needs are overwhelmingly located in the west.

In addition to the aforementioned chapter on literature by Gibian, there are three other essays in the section on society and culture: John Bushnell's analysis of a variety of public opinion sources, which leads him to conclude that the Soviet "middle class" has become increasingly pessimistic about the prospects for continued material advances; Ronald Grigôr Suny's interesting case study of Georgia and Soviet nationality policy; and Peter H. Juviler's chapter on the Soviet family, which includes fascinating information on women "in rebellion."

As with all collections of this kind, the essays vary somewhat in quality. In general, however, the level achieved here is quite high. The book can be warmly recommended as a useful source on the Soviet Union in the seventies.

DONALD D. BARRY

Lehigh University

Development and Diffusion of the Nuclear Power Reactor: A Comparative Analyis. By Peter deLeon. (Cambridge, Mass.: Ballinger, 1979. Pp. xvii + 324. $17.50.)

Peter deLeon examines the research, development, demonstration and diffusion of "slow" nuclear fission power reactors in the civilian sectors in six countries—the United States, Canada, Britain, France, West Germany and the Soviet Union. He is interested in the relative importance of four objectives—science for science's sake, national prestige, development of efficient technology, and political equity. Furthermore, he relates seven attributes of civilian technology development to these objectives. Among the attributes he examines are the existence of a concomitant military program, the existence of a technology delivery system in each country and the role of an active scientific community. The actors he examines are the organizations and bureaucracies within each state, rather than, at one extreme, the nation state writ large or, at the other, the role of individuals. These organizations, e.g., government bureaus, the nuclear construction industry and the electric utilities, play different roles at the various stages of the technology development process.

The book contains a wealth of detailed information on the comparative politics of nuclear energy in highly industrialized states. It is clearly organized and written, but the attention to detail could very well overburden the reader who is not a specialist in nuclear power. However, there is a handy glossary to assist the reader who may be overcome by too many acronyms.

As a result of the relationships derived from case studies which are compared according to the framework described above, deLeon offers "the basis for a testable set of policy hypotheses that point in the direction of a general model of government-sponsored civilian technology developments" p. 274). But, as he himself says, his model can only offer modest predictive claims. However, he does view it as a policy analysis tool which can be useful in explaining the innovation of other civilian technologies, but he does not wish to claim too much or become procrustean. He quotes Abraham Kaplan approvingly, "We tinker with the model when we might be better occupied with the subject-matter itself" (p. 254).

Furthermore, deLeon mentions possibilities for new objectives and attributes which he did not include in this study, e.g., "national culture" and "technology for export" (p. 276). This line of thinking must lead to the conclusion that what we have is a heuristic framework rather than a model in a scientific sense.

There is one conclusion that deLeon might have emphasized had he written since the Three Mile Island accident and the economic realities of today, i.e., "The early commercialization of advanced technology might be a mixed blessing" (p. 250). This points to the fact that he leaves out a possible stage of the technology development process—the death or the decline of the efficacy of the technology on grounds of lack of prestige, efficiency and equity. Another lacuna in the analysis is that, while deLeon compares nuclear power reactors to fossil fuel plants, a comparison to renewable and decentralized sources of energy is lacking. A third area of analysis that could have been undertaken would be to stress transnational and geocentric approaches to technology diffusion by emphasizing the role of transnational firms and regional actors.

These are some areas for further thought and examination; they do not reflect on the excellent contribution deLeon gives us on the comparative politics of technology development and commercialization. It is a sound and thoughtful addition to the literature on technological change.

JONATHAN F. GALLOWAY

Lake Forest College

Democracy and Development in Turkey. By C. H. Dodd. (North Humberside, Great Britain: Eothen Press, 1980. Pp. 226. $22.50, cloth; $12.00, paper.)

This timely book appears when an increasing number of observers wonder whether Turkey's democratic political system will survive. Reports of violent conflict between various extremist groups, ethnic and religious conflict, and assassination of prominent political figures appear almost daily in the Turkish press. As its title suggests, this book offers some insights into the question of the development and survival of democratic politics in Turkey.

Democracy and Development in Turkey is perhaps the first general work on Turkish politics written from the perspective of a social scientist. Orientalists and historians have authored most general works on Turkish society. C. H. Dodd brings a fresh interdisciplinary perspective to his work and provides an informative and stimulating synthesis of much of the best and most recent scholarship on Turkish society. *Democracy and Development in Turkey* is also well written and well organized. After an introduction to Ottoman and Turkish history and society, Dodd examines political elites, political culture and political ideas, the normative framework of the Turkish Constitution, political parties and voting, the military and the bureaucracy, unions and industry, and students. He concludes with a valuable discussion of the prospects for Turkish democracy.

In most instances, Dodd is sensitive to different views among scholars. One example is his treatment of the question of whether the state or the economy was responsible for Ottoman decline. In a fair and balanced presentation, he persuasively suggests that the significance of economic factors was not as great as some advocates of dependency theory claim.

One of this book's significant contributions is its analysis of Turkish political culture and ideas. Dodd describes the political implications of such cultural themes as trickery and intrigue, strains of egalitarianism coexisting with a perception of the state as *devlet baba* (father state). He then goes on to examine these and other cultural themes in the light of available empirical studies.

A work which covers so many important areas is likely to raise some questions about interpretation. Dodd is perhaps overstating the case when in discussing the Kemalist period he speaks of "the notorious secularist regime of Mustafa Kemal" (p. 5) and "the massive secularization of the Atatürk revolution" (p. 76). Such remarks ignore the complex realities of the Kemalist era in which both Islam and local authorities were more important than many have hitherto acknowledged. An excellent treatment of Atatürk as "Champion of Change or Camouflaged Conservative" can be found in J. A. Bill and C. Leiden, *Politics in the Middle East* (Little, Brown, 1977), pp. 191-96. Similarly, Dodd tends to discuss Atatürkism as an agreed-upon set of principles. In fact, it is a hotly debated issue and is probably closer to a symbol which conflicting groups attempt to use for their own purposes. Dodd's excellent discussion of the military should have noted the growing concern expressed in the Turkish press about its increased politicization (and of many police units as well).

These reservations notwithstanding, Dodd's book is one of the finest introductions to Turkish politics and society available today.

ARNOLD LEDER

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Government Versus Trade Unionism in British Politics Since 1968. By Gerald A. Dorfman. (Stanford, Calif.: Hoover Institution Press, 1979. Pp. vii + 179. $12.95.)

In this book Gerald Dorfman traces the attempts of successive British governments during the decade 1968-78 to curb the political power of trade unions, particularly of the Trades Union

Congress (TUC). Following an introductory chapter, which treats organized labor's postwar emergence as a force capable of paralyzing national economic policy and which summarizes the book's central argument, the next three chapters chronicle the governments' "counterattacks." A final chapter examines the impact on union-government relations of Britain's membership in the European Economic Community.

The main strength of the book lies in the brief histories of TUC interaction with the governments during the period. These accounts are grounded upon a large number of interviews (more than 40 are listed in an appendix, and other anonymous interviews are mentioned) with nearly all of the main participants. Dorfman weaves his materials together skillfully to create a richly textured narrative. I especially admire his ability to write concisely without being overly parsimonious.

Dorfman's analytical perspective is unusual, and—despite the clear language in chapter 1—I originally misread his purpose. Following the conventional assumption that the key issue in British labor-government relations was "Who governs?" I finished the book with a feeling of disappointment, for all the interesting description provided few new insights on this question. The power of the unions to bring governments to heel during the period studied was amply demonstrated and the governments' counterattacks generally seemed feeble in comparison; but this situation was already well known.

Only after I reread the first chapter did Dorfman's intent sink in. Parenthetically, the book lacks a conclusion; thus, a final opportunity to disabuse me of my confusion was lost. Dorfman sets out his argument in the book's initial paragraph: "In spite of periodic industrial conflicts, government for the first time is gradually winning the cooperation of trade unionism for effective economic management." Rather than focusing on the overall power quotients of labor and government as I had assumed, Dorfman focuses on governments' attempts to *tame* labor and draw it into a pattern of collectivist politics that "might work at last to produce economic stability or even prosperity" (p. 5). The prospect of success depends, he suggests, "on the development of coinciding incentives for unions and government to cooperate; or, at the least, on the development of government leverage which would effect union cooperation or perhaps acquiescence." Although the title and many passages in the substantive chapters reinforced my original assumption, the thread of Dorfman's argument can be traced throughout the book.

Even when measuring governments' victories only by their abilities to co-opt labor, however, the record Dorfman sketches is at best mixed. Harold Wilson sought to restore collectivist cooperation in 1968 by helping the TUC leaders regain authority from the shop stewards, but the warring factions perversely united against the government. Edward Heath's conversion to collectivist politics in 1972—following the twin debacles of the Industrial Relations Act and the miners' strike—was too late, and the TUC leaders were too divided to cooperate with the Tory "enemies." Harold Wilson (and later James Callaghan) gained the TUC's cooperation in implementing the "social contract," a relationship that was until mid-1975 mainly to the unions' advantage; then the continued cooperation, spawned by the deepening recession, was mainly to the government's advantage. Also in 1975, the TUC voluntarily became involved with the government in the affairs of the European Economic Community, both for defensive and offensive reasons.

With regard to this mixed record, Dorfman is optimistic but worried. On the left, his chief worry is that "union-Labor Party cooperation is still an unproven possibility in less than crisis situations" (p. 132). On the right, his chief worry is that the TUC will again find it impossible to cooperate with the Conservatives for ideological reasons and that "the next Tory Government will probably go out of its way to avoid entangling relations with the union movement" (p. 104).

The experience of the first year of Margaret Thatcher's government makes the latter statement seem prophetic. The *Economist* (17 May 1980, p. 47) has characterized the present situation: "In just 12 months the government has overturned what had become almost a constitutional convention—the right of the unions to be consulted on economic policy." Following monetarist policies and the principle of laissez-faire in labor negotiations, the Conservatives are quite interested in "Who Governs Britain?" And they are not interested in sharing power with the TUC. As Dorfman has pointed out, organized labor's power is limited by its negative aspect (p. 6). Because of the state of the economy, the government's intransigence, and the TUC's disarray, the unions have not been successful in focusing their power against the Conservatives; the spectacular failure of the TUC's "Day of Action" on 14 May 1980 is a case in point. Nevertheless, on the basis of Dorfman's able analysis of the previous decade of labor-government relations, one should not be surprised if such factors as Britain's climbing inflation and unemployment rates should produce political consequences compelling the Conservatives to reassess their position and seek the TUC's cooperation. The *Economist* article quoted above concludes: "How long will the new dispensation last?"

The book under review is a sequel to the author's *Wage Politics in Britain 1945—1967* (Iowa, 1973), and the new book might have been entitled *Son of Wage Politics*. Perhaps in due course Mrs. Thatcher will deserve a Dorfman book all her own, *Grandson of Wage Politics*?

LARRY B. HILL

University of Oklahoma

Political Manipulation and Administrative Power: A Comparative Study. By Eva Etzioni-Halevy. (Boston: Routledge & Kegan Paul, 1979. Pp. x + 218. $22.50.)

Eva Etzioni-Halevy intends this book as a study of "manipulation of the rank and file public" by political elites in Great Britain, the United States, Australia and Israel. Curiously, in view of the histories and analyses under which the library stacks groan, the subject is said to be "frequently alluded to" but "much less frequently analysed and documented" (p. 1). In point of fact, little new documentation is adduced in this book, which relies heavily for data on standard histories, secondary sources and even textbooks. While this innocent reader found the discussion of Israel informative, discussions involving the other three countries lack the nuance and sureness of feel found in works by writers in full command of their material. The mainsprings of American political history are particularly unappreciated; party competition, the vast range of party types across a continental society, the distribution of power and norms through time, the correlates of voting choices, the dynamics of reform movements, are all neglected or poorly understood as excessive (and poorly digested) attention is paid to the urban political machine as the presumed key to elite manipulation.

The book is rather over-titled as well: symbolic display is excluded from analysis on the grounds that "non-symbolic" manipulation, that is, the offer by politicians of material inducements to attract votes "poses a greater thrust to democracy than the former." Vociferous demagogism is said to be less of a "menace" because it is easier to "neutralize" by the media and oppositions (pp. 8-9). No doubt many scholars would reverse this emphasis. In this study, political manipulation is synonymous with "micro-corruption," e.g., payment to individuals of jobs, money, services, etc. in return for votes. It is said to have been virtually eliminated in the U.K. and Australia since its nineteenth-century heyday, to have declined greatly in the U.S., but to remain significant in Israel.

Declines in "political manipulation" (read "electoral corruption") have occurred mainly, it is argued, because public administration in the U.K. and Australia became separated from electoral politics, thus reducing the payoffs available, a "modernization" process that was partially "arrested" in the U.S. and Israel. Accompanying this, norms emerged which "restrained" elites from using public-sector resources as payoffs to individual voters. In the United States and especially in Israel, these norms are stil marked by a certain ambiguity. This may very well be correct, but the data introduced in evidence are often handled superficially, even wrongly. Consider one of Etzioni-Halevy's illustrations. The Nixon White House sought to use the Internal Revenue Service, she claims, to "embarrass political enemies. And if the attempt was not as successful as expected, this was allegedly not because Nixon's staff of public servants refused to cooperate or because the Internal Revenue Service resisted political pressures and regarded them as unethical, but because it had a greater commitment to the rival party" (p. 187). This is quite wrong. The canard that the IRS was uncooperative because it was "controlled by Democrats" originated with John Dean in his days of "blind ambition" in an undated memorandum to H. R. Haldeman. In it, Dean complained that IRS Commissioner Randolph Thrower (a Republican) had been unresponsive because he had become a "total captive" of the Democratic assistant commissioners. The next commissioner, Johnnie Walters (a Republican) was no more forthcoming. He had not shown "leadership," being either "reluctant or unwilling to do so." As a result, Dean wrote, "We have been unable" to "crack down" on "left-wing" foundations, to stimulate audits of or get information concerning "our political enemies," or to place "R.N.'s" people in the agency. On September 11, 1972 Dean met with Walters and turned over a list of 490 "enemies" to be investigated. Walters discussed the matter with Treasury Secretary Schultz (a Republican), who told Walters to "do nothing." Dean again met with Walters on September 25 and inquired into Walters' "progress" on the matter. Walters advised Dean that the matter should not be pursued, but agreed to bring it up again with Shultz. According to the Senate Watergate Report, "The matter was never taken up again, and the list which was given to Commissioner Walters was sealed and locked in his safe in the Commissioner's office." There were some instances of apparent abuse from lower levels in the IRS, but the facts are in dispute as to who was involved. In any case, Etzioni-Halevy's illustration is inaccurate. Thrower, Walters and Shultz (all Republicans) did in fact "refuse to cooperate," and did "resist political pressure" because they believed it inappropriate to use the IRS for political purposes, not because of an absurd "greater commitment to the rival party." In writing about personal corruption, there is a profound responsibility to get one's facts right. In other instances as well (which space limitations prevent detailing), although none as damaging to concrete individuals as this one, the lack of solid research is evident.

L. L. WADE

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India's Political Economy, 1947-1977: The Gradual Revolution. By Francine R. Frankel. (Princeton University Press, 1979. Pp. xiv + 600. $35.00, cloth; $12.50, paper.)

India's political economy presents a "basic dilemma: How can a gradual revolution be accomplished" that can reach its poorest people "without exacting fearful costs in human life. . ." (p. xiii). In stating the problem concisely and dramatically, Francine Frankel charts her middle ground between the two most commonly argued approaches to Indian development. In proposing what she terms a gradual revolution, she rejects alike a simplistic revolutionary approach and a simplistic accommodationist approach to India's basic problems.

Revolutionary theories are rejected on two grounds. First, they are not appropriate to Indian conditions which, Frankel maintains, center on religion, caste, and family. A national movement based on class appeals, therefore, becomes very difficult to organize. Moreover, a revolutionary solution could result in "incalculable levels of destructive violence" (p. xii).

An accommodationist approach, such as attempted by Jawaharlal Nehru and India's first generation of post-independence leadership, also is not seen as appropriate to Indian conditions. From an economic perspective, Frankel agrees with Gunnar Myrdal that evolutionary growth models will not resolve India's complex problems. Carefully conceived investments reach a dead end rather than serving as catalysts for self-generating economic development. Political accommodation in India rests on the key linkage role of dominant landowning castes which block radical social change, i.e., "a significant redistribution of income, status, and power to alter the structure of society" (pp. 23-24, 575).

Most of this substantial study is devoted to an impressive and careful elaboration of these central points. In the first four chapters, Mahatma Gandhi's and Nehru's approaches are described and analyzed. Both attempted to resolve what Frankel terms the "resolution of the paradox of accommodative politics and radical social change" (p. 24) by essentially avoiding direct confrontations with the dominant landed castes. On the other hand, rural elites would be gradually weakened by increasing political and economic mobilization of the poor. Gandhi emphasized a Constructive Program for village uplift which not only would improve social and economic conditions, but, the author maintains, was also an organizational device intended to weaken the dependence of the poor on the dominant landed castes.

Nehru's ambivalence during the pre-independence period is explored, particularly as he led class-based actions in Uttar Pradesh and supported the emergence of the Socialist Group in the Congress party. After independence, the Nehru-led leadership emphasized the importance of the Congress party as a national unifying force within an accommodationist political framework. At the same time, it was anticipated that a socialist approach to society coupled with new village-level institutions could lead to pressure from below so as to accomplish radical reforms.

Nehru's attempts to speed up the process as in the Nagpur Congress Session in 1959 failed as the landed elements interpreted the measures for agrarian change as resulting in "their own destruction" (pp. 162f.). In turn, new village institutions such as *Panchayati Raj* were captured by large landlords.

India's experience, Frankel concludes, shows that "attempts at social transformation through institutional change that stopped short of direct peasant organization only compounded the problem of democratic reform" (p. 200). Propertied groups, rather than succumbing to pressure from popular majorities, were adept at manipulating the fragmented peasantry. Political unity and stability accruing from the accommodationist approach were "purchased at the price of effective implementation of economic reforms" (p. 434).

Prime Minister Indira Gandhi in splitting the Congress party in 1969 specifically rejected the accommodationist approach.. By this point, as Frankel reviews in great detail in separate chapters, there had been failures of plan implementation, attacks on the socialist principles of planning, continuing domestic constraints and foreign pressures, and crises involving political stability. Political language changed from that of conciliation to that of conflict as Mrs. Gandhi allied with leftist forces and seemed to be engaged in an effort to what Frankel terms the radicalization of Indian politics.

Intermediary relationships between the leadership and the populace appeared to be broken with the 1971 Indira wave that promised to eliminate poverty. An overwhelming majority in Parliament and personal power combined in what some commentators saw as a political version of the goddess

Durga. Prime Minister Gandhi's power, nonetheless, did not result in basic changes despite a spate of new programs.

Radicalization was no more successful for Mrs. Gandhi than the previous accommodationist approach. Economic conditions deteriorated while internal Congress factions applied contrary pressures, and opposition elements became increasingly militant. Her ringing election promises of 1971 and 1972 failed "under conditions of an organizational vacuum in the states and ideological polarization at the Center, both within the ruling party and between the government and the opposition. . ." (p. 490).

Frankel also deals with the June 1975 Emergency and its subsequent termination in March 1977. Her narrative and analysis does not encompass the Janata party period, nor the subsequent return to power of Mrs. Gandhi in January 1980. Nonetheless, Frankel's basic argument, as summarized in her final chapter, remains valid.

First, Frankel concludes, radical agrarian reform is essential so as to change the pattern of economic concentration in the rural sector. Secondly, non-political changes at the village level are not sufficient as agents of change. Cooperative and *panchayat* institutions are captured by the rural elites. Flowing from these two conclusions is her major operational point. Peasants must organize or be organized in class-based associations if the rural poor are to become politically effective (p. 548 f.).

In her notable book on *India's Green Revolution,* Frankel brought into bold relief the political costs that result. In this equally notable volume, she effectively sets forth the need for more effective political institutionalization in India. In contrast to the past, she emphasizes that it must reach down to the rural poor rather than just to the rural elite. Her conclusions are not necessarily new. Nonetheless, the quality of the presentation will establish this as a standard book on India's political economy.

PAUL WALLACE

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Mao's People: Sixteen Portraits of Life in Revolutionary China. By B. Michael Frolic. (Cambridge, Mass.: Harvard University Press, 1980. Pp. 278. $15.00.)

While interviewing Chinese refugees in Hong Kong in 1975, a moment of ultra-left policy surges in China, B. Michael Frolic developed an image of daily life in the People's Republic of China which differed fundamentally from that painted by short-term travelers. These travelers had been describing a crime-free China whose young people and old leaders happily and selflessly served peasants and workers in a climate of economic equality and popular, political participation. Frolic's people, in stark contrast, sketched in a rape-plagued China where citizens lived in fear of theft by strangers and uprooted youngsters escaping the countryside, a China where bribes were so frequent they were considered normal, not corrupt. Frolic's people in 1975 portrayed a China whose military and civilian officials lived in privilege and power with an unbridgeable chasm separating them from frightened, ordinary folk who lacked connections or power.

By 1979, Frolic confidently revised and condensed 16 refugee interviews into *Mao's People.* He pointed out that official accounts from China now supported the painful reports of his people and contradicted the happier tales of earlier travelers. Frolic is correct. After the October 1976 arrest of ultra-left leaders, a controversial attempt was initiated on high in China to confront honestly problems of the dark side of China's polity. For a most vivid, readable and useful introduction to vital aspects of pre-1976 daily life in China, Frolic's people, *Mao's People,* can be highly recommended.

But it is not a scholarly volume. It has little to teach the professional analyst of Chinese politics. In fact, any of the scores of foreign researchers in China or even readers of the Chinese press could by 1980 discover more and better information on all the major topics discussed by Frolic's people.

Of course, the book makes little scholarly pretense. It does not even have an index. Its 60 pages of introduction, commentary and notes are often too simple or even misleading. Frolic implies that "Let me talk" (*Wo lai tan ba*) means "timorously" (p. 163). He grossly overstates the number of full-time agricultural workers (p. 242). Frolic underplays the role of all but Li Zhengtian in the Li Yizhe group (p. 257 f). His annotation of names and events is superficial (e.g., p. 17, the 5-16 group; p. 270, Wang Jinxi). He is wrong that "Chinese oil is an area that has remained above politics" (p. 59). One has to know about the Socialist Education Campaign to understand the references to 1964 (pp. 26, 165, 187). In short, only a teacher who really knows Chinese politics can get maximum mileage out of some of the insightful comments, asides and anecdotes sharply recounted by Frolic's people.

A non-specialist, as popular reviews indicate, confuses learning catchy nicknames from Frolic's people with getting inside the real guts of Chinese politics. The first two tales, for example, are full of delightful memories of raising more and bigger pigs. The reading is great fun. But the serious politics is omitted. Nothing tells the non-specialist why, after the North China Agricultural Conference of 1970, an all-out drive to raise pigs was so much a major policy opposed by the ultra left that in 1976, Jiang Qing, Mao's wife, on seeing a new, large, village pigsty, literally shouted out the charge that it was a Deng Xiaoping village.

Frolic's people usually offer daily gossip, not political analysis. Frolic intentionally "avoid[ed] asking political questions" (p. 3). He also avoided adding sufficient political context to make the personal accounts politically meaningful.

Naturally, Frolic's people lie. He doesn't put in the time to sort out truth from fiction (p. 64). Only once, by accident (pp. 215-16), with regard to a vicious criminal, who presented himself as a victim of the system, is a lie discovered.

Everyone distorts. But some of Frolic's people's lies were so stark and self-serving that I could not help but conclude that Frolic did not fulfill his scholarly duty of setting the record straight. It is misleading simply to record, as Frolic does, two rosy accounts by Han refugees of inordinately favorable Han treatment of Chinese ethnic minorities, Li and Tibetan, especially during the Cultural Revolution. In fact, at that time traditional cultures were treated by Red Guards as feudal remnants requiring destruction. While it may not be surprising that Frolic's people understate the mayhem they perpetrated or overstate their loyalty to Mao, these distortions cry out for scholarly correction.

Most misleading is Frolic's ahistorical claim that one villager's life "story is fairly typical" (p. 23), that Frolic's 16 people are a representative "cross section" of China (p. 5). One never meets the tens of millions of peasants who support the privileged military and distrust the campaign to end its special privileges. Those peasants believe they benefit from military privilege. One only gets a hint (p. 142) of the military people who consider themselves the guardians of the revolution, people who could act to defend themselves and their revolution against Party democrats.

People change as policy and possibility change. One does not meet those who come out of the Cultural Revolution decade committed to science, democracy and markets or those committed to re-entrenching themselves against further shake-ups. There is far more in China, including continuing political struggles over its fate and future, than Frolic's low-level people can tell.

Too much is claimed for Frolic's people. But that does not stop *Mao's People* from being a uniquely important supplementary text for those beginning to study Chinese society. If it were an inexpensive paperback, I'd strongly recommend *Mao's People* for course adoption.

EDWARD FRIEDMAN

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Jawaharlal Nehru: A Biography: Vol. 1, 1889-1947; Vol. 2, 1947-1956. By Sarvepalli Gopal. (Cambridge, Mass.: Harvard University Press, 1976, 1979. Pp. 398. $17.50; Pp. 346. $20.00.)

Jawaharlal Nehru was among the great Asian nationalist leaders and statesmen of the postwar era. His public career spanned almost half a century and since his death in May 1964, the Nehru literature has begun to grow rapidly. Nehru's letters, articles and statements up to the year 1936 now being published by the Nehru Memorial Fund have already reached six volumes. The secondary literature has also begun to proliferate as scholars, academics, journalists and political contemporaries both in India and abroad begin to attempt to assess Nehru's impact on India and the world.

Sarvepalli Gopal, a historian, former Director of the Historical Division of the Ministry of External Affairs from 1954 to 1966 and now professor of Contemporary History at Jawaharlal Nehru University, is in the process of completing a three-volume biography of Nehru and his times. The first two volumes, covering the period up to 1956, deal with Nehru's gradual rise to prominence in the freedom movement and his initial nine years as independent India's first prime minister.

Gopal's biography is built around Nehru's letters, diaries and notebooks, the earlier ones lodged in The Nehru Memorial Museum and Library in New Delhi, those written after September 1946 in the custody of Indira Gandhi, Nehru's daughter. Gopal has combined his use of the Nehru papers with a careful review of archives in India and Great Britain as well as interviews with many of the surviving participants in the events of the period. The result is a significant contribution to our understanding of Nehru and modern India.

Gopal is extremely sympathetic to the hero of his youth. Although Nehru emerges as a thaumaturgic personality who runs the country almost singlehandedly, we learn very little about his personal life. Moreover, except for Gandhi and Motilal Nehru, Jawaharlal's father, almost all of Nehru's close associates appear to either tarnish his image or undermine his personal authority. Key Congress leaders are out of tune with the general Nehru line and receive little attention, while close personal associates prove to be inferior people who function in a conspiratorial fashion. Krishna Menon is pictured as a deranged personality; M. O. Mathai, Nehru's private secretary, is seen as a usurper, disloyal, avaricious and opportunistic; and Rafi Kidwai is simply a manipulator who provided unsteady assistance rather than solid support.

Although Gopal's biography is extremely well written, it fails to develop a set of common themes. In an attempt to avoid being too eulogistic, the author chooses to focus on the historical and impersonal forces at work in India and the world. The reuslt is a sketchy biography which lacks human content. It becomes more history than biography. We learn a great deal that is new about Nehru's political ascendency, his role in Uttar Pradesh Congress politics and his relationship to Gandhi. We learn very little of Nehru as a human being. The units of analysis are a series of major historical events from the Cripps Mission to Kashmir, Suez and Hungary. These events are reconstructed primarily from the flow of Nehru's letters and correspondence. The result is a discrete set of detailed shapshots with few links between them and little relation to a larger policy process.

The picture of Nehru as prime minister of India that emerges from these two volumes is incomplete: Nehru's major contributions are seen largely in foreign policy terms. Gopal's biography reflects a view of someone sitting in the External Affairs Ministry and not in the prime minister's secretariat. At least two-thirds of the second volume deals with foreign affairs or domestic issues such as problems in Kashmir or Goa which had a strong international dimension. Thus despite the work's skewed focus, this very preoccupation with Indian foreign policy will make Gopal's *Nehru* essential reading for those interested in the early foundations of Indian foreign policy. Nehru is credited with helping to secure settlements in Korea and Indochina, enabling the British and French to redress their mistakes in Suez and doing his best in the circumstances for the people of Hungary. Any errors which might have been committed in dealing with the Hungarian crisis are attributed to a deranged Krishna Menon, Nehru's U.N. representative.

All criticisms of Nehru's handling of foreign affairs are dismissed as unfair or untrue. Nehru did not, in Gopal's view, concentrate on foreign affairs to the detriment of domestic needs, he did not treat India's neighbors differently from other more distant areas and he was perfectly "clear-eyed" about the Chinese leadership and the innate potential for a clash with China. He simply did not expect it in the immediate future. As for the absence of equidistance in Nehru's nonalignment between East and West, it was the Western powers and their attitudes toward colonialism, racism and their willingness to contemplate a general war that were at fault, according to Gopal. In particular, because of their great economic and military strength, the Americans tended to be overbearing and treated India not as an equal but as a country to be humored. This American attitude created a distinct prejudice against the United States in Nehru's mind.

Although Gopal credits Nehru with a wide variety of domestic achievements, domestic affairs do not get the treatment they deserve. There is almost no discussion of the formulation of India's Industrial Policy Resolutions, the creation of the Planning Commission, the first and second five-year plans, Nehru's role in the Congress party, the Nehru-Patel relationship, and the creation and development of the Indian political system and its key institutions.

Gopal's narrative ends in 1956, the "summer solstice" of Nehru's prime ministership. There existed, he argues, a sense of fulfillment in the air and confidence in India's future destiny. Individual freedom, social justice, popular participation, planned development, national self-reliance and a posture of self respect in international affairs were steadily being achieved under the guidance of a prime minister brimming with self-confidence.

Although imperfectly balanced, Gopal's *Nehru* is the most significant biography of the late prime minister to appear since his death. It contains an enormous amount of new information and is essential to understandingg the foundations of India's foreign policy. The biography will be valuable to both political leaders and scholars, and one looks forward with great anticipation to the concluding volume.

STANLEY A. KOCHANEK

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The Cruel Dilemmas of Development: Twentieth-Century Brazil. By Sylvia Ann Hewlett. (New York: Basic Books, 1980. Pp. x + 243. $15.00.)

In form this book is a study in the political economy of development, using Brazil as an illustrative case. It is written by an economist who attempts to link her economic analysis to the central political issues of development and modernization. Given Sylvia Hewlett's approach to Brazil, this book, from the point of view of political analysis, fits into the general theme of "political models" of development. Brazil since 1964 is viewed as an example of a harsh authoritarian political model. This model is presented as the all but inevitable result of the regime's economic growth strategy within the national and international context of the time. Specifically, it emerged from a strategy of industrialization within the context of delayed dependent development—a situation characterized by certain "cruel dilemmas" which severely limit policy options. In brief, these dilemmas involve the inescapable political and economic costs of the growth strategy and particularly the maintenance of a system of state-structured and state-guided capitalism. In this mode of modified market economy the costs must fall on labor, the rural underclass and the growing mass of urban marginals. Authoritarianism is necessary to enforce the essential underlying pattern of distribution, and therefore civil and political liberties become another at least short-term cost or tradeoff of the approach. Most of the book is aimed at demonstrating this central point, and it documents the harsh economic and political consequences of the Brazilian model for the bulk of the population.

Overall, this work does not break any new theoretical or empirical ground. Basically it is a work of synthesis, both in terms of the theoretical perspective and the materials on Brazil. Hewlett begins from the well-established point that there are no easy ways to development and that all known routes involve costs and tradeoffs. The costs are determined by the nature of underdevelopment and the international economic order. The tradeoffs are determined by previous national patterns of political economy and strategic choices such as use of the market, role of international investment, industrialization versus agricultural development, capital-intensive versus labor-intensive industries, etc. Finally, certain global ideological choices—capitalism versus socialism—will have certain implications for emphasis on tradeoffs of social welfare values (capitalist Brazil) or of formal political freedoms (socialist China). The theoretical part of the study is a valuable synthesis of existing thought but it is a bit overly concise. Moreover, the study does not really dig deeply into the emerging body of literature on state society models in Latin America; nor does Hewlett probe the considerable debates on the relationship between economic states and political forms spurred in part by O'Donnell's hypothesis regarding the deepening of capitalism and modern authoritarianism.

The case-specific material is also mainly a synthesis of existing work. However, this material is handled quite well. Hewlett makes a great deal of economic information and analysis intelligible to the political scientist and she shows a sensitivity for the ways in which political and economic factors interact. The case study provides a compact introduction to the main issues of political economy in contemporary Brazil.

In the main, the theoretical and case-specific sections are well integrated and argued, with the exception of the concluding chapter which attempts to draw some comparative conclusions regarding Brazil and China. The book is well written and concise and should prove interesting and useful to social scientists interested in development, Latin America and Brazil. The major strong point of the book, however, is that it will heighten the political scientist's knowledge of and sensitivity to crucial aspects of development economics.

James M. Malloy

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African Upheavals Since Independence. By Grace Stuart Ibingira. (Boulder, Colo.: Westview Press, 1980. Pp. xvii + 349. $22.50.)

Grace Ibingira was a cabinet minister in Uganda until 1966 when he became a political prisoner, as a result of Obote's changes in the government. It was thought that Ibingira would oppose them. In 1972, he was released and served as ambassador to the United Nations until 1974. It is with this background that he analyzes the circumstances leading to the coups in Uganda, Ghana and Nigeria.

Ibingira compares them and then traces analytically first the effects of the colonial legacy on the governmental processes, and second the consequences of winner-take-all politics on each of these systems. Some interesting information is brought together about coups all over Black Africa from 1963 to 1978, with tables about imports and exports from the 1950s to the 1970s.

From these observations, Ibingira then concludes with some basic principles that should be included in the governmental process, including an ideological premise for government; leadership that accepts adversity and the idea of responsible opposition; constructing a sound, viable constitution; guaranteeing basic human rights; providing for "catch-up" programs in backward areas; an emphasis on common interests, promoting meaningful regional cooperation, and developing an enlightened foreign policy.

One should keep in mind Ibingira's own experiences as one reads these evaluations of the coups in his country, and in Ghana and Nigeria. The volume does share some useful insights on political development in the new African states.

Josephine F. Milburn

University of Rhode Island

Political Crime in Europe: A Comparative Study of France, Germany and England. By Barton L. Ingraham. (Berkeley: University of California Press, 1979. Pp. xiii + 380. $22.50.)

History has spared few polities what has been traditionally viewed as political criminality. Few acts have not been treated as criminal by some governing authority. The identification of political crime—irrespective, perhaps, of its cultural, political or historical setting—is the task that Barton Ingraham has set for himself, among others. Whether it is enough to say that it comprises acts which officials *treat* as political and criminal, is arguable, granted that this may well be the only view that withstands critical examination. What has been done quite effectively, at any event, is the marshaling of much of the relevant historical, legal, and sociological data taken from the three political cultures of which Ingraham writes, and the inspiring of sensitivity to a few inferences and propositions concerning political crime as it exists within the theoretical criminal law model that each culture retains. Historical scholarship, for example, provides us with an awareness that pre-Enlightenment doctrine stressed the need for personal fealty. Political crime was initially regarded as a breach of faith and allegiance owed to a personal monarch or lord rather than political opposition on ideological grounds to an institutional order or to a regime. With the approach of the French Revolution, however, some modification of the severity of penalties may be discerned with detention and banishment coming into use, and death as a penalty being seldom invoked. Though England provided the model for liberalism and constitutionalism, change, once commenced in France and even Germany, moved at a more rapid pace. In all three countries, the concept of the state was given new meaning with the growth of administrative government and bureaucracy, forcing recognition of separate aspects of political power with competing claims on the loyalty of the monarch's subjects.

The revolutions of 1830 provide an understandable springboard for what Ingraham calls the springtime of the policy of leniency. The doctrine that political offenders should be dealt with politically, not in accordance with the strict provisions of the law, but with a leniency designed to conciliate political differences between governments and their opponents is seen to have emerged in the third and fourth decades of the nineteenth century. Specific statutory measures, enacted to protect the political criminal from arbitrary prosecution by government and to alleviate the punishments prescribed for "political offenses," bear this out in France. In Great Britain, sedition undergoes redefinition. Even in Germany, where aristocratic-bourgeois governments long held sway, liberalism gains acceptance, with political dissent verging on respectability. A shifting perception of politically deviant behavior underlay all these changes; doubt and ambiguity invest its once "evil" nature. With the revolutions of 1848 and up to the time of the First World War's outbreak, the view of the political criminal as a man of honor whose crimes are prohibited and punished on political rather than moral grounds reaches its greatest degree of acceptance, even in official circles. Slowly, however, moral reprehensibility presses for reacknowledgement, accompanied by concomitant punitive measures. The failure of revolutionary mass movements was to play no small part, as was the growth of nationalism, marked by a shift of attention to *external* political crime like espionage and subversion. Increasingly, the populations of all three countries coalesced in support of their governments. The purely domestic radical began to be perceived as a betrayer and weakener of national unity rather than the patriotic harbinger of needed social change.

The last great phase of which Ingraham writes is a so-called Age of Ideology, characterized by a withering of the policy of leniency. Between 1914 and 1970 conventional political crime begins to lose its aura of respectability and to become equated with external political crime; there is a merging of the offense against authority with the crime of betrayal. Nationalism's growth, increasing democratization, Russia's revolution, and positivism's enhanced standing, all play a part. The statist and racist ideology of fascism completely repudiated liberalism and this was enough to lend weight to the positivist view that political behavior might necessitate social defense against social danger. The differential treatment of political offenders—meaning preferential treatment in the nineteenth century—had, after all, been perverted under totalitarianism, had it not? This basically negative view of political crime in the twentieth century is held out as ensconced; it is the author's view that the fundamental split in ideologies between East and West makes it unlikely that nineteenth-century liberal perceptions will return. It is there that his case rests, impressively supported throughout by a vast welter of authority diverse in nature as much as are the social disciplines that they represent. Pound, Durkheim, Parsons, Mosca, Kirchheimer, Guizot, Bentham, Beccaria, Dicey, Lombroso, Sorokin and Weber are but a few of the seminal thinkers repaired to, noted at the risk of giving offense to the many of more current vintage referred to. It is there perhaps that something of the argument tends to founder, however, given the absence of a more overarching theme than is provided on so essentially cosmic and transcendent a subject as the one embraced. There are a number of melodies, and each tends to be well defined. There is, for example, the one that contends that there is inevitably a conflict between measures taken by governments in self-defense against illegal political opposition and the principles on which the liberal legal order is based. There is also one that argues that the limited use of legal repression for defensive purposes has not led to the growth of a "police state," but rather it has been the failure to take such measures during periods of weakness that has led to the polarization of factions within society and the defeat of democracy.

It is, perhaps, the theme that the political philosophy shared by most members of the government, including the legal fraternity, that limits and shapes the legal measures adopted to meet the political exigencies of the age that Ingraham seeks most to underscore. Supported by a copious amount of documentation, this is no small insight. It is, however, in the realm of prophecy that some feeling of frustration tends to follow. Granted that ideology is characteristic of our age, it would be nice to get some sense of the direction in which we are going, tentative and philosophic as such prognostication might well be. It would be nice to know what will become of ideas if not for people like Ramsay Clark, or Philip Agee, or Barbara Timm, or Andrei Sakharov. It would have been well to speculate on the future, if any, of milestones like the Helsinki Accords and SALT II. That a venture into international politics would necessarily follow is inescapably true, but better even a slight misadventure than a form of obliviousness. It is, especially since the Second World War, that a discipline of international relations has come into being, and this kind of work suggests the essential inextricability of the domestic with the international. There is little to suggest, for example, that domestic inflation and unemployment are unrelated to the perception of Armageddon's threat. Election-year politics certainly makes this a valid question, at any event. The confluence of the disciplines becomes apparent too in these contexts. Tall ordering this may be, but there is nothing inconsistent about this kind of suggestion and the fundamental ambitiousness of Ingraham's work.

HILLIARD A. GARDINER

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Traditional Authority, Islam, and Rebellion: A Study of Indonesian Political Behavior. By Karl D. Jackson. (Berkeley: University of California Press, 1980. Pp. xxiv + 375. $20.00.)

Through historical study of village-level participation in the Dar'ul Islam rebellion in West Java and a survey of Sundanese villagers in 1969, Karl Jackson seeks to establish that "traditional authority relations" rather than ideology, *aliran*, media exposure, socialization into national symbols, economic deprivation or physical isolation, are the key to Indonesian peasant political behavior. Primary and secondary historical sources pertaining to the rebellion are cogently synthesized in the opening chapter, and an appendix contains translations of several documents, including a draft constitution for an Indonesian Islamic state. The final chapters are devoted to more theoretical concerns. The central concept of traditional authority is defined and differentiated from more material, opportunistic, and short-term patron-client relations. The conclusion that traditional authority and physical coercion are the most effective means of political integration among the Sundanese becomes the basis for a dichotomous (traditional-modern) typology of political integration. In their mechanisms of integration, societies will "vary substantially within the context created by (1) the modernization process and (2) the organizational modes chosen by elites and counter-elites" (p. 278).

Jackson postulates the generality of traditional authority bonds from the vast majority of sample respondents who perceived themselves in both roles of advisor-patron and advisee-client and reported weekly consultation and that some of these bonds had endured for more than 25 years. Combining respondents' reputed importance in village affairs with the number of clients indicated narrow leadership pyramids in each of three primary villages. Readers may be unconvinced, however, since 84 percent of the sample was composed of members of the village elite and most respondents admitted to multiple advisors instead of a single, omniscient figure; however, Jackson denies that this could have been a source of pluralism and thus could have been problematic for its implication of advisee choice.

Second, traditional authority bonds are said to grow out of advisor-advisee relationships because of expressed feelings of moral obligation and the lofty father-child (*bapak-anak buah*) relationship toward them. Jackson infers a *paramilitary* mobilizational potential from respondents' assertions about their capabilities of obtaining help from advisees when in danger. The connection between insurrection and traditional authority "is most clearly displayed" by responses to a fictitious, "projective" essay about "a disturbance in the region"; 42 percent felt that a man should without deliberation go to the aid of his ex-commander to whom he also has a financial obligation.

Third, despite "structural isomorphism" in the politics of the three villages, Jackson found in the former Dar'ul Islam villages greater centralization of respondents' advice-seeking about extra-village problems, higher support for the idea of an Islamic state and, among only top village leaders (N = 20), greater expressed willingness to kill for religion. Because top leaders in both pro-government and Dar'ul Islam villages were found similar in social background, political knowledge, and religious beliefs, Jackson infers that traditional authority structures binding top leaders to political movements beyond the village rather than intra-village factors best explain the above differences between Dar'ul Islam and pro-government villages. Yet this argument seems contradicted by his findings regarding differences between top leaders in their attitudes toward an Islamic state, their willingness to kill, and their age and occupation. Finally, Jackson presents historical sketches on each village totaling 12 pages to establish definitively that traditional authority relations "dictated the character and content of national integration and political participation" (p. 276).

The problem of drawing large inferences from weak, indirect and contradictory evidence aside, fundamental problems in the methodology and research design increase my doubts about the *reliability* of the findings. The usual difficulty of establishing causation is compounded here by the sensitivity of insurrection and religious fanaticism in interviews, the seven or more years that elapsed between the end of the rebellion and field research and the meager resources available for the research. Even if one overlooks the small sample sizes and some arbitrariness in selection of the initial 19 villages, in light of unknown and difficult field conditions, the procedures used to select the subsample of 3 villages from which the main sample of 200 villagers was drawn likely resulted in considerable reduction of variation and hence a bias against alternative hypotheses. The inclusion of only 8 percent of the villages' total population in the strata for sampling and the heavy elite sample (from 79 to 84 percent) disregards the importance of careful stratification with small samples. Jackson employs questionable techniques for statistically gauging the effects of village structure on individual behavior and uses weak bivariate techniques with alternating variables and an occasional three-way table instead of multivariate techniques to evaluate the relative influence of competing/alternative independent variables.

Jackson's flare for measurement and for questioning most past explanations of political behavior in the literature on modern Indonesia make this a suggestive work, if a problematic one. It is likely to be of greatest interest to students of the Dar'ul Islam and those contemplating survey research in rural West Java. Those with more general interests may benefit from the discussion of the distinctive style of supra-subordinate relations among the Sundanese.

DWIGHT Y. KING

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Socialism in Provence 1871-1914: A Study in the Origins of the Modern French Left. By Tony Judt. (New York: Cambridge University Press, 1979. Pp. xiv + 370. $42.50, cloth; $12.95, paper.)

British Socialists: The Journey from Fantasy to Politics. By Stanley Pierson. (Cambridge, Mass.: Harvard University Press, 1979. Pp. viii + 403. $17.50.)

The titles of the books here reviewed suggest a greater degree of parallel development and comparability than either their focus or approach provides. They both deal with the "peculiar trajectory of Marxism" (Judt, p. 310) the one in France, the other in Great Britain. Judt's study is a masterful synthesis of social and political history ("social history which ignores the state . . . necessarily ignores an important dimension of the life of even the most ordinary of ordinary men and women"), while Pierson offers an empathic intellectual history of "strange and difficult men" and of marginal movements. The difference in treatment is not solely owing to the fact that early French and British socialism walked a different path.

Judt's explanation why a fairly extreme form of Marxist-inspired socialism took root in the rural department of Var refutes the many reductionist (often ecologist) theses that have been advanced concerning peasant attitudes and especially those who like Marx and others have dismissed the specific political identity of the peasantry in France and elsewhere. What transformed the historical and economic characteristics of Provence into fertile soil for the socialist movement, Judt demonstrates, was the economic crisis of the last third of the nineteenth century. Out of that crisis emerged a need for the peasantry to shed the traditional and no longer meaningful authority of the radical-leaning village notables, and to acquire a more collective and participatory role in their own affairs. Socialism appealed to them in class terms as it did about the same time to the working-class Guesdists in the Nord. True, a Proudhonist undertow in all of French socialism made the link between distressed peasants, cooperative ideals and socialist politics easier to establish. Yet, instead of merely protesting in a negative, atavistic and anarchist fashion, the Provençal peasantry favored the intervention of the state deemed necessary to ensure their survival even at the price of some independence. This made the "urban socialism" of Guesde and of other Socialists even more applicable to the problems the peasants had to face.

Once a relationship between the rural communities and the socialist movement was established it grew into a tradition and acquired thereby the same characteristics of a "structural" feature of the region's social and political life as anti-clericalism had been earlier. The older and often described regional habits of intense sociability had undoubtedly helped provide favorable conditions for the rise of local socialist support. Once made part of the culture, organized left-wing politics became the new form of sociability, long after the socialist party, the SFIO, had lost much of its revolutionary character and impetus that had made it attractive to begin with.

In the three concluding chapters Judt seeks to determine which insights his regional study might hold for an understanding of French socialism in general and of its failure on a national scale. In doing so he undoubtedly makes a contribution to what he calls appropriately "our sense of the infinite incomprehensibility of France" (p. 248). But on this scale, his discussion, while still opening valuable new perspectives on a number of puzzling phenomena, does not have quite the same explanatory power as his regional study. One may follow Judt in his demonstration that the French peasantry exhibits some of the characteristics of a class. Yet his (admittedly tentative) attempt to link the politization of the French peasantry in the second half of the nineteenth century to some broader view of the relationship betweeen social class and political behavior, does not even explain fully the difference between left- and right-wing rural areas. Indeed, "under certain conditions," and obviously not only under those prevailing in the Var, the peasantry provided a favorable soil for the socialist (and later for the communist) movement (p. 279). But it does not become entirely clear what these conditions were in different parts of a country "whose political development, at the national level, so outstripped and outran its rate of social change" (p. 277).

A sober discussion of the weaknesses of the Left in France distinguishes between "views from within" and "views from without," reviewing and debunking, without any acerbity, the divergent arguments that are most frequently put forward in discussions of this kind. In Judt's opinion, a history of the Marxist Left in France (and in other countries of "Latin Europe") must recognize how central a place the peasantry and its problems have occupied in that history and to what extent they have determined both the strength and the weakness of modern socialism.

Some of the discussion dealing with the effect which the welding of part of the peasantry to the SFIO has had on both the peasantry and the party is quite brilliant. What had begun as a relationship born of a meeting between the revolutionary doctrines of the Socialists and the social discontent of the peasantry ossified into a common conservatism. But there are other and weightier causes for the socialists' conservatism and the communists' sectarianism which resulted in the failure of both. Judt himself is well aware that "a book which has taken as its subject one small department of France ventures into broader reflections at considerable risk" (p. 309). That Judt takes this risk even the reader who remains unconvinced by his arguments has no reason to regret.

In a study of early socialism in Britain one would not expect a discussion of socialist sympathies among farmers. But it is somewhat surprising that also the labor movement is all but absent from Stanley Pierson's book. His study proposes to clarify the dilemmas that arose from the struggle to relate Socialist ideology to political realities in Britain during approximately the same period covered by Judt's book. Why were the Socialists, he asks, unable to convert any part of the working class to their ideas? But the "political realities," indeed the social and economic structures of the era, hardly surface on the pages of a book which instead is almost exclusively concerned "with the individuals and groups calling themselves Socialists and, particularly with those who served as organizers and propagandists" (p. 3). For this reason the book's subtitle is somewhat misleading: it is a story of fantasies which never led into politics. The various forms of the Socialist ideology, divorced as they were from the labor movement, always dissolved into sectarianism and headlong escapes from reality.

In many ways the book appears to be an expanded and rewritten version of Pierson's earlier *Marxism and the Origins of British Socialism* (Cornell, 1973). What has been added, on the basis of new source material, are more detailed biographical data (informed in part by Erik Erikson's approach), more insights into the contrary pulls between competing groups, more sound and fury about futile intra-faction fights, but little new analysis.

If in France Marx was frequently tempered by Proudhon, the thinking of the British Socialists used Marxist analysis, as it were, only in passing and seldom without apologies. The foremost saints of the various socialist churches and chapels were utopian aesthetes such as William Morris, Carlyle, Ruskin, Nietzsche (again and again!), and of course, Christ. The spread of socialism among churchmen, both Anglican and nonconformist, and among the Scots and the Welsh who had reasons to feel colonially exploited, is discussed interestingly enough. There is, however, no adequate explanation of the spurious alignments of the various socialist movements during the First World War, here with chauvinism, there with pacifism. It is true that the prevailing confusion was the fault of the actors rather than the author. The postwar pro-Soviet sympathies of a small number of Socialists, Beatrice Webb among them, are characterized, probably correctly, as another fascination with a presumed "spiritual power" of communism, rather than as a political conversion. Earlier some of the socialist thinkers appear, at least in this study, as Leninists-cum-New Left *avant la lettre*; these marginal individuals, Victorians all and all socially estranged from the middle classes to which they belonged, dreaded the "trade-union consciousness" of the working class and, with only a few exceptions, frowned on any "permanent and intimate cooperation with the masses such as was urged by Marx and Engels" (p. 258, quoting from the *Social Democrat* of 1908).

As is well known, the only socialist group which came to terms with the British political system were the Fabians and they did so by losing their soul (or most of it) in the process. Pierson's discussions devoted to prominent leaders of the group are quite enlightening. But here too attention is focused on doctrine and personality rather than on explaining the ascendancy of the Fabians, which was responsible for the insertion of Clause IV in the present constitution of the Labour party.

Pierson concludes (p. 346) that "the Socialists . . . did much to awaken the lower classes politically." That may be true. But this book which shuns a discussion of the labor movement has not demonstrated this fact. And when Pierson expresses his belief that "their struggles (i.e., those of the Social Democrats, the Fabians and Ethical Socialists) still hold much to instruct those who seek the road to socialism in a new time" (p. 350), one is left to wonder. If lessons there are, are they not all negative?

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Comrades and Christians: Religion and Political Struggle in Communist Italy. By David I. Kertzer. (New York: Cambridge University Press, 1980. Pp. xxiii + 302. $24.95, cloth; $6.95, paper.)

The search for an explanation of the regular, if not spectacular increase in votes for the PCI in Italy since 1945, in spite of the battle waged by the Catholic Church and the parties it favors, has been a central concern among political observers of the *via italiana*. More than nine Italians out of ten are baptized Catholics but one out of three, and one out of two in the red belt, favor the PCI. *Comrades and Christians* seeks to understand how the major protagonists interact at the grassroot levels and how the people respond to the struggle. How does a hegemonic communist party maintain a neutral stance towards religion at the national level, while combating it in the towns and villages of the peninsula? How does religion fare in the political struggle in the midst of communist Italy?

This is a study of Bologna, a beguiling, though possibly an anomalous communist-administered city, and more specifically of an atypical *quartiere* of the town, an overwhelmingly working-class neighborhood located in its poorest area, with a stagnant population in spite of a proportionately strong immigration from the south (at least until 1972 when the basic research was completed). In Albora (a fictitious name) the PCI draws two-thirds of the vote, the left in excess of three-quarters and the DC barely one-sixth, and not more than 10 percent of the 9000 inhabitants attend mass with any regularity. David Kertzer, an anthropologist, spent one year as a participant observer in Albora, and in *Comrades and Christians* attempts to impart a sense of the people's daily lives. He offers an interesting study, supported by much information, well-penned vignettes, attentive description, careful analysis and discussion of theoretical issues.

Kertzer deals with the communist and Catholic worlds (and the secondary groups that support them), the policy of the former and the crusade of the latter, using national and local materials to introduce the main theme of the book, the impact of the "strategy of interpenetration of Party and popular social life" (p. 36). In Albora, membership in the party—and 1800 of 6200 adults are members—"is neither a sign of life devoted to proletarian struggle nor a sign of personal dissatisfaction or protest" but "a badge of social identity rather than an expression of political militancy" (p. 48).

The impact of the social pressures exercised by a dominant party is explained and illustrated in chapters describing the plight of heavily discriminated-against southerners and their conversion to the PCI, the neutralizing tactics used by the party against a short-lived Catholic *gruppetto* and against a poor man's storefront Catholic church which, in the wake of Vatican II, attempted to penetrate the area and threaten communist supremacy. Of special interest is Kertzer's discussion of the battle for ritual supremacy where he distinguishes between rites of passage and rites of community. In spite of the tensions created for communists and Catholics alike, rites of passage remain monopolized by the Church, while the party dominates community rites, having transformed the old Catholic *feste* into *feste dell'Unità*, and created its own sacred symbol, the *Resistenza*. If the party cannot openly oppose church rituals because of its national religious policy, it counterbalances them by its social supremacy. Having officially repudiated integralism, it pursues hegemony viewed as a process where, in varying degrees, "people are imbedded in the social networks, organizational structure and symbolic system of a political force that seeks their allegiance" (p. 255).

To ascertain the social bases of political allegiance, Kertzer spends in his conclusion more powder than is necessary "to counterbalance the excessively mentalist bent of the literature" (p. 238), notably the Almond cold-war social pathology model of communism and the "belly communism" theory of folk wisdom. He goes beyond the two-worlds, two-subcultures models looking for tensions and interpenetrations, focusing on the examination of local organizational policy and activity, people's norms and people's behavior.

The argument is convincingly made in favor of the study of rituals, symbols, and the importance of social dominance by one party; by choice Kertzer devotes little attention to the ideological determinants of the elite and people of Albora. This is unfortunate and might have been ascertained by a study of the *consiglio di quartiere* and the various party sections and militants, socialists included. One troublesome question is the self-fulfilling nature of the theory of social constraint in this particular environment where 80 percent of the people vote left. Possibly, ideology is a poor explanation for the vote, but in a diffused form, ideology could (and does) play a greater role in other mixed *quartieri* of Bologna or areas of Italy, as others have demonstrated. While Kertzer does not deny this, he tends to side-step the issue. He has nevertheless provided a solid study which helps fathom the complexities of Italian voting behavior while illustrating how the PCI operates at the grass-root level.

ROBERT H. EVANS

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Respectable Rebels: Middle Class Campaigns in Britain in the 1970s. Edited by Roger King and Neill Nugent. (New York: Holmes and Meier, 1980. Pp. 200. $30.00, cloth; $17.50, paper.)

This book invites comparison with *The Middle Class in Politics* (Teakfield, 1978) edited by John Garrard et al. The subject matter is similar and one of the editors of this book contributed one of the papers contained in the other book. Given the variety of approaches it includes and its effort to provide a bit of historical background, Garrard et al. is overall the better book. On the other hand, King and Nugent do provide more of the "feel" of politics and offer more for the student of pressure groups.

The stimulus behind both books was the increased willingness of various segments of the middle class in Britain to organize in the 1970s to protect their interests and, more importantly, to move outside the normal channels of party politics and to employ direct, almost disruptive tactics frowned upon by established pressure groups. This increased activism and militancy caused many observers to assert that the middle class was in revolt. Ascertaining the validity of this view is the underlying concern of both books.

King and Nugent focus primarily on the middle-class pressure groups that sprang up during the 1970s in Britain, providing an account of their activities and internal politics. Separate chapters discuss ratepayers groups, small business organizations, middle-class unions, the National Association for Freedom, and moral groups like the Festival of Light. Another chapter examines the relation between these groups and the political parties. While it would not be correct to say that these chapters are descriptive only, yet they are more reportorial than analytical. While the authors (four of the seven main chapters are by King or Nugent with the other three contributed by others) comment at times on the ability of the groups to influence policy, yet these chapters are not case studies of the policy process. Instead, the authors simply recount the events that seem to have stimulated the formation of the groups, the actions then taken, and the internal squabbles that frequently weakened the groups.

While one would not want the authors to waste time in arid disputes over definitions, their unwillingness or inability adequately to conceptualize the middle class is a problem. They doubtless are correct that the middle class is diverse and diffuse, but simply to leave it at that makes it difficult to provide any sensible answer to the question of whether the middle classes are in revolt. One of the editors' conclusions is that middle-class unionism has much greater solidity and permanence than do the other movements. But since the author of the chapter on unionism could provide no satisfactory definition of union—does one include professional associations, for example?—one hesitates to accept this idea without question. Interestingly, insofar as one can identify a phenomenon of middle-class unionism, its growth begins before the 1970s and thus differs from the other movements covered in this book in this way as well.

While the groups discussed have not been entirely uninfluential, they do seem to have been at the fringes of power so far as the policy process is concerned. Given their limited impact, one wonders whether they are worth this much attention, whatever their visibility. The justification lies not so much in the impact of the groups as in their relation to the parties and what this suggests about future developments in British politics. Growing unionism among the middle class is not necessarily a Labour party bonus. Since non-manual unions tend not to affiliate to the Labour party, there is a growing divergence between unions affiliated with the TUC and those affiliated with Labour such that only about half of the TUC's membership also belongs to Labour. Furthermore, in those non-manual unions that do join the Labour party, more than half of the members contract out of the political levy.

As for the Conservatives, while the party has had close connections with some of these groups—the National Association for Freedom, for example—yet many of them strongly have attacked the Conservatives, particularly when Edward Heath led the party, because they felt that the Conservatives were not adequately defending middle-class values and interests. Thus while the editors do not believe that the middle class as a self-conscious, unified group was in revolt in the 1970s, yet they do see its increased activism and militancy as part of the recent fluidity of British politics; neither party can count on automatic loyalty within this segment and must work at winning support; substantial shifts in middle-class votes from one election to the next are quite possible.

While a review should not spend time criticizing authors for failing to write the book that the reviewer would have preferred, I do find it strange that this book has so little on environmentalist groups, and says not one word about the long-running saga of opposition to a new airport in the London area. Attention to the latter issue probably would have revealed a good deal more about the political process and the distribution of power than do the accounts of several of the movements included in this book.

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Italy in Transition: Conflict and Consensus. Edited by Peter Lange and Sidney Tarrow. (Totowa, N.J.: Frank Cass, 1980. Pp. 186. $25.00.

To even the most casual observer, it is obvious that Italy has undergone tremendous political, economic, and social change since the end of World War II. But, as Peter Lange points out in his preface to this excellent set of essays which brings out both the conflict and consensus in Italian society, American scholars, with the exception of a very few specialists on Italy, have not had sufficient background to interpret these changes knowledgeably. American universities have placed relatively little emphasis on Italy and seldom has Italy been studied in a comparative framework. The result is that writers often have viewed Italy's economic, social, and political problems as unique, and because of their lack of background, they have tended to be unduly pessimistic about Italy's ability to weather crises. One of the aims of this volume is to show that Italy is a modern industrial nation and that its problems are similar to those of other advanced industrial democracies. However, the manner in which the political, social, and economic forces react with one another may occur only in Italy.

In the volume's concluding essay, Sidney Tarrow emphasizes how important it is to give a balanced interpretation to Italy's problems and points to the many encouraging developments that have taken place in Italy during the past 35 years. Among these are better living conditions for the great mass of the Italian people; increased educational opportunities for the average Italian; changing social attitudes which have resulted in greater freedoms, especially for women—divorce and birth control now are legal; lessening of barriers between classes; and the more effective role played by the trade unions. All these changes are illustrative of the democratization and modernization of Italy during the past three and a half decades.

The essays on Italian political parties are particularly valuable for the political scientist. Lange's discussion of the PCI's alliance strategy and of the Communist party's decision to play down this policy in 1977-1978 is a penetrating analysis. The essay by Pasquino on the Christian Democratic party, in which he discusses the changes that have taken place in the party during the past 35 years, the relationships between the Christian Democrats and the government, and the future of the party, contributes significantly to one's understanding of the strengths and weaknesses of the Christian Democratic party.

Hines' study of the Italian Socialist party is a lucid analysis of the pivotal role that the Socialists have played in Italian politics during the past two decades. Hines points out that the PSI has had an influence disproportionate to its size because "unless the Christian Democrat party were [to have gone] over the heads of the Socialists to work with the Communists . . . any coalition [formed in the 1960s and 1970s] had to enjoy Socialist support" (p. 133). Yet once the PSI formed a government with the Christian Democrats, the Communists accused the Socialists of abandoning their socialist principles. Because of the cooperation between the DC and PSI, the Socialists have lost support among left-wing voters and the PCI has been able to monopolize the opposition. The PSI never has solved its dilemma; as a result, it is "surviving, but as yet it is showing little signs of reviving" (p. 147).

Five years ago, Tarrow and Blackmer coedited a series of essays on Communism in Italy and France which have provided students of Eurocommunism with a wealth of valuable source material. This smaller volume on contemporary Italy should give the same sort of stimulation to those who are interested in Italian affairs and who believe that Italy's problems are similar, in many instances, to those of other Western industrialized nations and thus can best be understood if examined within a comparative framework.

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Conservative Party Politics. Edited by Zig Layton-Henry. (Atlantic Highlands, N.J.: Humanities Press, 1980. Pp. xvii + 286. $30.00.)

Perhaps because of their own reformist political inclinations, political scientists have investigated socialist and liberal political parties much more thoroughly than they have conservative ones. In recent years, however, this imbalance has been redressed to some extent. The British Conservative party, never as ignored by political scientists as other European conservative parties have been, is the subject of detailed scrutiny in *Conservative Party Politics.*

The book focuses on policy making and internal politics of the Conservative party in the post-World War II period, especially 1964-1978. Despite its publication date, the book devotes almost no attention to how the party regained office in the May 1979 general election or to its activities since then. Most of the contributors are well-known analysts of their particular topics, including Andrew Gamble (economic policy), Zig Layton-Henry (immigration), Vernon Bogdanor (devolution), Nevil Johnson (constitutional reform), David Butler and Michael Pinto-Duschinsky (social background of Conservative leaders), Patrick Seyd (factionalism), and Ivor Crewe and Bo Särlvik (public opinion). Newcomers Nigel Ashford and Martin Burch contribute essays on the European Community and party leadership styles, respectively. Conservative party professionals Chris Patten and Andrew Rowe write on the general problems of Conservative policy making in opposition and the specific problem of developing links to trade unionists.

Many of these essays take issue with the conventional notion that the Conservative party is strongly hierarchical in its internal power distribution. Instead, it seems that the Conservative party leader has to struggle in various areas to have his/her wishes followed by the front bench, the party in Parliament, and the constituency parties. This may simply be part of the well-documented tendency in recent British party politics for dissident groups to have influence on specific policies. An alternative hypothesis, however, is that party policy making in opposition is less hierarchical than in government. Burch's systematic, careful comparison of Edward Heath and Margaret Thatcher as party leaders indicates that the government/opposition distinction is an important one. In a complex analysis, Burch suggests several interrelated hypotheses to account for differences in policy influence, both in opposition and in government.

The major shortcoming of the book is that few of the articles are theoretically oriented. In addition to the Burch article, the only other strongly theoretical pieces are those by Johnson and Crewe-Särlvik. Johnson examines the philosophical tensions underlying recent Conservative thought on constitutional reform. He argues that once the Conservatives regain office, they are unlikely to pursue major reforms. The behavior of Mrs. Thatcher's government as of this writing indicates that he is correct. Crewe and Särlvik closely study the nature of British public opinion and its implications for alternative Conservative electoral strategies. They find three distinct attitude dimensions within the public—left-right socioeconomic, populist-authoritarian, and racial. Although at times their argument goes beyond the data, they have some suggestive observations about how the Conservatives can appeal to complex public attitudes.

The other articles offer competent description of recent tendencies within the party, although some of them, such as those by Bogdanor and Gamble, are more insightful than others. Among the topics not covered are Northern Ireland, Rhodesia, and defense. With reference to Bogdanor's comments on Scottish devolution, Ulster is another issue which the Conservative party, like its opponents, prefers to ignore. Enhanced by useful appendices and bibliography, this book is rich in information, even though it often fails to place the information in broader perspective.

DONLEY T. STUDLAR

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A Comparative Approach to Policy Analysis: Health Care Policy in Four Nations. By Howard M. Leichter. (New York: Cambridge University Press, 1979. Pp. x + 326. $21.50, cloth; $7.50, paper.)

Howard Leichter has written an interesting book, scholarly in its methods and tone, wide-ranging in its topics and evidence, thoughtful in its treatment of how and why the governments of Japan, Germany, Great Britain and the Soviet Union came to have the health care programs and policies they do. But the long title is a clue to the dilemmas he faced in giving focus to the effort. On the one hand he addresses the issues of comparative policy analysis, offering in the first part of this book a review of the competing conceptions, differing approaches, and conflicting findings of comparative analysts. For this effort, health care policy is not central at all, just one of many possible applications of the mixed approach he recommends. On the other hand, he "applies" his method to four cases in comparative health politics. Between the two efforts there is a large gap, one which Leichter only partially bridges.

Cross-national policy studies, as Leichter well understands, provide quite different types of data and inference from which one might learn. Some advance the limits of what is possible for government to do and teach us various ways of regarding familiar problems. Others allow us to weigh more carefully the determinants of policy; they permit the separation of culturally specific determinants from more generic ones, a task impossible within a single national case. In addition, cross-national study is relevant, though far from decisive, in appraising the probable effects of health policy options put elsewhere in practice. Finally, comparative analysis helps identify the ways in which different institutional structures shape the politics played out within them. In each case, the analyst stretches his mind, learning what is conceivable or, more ambitiously, what is likely under specified conditions. The major intellectual dangers lie in a facile importation of "lessons" from abroad or, still worse, a "world series" stance that seeks the winning strategy for a particular policy problem. National institutions and policies, as Leichter illustrates, lie embedded in particular histories and cultures.

Happily, Leichter's book is alert to both the benefits and the dangers mentioned. But the structure of the book nonetheless left me somewhat disquieted. While he makes a reasonable case for mixing the most similar and most different designs for comparative studies, Leichter has chosen the most different approach. He is interested in the effects of various regimes on public policy and accordingly wanted data on polities as different as Germany, Great Britain, Russia, and Japan. But the rest of his regime hypotheses would have been better applied to a wider set of nations and a smaller number of hypothesized relationships. At the same time, Leichter is drawn to configurative studies, efforts to sort out how what he calls situational factors, environmental influences, cultural and structural features combine to produce policy results in particular cases. So we are given literate, sensible, and thought-provoking essays on the health care policies of four regimes.

What we do not get is a tightly interwoven book on how health care policies and politics illuminate particular generalizations about modern governments. The sequence goes the other way around: plausible claims about comparative politics are subject to illustrative support or attack on the basis of characteristics of widely different regimes and those regimes' approach to medical care. At the close, Leichter offers some interesting "lessons" one might draw for the United States, but they are at some remove from the details of likely American experience. For that he would have had to investigate more similar regimes.

Not startling in method or finding, Leichter's book is nonetheless worth reading, particularly for those whose familiarity with public policy elsewhere is at the level of budget outlays. It is further worth reading because Leichter has composed it in an unorthodox academic style; he has not published these findings in articles and indeed is unknown to the specialists in health politics. That has, in some ways, hurt his book, as with the omission of the scholarship of Odin Anderson (on the U.K., Sweden, and the U.S.) and Deborah Stone (on Germany). But in other respects this book is a surprising and welcome addition to the literature of comparative health politics.

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The Collapse of Welfare Reform: Political Institutions, Policy, and the Poor in Canada and the United States. By Christopher Leman. (Cambridge, Mass.: MIT Press, 1980. Pp. xviii + 292. $19.95.)

The intensive examination of similarities and differences among a small number of countries has become an increasingly popular research strategy for scholars in comparative public policy. Perhaps as a response to the findings and criticisms of the comparative American state policy research of the 1960s and early 1970s, researchers have recognized the need to explore in much greater detail the ways in which politics makes a difference for the making of public policy. Increasing numbers of books and articles are now appearing which explore such questions for a small number of governments. In *The Collapse of Welfare Reform,* Christopher Leman provides us with one of the very best examples of this sort of work. Focusing on welfare policy and welfare reform in Canada and the United States, Leman examines similarities and differences, and argues that the principal explanation for the differences between these two similar systems lies not in political culture or socioeconomic factors but rather in political structures.

Chapter 2 discusses historical and demographic factors in the growth of poverty policy, emphasizing particularly the relatively greater insularity from public antagonism toward welfare in Canada. Chapter 3 describes the very similar reform proposals in the two countries which laid the groundwork for debate on welfare policy and reform. Chapters 4 and 5 chronicle the resulting debates over these reform proposals in each country. The core argument for the explanation of differences between the two countries—differences in political institutions—appears in chapters 6 and 7.

The book has several strengths which, I believe, distance it from other similar comparative efforts. First, it discusses the problems of poverty in considerable detail and in a dispassionate manner so necessary for social analysis, while at the same time attempting to explain the emergence of poverty policy and policy reform. Systematic efforts to explain public policy often deal rather clumsily with the concept of "problems" in that policy area. Likewise, treatments of policy problems often lack the dispassionate perspective necessary for good scientific explanation of resulting public policies. Leman seems to combine these two perspectives successfully.

Second, the chronological discussions of the stages in welfare reform are both non-tedious and comprehensive, and are appropriately intermeshed with analysis of the significance of those events. Third, the book devotes considerable attention to the economic bases for arguments over welfare reform. The discussion of the negative income tax and its complexities with regard to income supplement and work incentive is one of the best I have seen—although it is presented in entirely non-technical terms. The detailed treatment of the problem of measuring poverty in two countries is equally well informed.

The principal weakness of the work is that it really has no broad theory which guides the research. The "framework" presented in the first chapter is not a theoretical framework but rather a statement that the impact of politics on similarities and differences between welfare programs in two economically similar nations will be explored. The analysis certainly remains true to this purpose. But, in the end, the components of the explanation become very familiar ones reminiscent of the 1950s preoccupation of political scientists with the virtues of responsible party government in a parliamentary system. While there is not much new, however, in the contribution to theory, there is great value here in what is provided on this older and very important question. Leman shows in comprehensive fashion that variation in political parties, legislatures, elite goals, bureaucracies, and prior public policies contribute systematically to the differences. For that reason and others, this book is one of the very best analyses of public policy differences between countries. It should be read with equal care by specialists in public policy, comparative politics, and American politics.

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The Hare and the Tortoise: Clean Air Policies in the United States and Sweden. By Lennart J. Lundqvist. (Ann Arbor: University of Michigan Press, 1980. Pp. xii + 236. $15.00.)

Lennart Lundqvist has carefully analyzed the differences between the United States' 1970 amendments to the Clean Air Act and the Swedish Environment Protection Act of 1969. He concludes, as have such American analysts as Charles Jones, that policy makers for the "hare" (the U.S.), unencumbered by realistic considerations for technological or economic feasibility, chose a radical "technology-forcing" approach to pollution control. It set radical goals for the ambient air, centralized authority in the Environmental Protection Agency, provided for citizen participation in the enforcement process through lawsuits, and established unrealistic deadlines for the accomplishment of the goals. Swedish policy makers, concerned about unemployment and Swedish industry's competitive position in international markets, established no ambient air quality goals. Instead, the "tortoise" set up a franchise system through which individual industries bargained for permits from a centralized government agency (the Franchise Board for Environment Protection), from which process the public was excluded.

The reason for the different treatment by the "hare" and the "tortoise" was not a difference in the amount of pollutants in the air, nor even a difference in public perception of the problem, which Lundqvist describes as rising simultaneously in both countries in the late 1960s. It is rather owing to differences in the political structures, an appealing conclusion for those of us who wish to believe that the system does make a difference on policy outputs. Lundqvist argues that the Swedish parliamentary system, with its responsible parties and need to consider the possibility of taking over the administration of programs, forces individual politicians to be conservative in their approach to any new policy, as they must face the possibility of being held accountable for its failure. Ergo, the tortoise approach to public policy making. The individualistic politics of the United States, on the other hand, forces politicians such as Edmund Muskie to respond to the whims of the moment and formulate unenforceable policies. This makes for very plausible reading, especially when one considers the U.S.'s backing down on clean air depicted so graphically by Lundqvist after the problems of energy and the economy supplanted it in the public's mind in 1974.

Several problems remain unanswered. First is the characterization of U.S. air policy as a radical departure from the past and radically different from the Swedish. Neither is entirely accurate. Just as the Swedish clean air strategy came partly from modifications of its water pollution policy and partly from innovations made by the U.S., the hare's policy did not spring full-blown from the head of Zeus. It came after years of incrementalizing from the first Clean Air Act of 1956, which was in turn dependent on the water pollution control act begun in 1948. Admittedly, these two pieces of legislation contain assertions of public health goals and ambient air and water quality, but they are based, like the Swedish one, on an intricate system of guidelines for emissions control and individual permits that are negotiated in excruciating detail in the administrative process.

While the 1977 amendments did relieve industry of some of its responsibilities, it is important to remember the 1970 law's provisions for postponement of the automotive emissions deadlines as the cause of the first retreat on goals. Industry was not so asleep at the switch as it has been depicted, nor was the U.S. Congress so unrealistic or unresponsive to the traditional vested interests. Nor has all the hare's behavior since 1973 been strategic retreat. Prevention of significant deterioration of clean areas was added incrementally first by the courts, then EPA regulation, and finally ratified by Congress in 1977. While it is true that U.S. policies emphasize due process and the judicial role, it is also true that two can play at legalisms; industry has increasingly utilized its right to challenge administrative decisions in the courts. As Lundqvist rightly points out, U.S. policy has been less centralized than Swedish; and subnational politics has traditionally been more responsive to industry's economic arguments. A number of rival explanations exist for the hare's sluggishness in recent years other than a rapid response to changed public opinion (which, incidentally, polls indicate continues to support environmentalism).

American policy analysts, used to depicting the iron triangle of vested interest, congressional committee, and agency acquiescence as the example par excellence of bargaining, will be shocked to learn that Sweden is the true home of incrementalism. One wonders, given industry's stranglehold on the decision-making process portrayed by Lundqvist, why the Riksdag bothered to adopt a pollution control act at all. One further wonders how Sweden's progressive policies on such topics as social welfare, abortion, gun control, and taxation ever came to be. There is less mystery involved in the "steady progress" the tortoise has continued to make in air pollution; it must be attributable to the hare. If one's primary criterion for air pollution control is technological and economic feasibility, one clearly depends on someone else's (a technology forcer's?) development of the feasibility. This theory is given credence by Lundqvist's mention of the Swedish auto industry's response to American emission standards which in turn enabled the Swedish Pollution Control Board to adopt higher standards.

The representation of U.S. policy makers as eager respondents to every breeze of public opinion must also be questioned. Given the enormous public support for such policies as freedom of choice about abortion, as well as gun control, it is obvious that members of the U.S. Congress are bound much less by the pressure of majority opinion than they are by the threat of the organized few on crucial issues. Today's impasse on energy policy is a good example. Lundqvist describes the U.S. public as having been preoccupied with energy problems since 1973. Yet no radical departure from previous policy has been made, although innumerable suggestions have been offered. The iron triangle of policy making seems to be effectively representing the point of view of the major interest group (the energy industry). Yet environmentalists have been able to stalemate some of the most ecologically irresponsible programs also. The American system of gradualism (or even immobility) seems to be intact. Perhaps the Clean Air Act amendments of 1970 were an aberration that Lundqvist has mistaken as the United States' normal mode of policy making, or perhaps it was not such a tremendous departure from normality.

LETTIE M. WENNER

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The Politics of Social Security in Brazil. By James M. Malloy. (Pittsburgh: University of Pittsburgh Press, 1979. Pp. xii + 200. $13.95.)

James M. Malloy has written an insightful and interesting analysis of the relationship between the state and society in Brazil, as viewed through the evolution of the country's social insurance system. Classifying Brazil as a patrimonial state that experienced delayed dependent capitalist development, Malloy shows how social insurance policy was used by the bureaucratic-administrative elite to link "potentially powerful groups into dependency relationships that are controlled from the top down" (p. 16). More specifically, the social insurance system originated and, for most of its history, was used as a mechanism to coopt first organized labor and later, the organized rural population. Today the social insurance system includes more than 43 million rural and 45 million urban workers.

Malloy devotes most of his attention to the two periods when the social insurance system worked more or less as planned, that is, during the Vargas period (1930-1945) and the current period of military rule that began in 1964. Malloy persuasively argues that social policy toward labor was "launched when labor was at one of its weakest points organizationally" (p. 33) and as a result, did not have "the power to define the key components of the program aimed at its problems" (p. 49). The entire social insurance system was an attempt by the political elite to escape class conflict by dividing organized labor into numerous unequal and competing groups. Such preemptive incorporation of labor led logically to a dependent and easily manipulated labor movement.

The most fascinating section of Malloy's analysis, however, deals with the 1945-1964 populist period. During these years, the bureaucratic-administrative elite lost control of the patronage that the social insurance system provided. The state continued to grow in size and formal power; however, "its capacity to act effectively in a number of important policy areas actually declined," in great part because "the patronage system [had fallen] under the control of middle-level political and labor leaders who used it to build their own power bases and further their own careers" (p. 78). Again, in Malloy's words, "While the Vargas regime had created a system in which no single group or alliance of groups could dominate the state for its own ends, the very structures of state cooptation and control allowed particularistic groups to reverse the flow of the process and in turn coopt and control pieces of the state" (p. 82). Malloy attributes this development to the inherent weakness of the Brazilian state: "If, under Vargas, the unions were in a sense weak and coopted, so was the state; the weak, in effect, fed off and depended on the weak. The upshot was increasing rigidity and immobilization" (p. 90).

The events of 1964 reversed this trend. After the coup, independent unions were destroyed. In the countryside, independent rural organizations were replaced by a "vast array of state-sponsored and controlled worker and employer unions" (p. 131). A rural social protection scheme was instituted as part of a system for the cooptation and control of the rural population. To state it differently, the 1930-1945 system for the cooptation of urban labor now has been adapted to the countryside. Malloy ends his study on a pessimistic note: "In the rural area in particular, overall central control has grown and the capacity to mobilize broad-based autonomous action has diminished" (p. 133).

Is it possible that Malloy is unduly pessimistic and that his pessimism is the result of his overemphasis of the bureaucratic, authoritarian and corporatist aspects of the Brazilian political system? The 1945-1964 period was not, after all, an aberration. It was part of the political system itself. The facade of the Vargas regime was stronger and more authoritarian than its interior. The attempts of a weak state to coopt a growing labor movement had built-in limitations that became manifest after several decades. Perhaps the same process will repeat itself in the future, as leaders of the now-coopted peasant organizations gradually appropriate to themselves the state's power to distribute patronage and use that control to pursue goals that are different from those of the bureaucratic-administrative elite. Such behavior already characterizes sectors of the labor movement. The fact that peasants were initially organized and incorporated by a weakly institutionalized military regime makes it at least reasonable to anticipate future independent action by now-coopted rural groups.

SUSAN KAUFMAN PURCELL

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The October Revolution. By Roy A. Medvedev; trans. by George Saunders. (New York: Columbia University Press, 1979. Pp. xix + 240. $14.95.)

This very interesting volume deserves two reviews, one on its analyses of the Bolshevik Revolution and one on what it reflects in contemporary Russia. Roy Medvedev is a most prominent Soviet dissident. This is his latest effort at honestly discussing Russia's past in the face of the Soviet state's relentless determination to control the image of that past to make it legitimize the present.

A renegade to Soviet authorities, Medvedev consistently asserts he is a true Marxist-Leninist.

Medvedev's objective is to explain where Lenin and his revolution went wrong, which of course means dismissing the official view. He considers the possibility that the revolution was premature, as various socialists argued at the time, but rejects this too. A revolution must be made, not simply awaited, and Lenin was right to seize the opportunity to achieve social justice and end the war. He finds Lenin's genius to have been indispensable at that point.

While applauding their intentions and often realistic judgments, Medvedev finds Lenin and his colleagues held to certain pre-revolutionary views that were "premature, unrealistic, utopian, and simply wrong" (p. 89), leading to serious errors once they were in power. He criticizes the dissolution of the Constituent Assembly; the proper course was to postpone the elections until they could better reflect Bolshevik popularity. However, the gravest mistakes lay in the reordering of economic life. Medvedev attacks the abolition of private trading, the attempt to organize everyone in cooperatives and the wide-ranging nationalization of industry as unnecessary measures and beyond the fledgling state's capacity. Black markets sprang up, speculation flourished, industrial production shrank, and—lacking goods to acquire for their grain—the peasants withheld it.

At this point, Medvedev argues, the government should have been conciliatory, should have adopted a tax-in-kind system to encourage grain production, should have permitted the revival of trade, and should have curtailed nationalization. In 1921 such steps were the core of the New Economic Policy (NEP); Medvedev wishes they had been adopted three years earlier. He feels that, along with such policies, a coalition government with other socialist parties would have been appropriate.

Instead, the Bolsheviks used coercion. Workers' detachments and military units were used to seize grain, and the official use of terror spread. Committees of poor peasants, set up to assist grain confiscations, proceeded to seize the larger peasant landholdings in a second wave of land reform. These policies alienated the Bolsheviks from the masses, making bitter civil war almost inevitable. Lenin learned from this and introduced NEP, but Medvedev feels his successors have returned to the mistakes of 1918 and persistently mismanaged rural life at great cost. Thus he concludes, "It does not hurt to keep in mind certain aspects of NEP even today" (p. 187).

His discussions are clear and stimulating throughout. However, for Western scholars many of Medvedev's interpretations will be familiar or flawed. The Bolsheviks get too much credit for organization and political realism. Their rural policy is described as mistaken, but seems more nearly an expression of profound antipathy toward and ignorance about the greatest part of the populace, hardly the epitome of realism. Medvedev ignores the fact that NEP was adopted when the Bolsheviks were eliminating their last organized political rivals. He doesn't see those two steps as connected, but a NEP in 1918 would have meant a dialogue and even a coalition with other parties (as Medvedev suggests), contrary to the pattern of Lenin's entire political career and style. Lenin was tolerant in politics only when his power was unchallenged and unshared. In fact, nowhere in the book is there a sense that revolutionaries thirst for power independently of their ideological goals.

As a Marxist analysis, Medvedev's views also fall short. Engels, Plekhanov, and Trotsky (among others) feared the effects of a premature proletarian revolution. Post-1917 events often seemed to confirm their apprehensions, yet Medvedev rejects such views in unconvincing fashion. He refers instead to understandable mistakes and misjudgments amid excesses of revolutionary enthusiasm. Surely a Marxist analysis would trace mistakes and failures to larger forces, to the socioeconomic context in which they occurred.

But the book merits attention for the attitude it reflects, a commitment to honest inquiry so that Russians might learn from the past rather than be hostage to it. In Medvedev's account the revolution's heroes make mistakes, their opponents are not without insights and virtues, and the meaning of events is open to debate. Medvedev believes that only Soviet historians can assemble a true picture of the revolution. For ourselves this is dubious, but he is surely correct for his own society. Soviet historians must guide their fellow citizens in exploring the events and implications of that misshapen struggle. He is also correct in seeking to reaffirm the goals of 1917. In that direction lies the reformation of Soviet life and the true triumph of the Russian Revolution.

PATRICK M. MORGAN

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Patterns of Policy: Comparative and Longitudinal Studies of Population Events. Edited by John D. Montgomery, Harold D. Lasswell, and Joel S. Migdal. (New Brunswick, N.J.: Transaction Books, 1979. Pp. vi + 345. $16.95.)

It has become practically a ritual for book reviewers of edited volumes to comment on qualitative differences between chapters and lament the lack of a clearer overall focus or analytic framework. This edited volume, however, is qualitatively different overall. In fact, it is exactly the diversity of approaches to studying population policies which is the primary issue of inquiry in this volume.

As theoretically argued in the introduction and several of the chapters, policies affecting population changes cannot be understood unless more careful attention is given to the multivariate interplay of political, economic and cultural factors which have influenced population increases, decreases and movements. In order then to discern patterns of population policy, these authors argue that a more multidimensional analytic approach is needed. The approach suggested is seen as having three related dimensions. First, there are the "internal dynamics" of any particular population policy experience. Here the use of single-nation or comparative case studies of the decision-making actors and the sequences or circumstances leading these decision makers to act as they do on population policy matters is cited for analysis. Five such "internal dynamics" case study chapters, covering countries from Central America to South Asia, have been included in this volume.

The second related dimension is the "context" within which population policy issues arise—from the context of policy initiation through the context of policy implementation. As pointed out in several chapters, the policy "context" can vary dramatically at different stages of any particular policy process. Hence the actors that are important in the internal dynamics of policy initiation may become ineffective and less important than other actors or institutions at the stage of policy implementation. Such changing patterns within any particular policy process are seen as being influenced by four types of contextual elements. Differences in levels of economic and social development, differences in cultural values, differences in past policy decisions, and differences in political institutions are seen as interacting factors which must be studied comparatively in order to discern patterns of policy context upon population policies.

Several chapters analytically wrestle with this question of interacting contextual factors influencing the internal dynamics of state policy actors and institutions in terms of public and private decision making. Both "public policies" and "civic policies" (actions taken by individuals and families) are correctly seen as being susceptible to the classic problems of defining rational and irrational decision making and predicting intended versus unintended or indirect outcome effects. This question of how to distinguish analytically between public and private decision making in policy areas as personal as the issues of birth, marriage, residence, mobility, death, etc., is provocatively discussed, but certainly not permanently resolved. Particularly useful for comparative political scientists are some of the questions raised in several of the chapters about the problem of how to define more adequately the differences between public and private population policies. This is especially a problem for studies of developing countries where the transition from more private traditional decision-making units, such as family, clan, ethnic group, etc., to the more public processes of the laws and regulations of centralized state bureaucracies is still under way. Analytic reminders of this need to incorporate both public and private decision-making processes into any description of any particular population policy or broader statements about patterns of policy are indeed important. Far too many single-nation or comparative studies in the policy research field still erroneously assume that discursive reconstruction of the policy decision making of public institutions completely and adequately describes "policy" in whatever policy area is being studied. An argument made here by John Montgomery, Joel Migdal and others is that even "public policy" cannot be completely understood by limiting one's analysis to public actors, institutions and their decisions. Ideally, as political scientists, we should not need to remind ourselves that the power of public arenas is complexly limited by how private arenas perceive attempts to influence "personal" values and behavior. A clear challenge is raised here to define more clearly our conceptualization of "policy" itself—a task deserving more analytic attention.

This question of more adequately defining "policy" and the "public" and "private" aspects of policy links the first two dimensions of internal policy dynamics and policy context to the third and last dimension of "policy projection." As succinctly stated in the introduction, "The basic objective [of policy projection] is not forecasting; it is control." Control is said to mean "bringing as much rigor as possible into the process of forecasting despite the difficulty of forecasting social change." Three very different chapters offer thoughts on policy projection. Phillip Clark offers an analysis of how Australian immigration policies will become more flexible because of the interrelated nature of past policy decisions, current values within the Australian population, and future economic development needs. Gaither Bynum considers the impact of possible scientific and technological extensions of the human life span upon demographic patterns and future policy changes. Donald Warwick discusses the problems involved in conceptualizing and projecting the complex interactions of internal-national and external-transnational policy ("environment") inputs upon policy formulation, implementation and feedback.

Although separated into the other two volume sections on internal policy dynamics and policy context, the other chapters of the volume correctly attempt to integrate these two dimensions of analysis. John Montgomery analyzes hypothesized links between population policy and social change, discusses the problems inherent in using population policies as social experiments, and suggests that "better knowledge [of the links between policy and social change] will depend on the cooperative participation of [population] program planners and managers in observing policy experiments of the future." John Ickis examines the changing nature of organizational behavior and interest group coalition-building within the context of the personal, professional and political networks of El Salvadorean population politics since the early 1960s. David and Frances Korten demonstrate the importance of regional policy models in their analysis of the similarities of the Costa Rican and Nicaraguan family planning programs with the El Salvadorean program, despite major cultural, economic and political differences between these countries. Farrukh Iqbal examines evidence that land reform policy in Pakistan and elsewhere has an indirect short-run pronatal effect on population growth rates. In the long run, however, land reform will have an antinatal impact if instituted along with other rural reform policies effective in improving primary education, health and nutrition, and old age security. Myron Weiner discusses the impact of past and present ethnic prejudices, practices and privileges upon migration and employment in the state of Assam, India. Joel Migdal suggests that the unintended effects or "administrative countereffects, the indirect and contradictory effects of direct [migration] policies" in the Third World have occurred primarily because the capacities of centralized bureaucracies to carry through effectively with the implementation of their policies has not yet been developed adequately to replace "old political forces, . . . [who] have proved exceedingly resilient and adaptable." Nancy Lubin, in contrast, looks at some of the unintended problems which have resulted from excessive centralization of population policy in the Soviet Union. Unwilling to adapt their national Party pronatal population policy to regional cultural differences and unwilling to invest the necessary funds for more effective policies in the social need areas of better housing, day care, family allowances, and household convenience consumer goods, the Russians have become increasingly frustrated with continuing declines in their national population growth rate and undesired disproportional population increases among their Muslim "minorities" despite Party policy goals. Eleanor Glaessel-Brown argues that the seasonal labor migration patterns from Haiti to the Dominican Republic and the emigration of excess population from the Dominican Republic to the United States fills economic needs in all three nations while relieving pressures for any substantive socioeconomic change.

There are unfortunately some editing problems —a missing table in chapter 1 and a less than thorough index. Despite these minor inconveniences, the concepts broached in this volume should concern anyone interested in comparative development. Whether one's research focuses on one nation, a specialized aspect of demographic planning, or on broader themes of comparative social, economic or political analysis, it is becoming increasingly important to understand how and why population changes may or may not be effectively affected by national and transnational population policies.

ANELISSA LUCAS

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The Reluctant Party: Labour and the EEC, 1961-1975. By L. J. Robins. (Ormskirk, Great Britain: G.W. & A. Hesketh, 1979. Pp. xi + 155. £5.95, paper.

Throughout its four-score-year history the British Labour party has had in its ranks an important idealist faction dedicated to internationalism. However, for a long period this faction was dominated by middle-class leaders and the working-class representatives took little interest in their activities. Furthermore, to a significant degree, foreign policy issues generally were the concern of the political wing of the party rather than the industrial arm of the Labour movement. The trade union leaders and their representatives in the party were concerned with domestic bread-and-butter issues. The decision to have Great Britain join the European Common Market went against the traditional factional interest of the Labour party. What began as an apparent foreign policy issue was quickly perceived as being a major domestic concern and all parts of the party became preoccupied with it. The almost total interdependence of domestic and foreign policy points up the complexity of the issue.

This complexity caused serious difficulties for the Labour party and resulted in a confused unclear policy throughout the period in which the issue has been relevant. L. J. Robins appropriately entitles his book *The Reluctant Party* as he traces the debate about British EEC membership within the Labour party from the period of the decision of the Conservative government in 1961 to apply for membership until the referendum on the issue in June 1975.

A far too brief chapter on the decision-making process within the Labour party and a general discussion of Labour's foreign policy aims introduces the book. Robins then describes events and activities related to Labour and the EEC based on a chronological approach. There are several useful themes which recur throughout the book. These include the role of leadership in the party, the everpresent problem of party unity, party pressure groups and electoral considerations. Robins also writes of Labour orthodoxy in several chapters but he is less convincing on this topic because of the complicated nature of the issue and he does not develop a clear meaning of orthodoxy over time. He also endeavors with mixed success to discuss policy outcomes in terms of the themes mentioned above.

While offering little of originality for students of the British Labour party, the book is a useful review of the implications of party factionalism and the resulting intra-party strife in this federated, pluralistic reform party. Particularly noteworthy are the descriptions of the difficulties faced by the leaders of the Labour party in directing party policy and, at the same time, maintaining party unity. The author correctly sees the party as vacillating, inconsistent, and at times, lacking in any policy toward Western Europe. Leaders Gaitskell and Wilson epitomized the vacillation and even contradictory efforts in their quest to maintain unity. Also, at times their inconsistency was manifest because, rightly or wrongly, they perceived threats to their leadership positions on the issue of the Common Market. The weaknesses of Gaitskell as a leader in this area may surprise some readers.

Although the referendum on continuing membership in the EEC did not receive sufficient treatment or analysis as a precedent-setting political option, the author does perceive it as still another in the long series of instruments and compromises used to maintain party unity.

The book includes a full and useful bibliography. It is unfortunate that Robins confined himself to a narrow descriptive treatment of his subject. Comparisons with a few of the other major issues which have divided the Labour party in a concluding analytical chapter would have improved the book.

STEPHEN P. KOFF

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Nepal: Profile of a Himalayan Kingdom. By Leo E. Rose and John T. Scholz. (Boulder, Colo.: Westview Press, 1980. Pp. ix + 144. $16.50.)

This is the first in a series of short introductory volumes concerning the countries of Asia. Unfortunately, the limited format only allowed Leo Rose and John Scholz to scratch the surface of their subject and to raise many provocative issues which could not be explored in detail. Nevertheless, the result is a concise and lucid overview of the essential historical, cultural, economic, and political characteristics of Nepal.

There are several reigning monarchs in the world, although few of them actually rule. Nepal's ruler is one of the exceptions. The modern history of the country dates from the restoration of King Tribhuvan in 1950 after more than a century of oligarchical dictatorship under the Rana family, during which the form but not the substance of the monarchy was retained. The next 20 years were critical in the political and economic development of the country and Rose and Scholz have devoted one of their five chapters to it.

While Tribhuvan seemed to favor parliamentary democracy, his successor King Mahendra did not. He abolished political parties and issued a constitution which defined the monarchy as the central political institution. The authors point out that the absolute authority of the king is limited somewhat by traditional practices, especially from Hinduism. While King Birendra has further strengthened the monarchy, pressures for liberalization have increased which culminated in an outbreak of violence in early 1979. Birendra's response was to call for a referendum to let the people decide whether they wanted to continue with the existing system or to establish party government. The status quo was endorsed by a majority, but 45 percent favored the party government idea, indicating the need to increase participation in the political process.

The book is oriented toward political and public policy matters, which probably reflects the fact that Rose and Scholz are both political scientists. They note the importance of such social systems as the persistence of traditional landholding patterns and authority which derives from it. The government has followed a slow and gradualist approach in building centralized institutions to integrate local elites into a national economic and political process. As the authors point out, this may not be adequate in the long run, as reformers may seek dramatic action in the name of social justice. The stability afforded by traditional institutions may be destroyed before modern ones can mature.

Nepal, like other developing countries, confronts the frustrations of trying to achieve both political and economic modernization. Birendra seems to have heeded the advice of his Nepali and foreign advisors in emphasizing economic growth while placing political development in "cold storage." In other words, the king has been advised "to emulate the Shah of Iran" (p. 65).

Nepal's geographical position places it precariously because two giants, like "a root between two stones," a circumstance which governs its international posture. The chapter on foreign relations develops in some detail Nepal's recent international experience. Basically, Nepal can pursue isolation, attach itself to one of the great powers in the area, or try to follow a balanced policy. While the latter course has been followed since 1950, Nepal's welfare depends heavily on the good will of India. China, for its part, has chosen not to play a very active role in Nepal's affairs.

This welcome addition to the growing literature on Nepal is a measure of the considerable interest in that country. This interest is due in large part to the inherent social and physical beauty of the place, but even more so to the genuine hope that Nepal can grow and develop without the disruptions and disasters that other nations have experienced.

LOUIS D. HAYES

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Politics and Class in Zaire: Bureaucracy, Business and Beer in Lisala. By Michael G. Schatzberg. (New York: Holmes and Meier, 1980. Pp. xi + 228. $29.50.)

Such is the fluidity of African stratification systems that it takes unusual analytic skills to hold them still long enough to delineate their contours. A major difficulty with class analysis is precisely its tendency to encapsulate social phenomena within a set of categories which in many instances turn out to be too rigid and artificial to convey properly the sense of flux in the African social landscape. Here is a book which explicitly confronts the issue, and does so constructively and intelligibly.

Unlike so many works in the Marxist tradition, Michael Schatzberg's work combines a rather undogmatic view of class phenomena with a keen sensitivity to the dynamics of social stratification. Along with Michael Cohen's work on the Ivory Coast (*Urban Policy and Political Conflict in Africa: A Study of the Ivory Coast,* Chicago, 1975) and John S. Saul's recent collection of essays on East Africa (*The State and Revolution in Eastern Africa,* Monthly Review, 1979), *Politics and Class in Zaire* is one of the most successful examples of the class-analysis approach to contemporary Africa. It is also a telling critique of Zairian politics under Mobutu. Through a judicious mixture of descriptive chapters and analytic sketches, the author conveys as much in the way of solid information about the Mobutist state as it does about the significance of class as a major structural component of this aberrant and ever-intriguing polity.

Much of Schatzberg's data is drawn from field work conducted in the town of Lisala, in the Equateur Region, and in a sense his book is first and foremost a fascinating case study of local politics in a small urban community. Almost every page bears evidence of his first-hand acquaintance with local actors, his privileged access to municipal and regional archives, and his own astuteness as a participant observer. The result is a remarkably detailed and vivid account of the mechanisms which enter into the process of class formation at the local and regional levels. His peculiar sensitivity to the significance of local resources is nowhere more tellingly revealed than in his effort to establish the relevance of beer—yes, beer—to the political economy of Lisala, an effort which has at least the merit of novelty. Who else would have thought of "the case of the disappearing depot" as a way of illustrating the decision-making process in Zarie? What better way of investigating issues of ethnicity than from the vantage point of bar ownership, wholesale houses and distributive networks? Running out of Primus in Lisala evidently means more than running out of beer. . . .

Schatzberg's book is not merely a richly documented case study of grass-roots politics; the Lisala arena provides him with a point of entry into the wider setting of Zairian politics. The contextual realities of Zairian politics are seen as the central variable in the background of class formation: historically, the rise of a "politico-commercial bourgeoisie" is traceable to a carry-over into the post-independence period of a social structure that had already begun to crystallize during the colonial days; the growing interpenetration of the commercial and political sectors after Mobutu's seizure of power in 1965 gave further cohesion to the symbiosis of wealth and power, with the party serving as the main avenue of upward social mobility. Wealth alone does not automatically confer power; access to power, however, opens up new opportunities for increasing—and losing—one's wealth. The overall picture conveyed is one of far greater fluidity than has generally been conceded by students of Zairian politics: "Social classes are constantly changing in response to differing socio-political contexts" (p. 31); furthermore, perceptions of class identity vary in such a way as to make it possible for an individual "to belong to differing class alliances at the same time" (p. 31). By explicitly emphasizing the dynamics of class formation by reference to historical and contemporary events, Schatzberg offers an interpretation of Zairian society which deviates markedly from conventional Marxist analyses.

Through a careful and eclectic handling of statistics and other types of evidence, including interview data, Schatzberg presents a richly nuanced view of Zairian society which goes far beyond the usual Marxist clichés about the relationship of African modes of production to class formation. The question remains, however, as to whether a similarly careful investigation of other local arenas might not reveal important discontinuities and deviations from the model proposed. One also wonders whether one can safely "evacuate," as it were, the ethnic dimension from one's analysis to the extent that Schatzberg does. Finally, while considerable attention is paid to the "linkages" between Lisala and the capital city, much less is said about how the surrounding rural sectors link up with Lisala. Even so, *Politics and Class in Zaire* is likely to remain a minor classic in the social science literature on Mobutu's Zaire, a field where classics of any kind are rare.

RENÉ LEMARCHAND

University of Florida

The Rancheros of Pisaflores: The History of a Peasant Bourgeoisie in Twentieth-Century Mexico. By Frans J. Schryer. (Toronto: University of Toronto Press, 1980. Pp. xii + 210. $20.00.)

This short book is stimulating and informative, though flawed in several respects. It is a local history of a county in the Mexican state of Hidalgo, which the author knows well, having lived there for a total of over a year's time during four separate periods in the late 1960s and early 1970s. Frans Schryer traces the county's byzantine politics of factional alignments and realignments, *caudillismo* and sudden death, since the Revolution of 1910, and arrives at several conclusions of general significance for the study of Mexico: to wit, that the better-off peasants, the small landowners he calls "rancheros," dominate rural politics; that this relatively well-to-do class is the major beneficiary of government programs, such as land reform, designed to help poor peasants; that ideology is used only for tactical purposes; that the Revolution and the development of coffee culture have reduced the pyramidal quality of the rural class system by increasing the number of landowners at the top, reducing the number of landless day laborers at the bottom, and fostering the growth of a class of small property-holders in the middle; that those less successful economically are more likely to be politically active; that when economic times are bad the number of political homicides rises.

Schryer has thus painted a convincing picture of a rural Mexico where politics is just another form of self-serving entrepreneurship, and where one gets ahead by exploiting others. The landless day laborers at the bottom of the scale are passive and apathetic, for the most part. The law is manipulated to serve individual and factional purposes and can often be bent into shape or overridden by the right political contacts—but not always, and it does have an independent existence and needs to be taken account of in one's calculations.

The local story provides an interesting counterpoint to the national history of the period. A new president's shift of policy provides a local faction with an opportunity of reviving some long-term objective; national prosperity changes the terms on which local struggles are fought; building a highway benefits one region at the expense of another. Political violence declines locally over time, as it does nationally; national political imperatives become more important as the country is increasingly centralized.

This makes a plausible, though not always completely convincing, picture. It is difficult for the reader to judge its accuracy because Schryer's conclusions are in effect incorporated into his narrative. His general depiction of the case feels realistic and politically sophisticated, but much must be taken on faith. One is a little uneasy at this, since there are clearly minor errors in descriptions of national politics and in the citation of sources, ambiguities in the presentation of at least one table, and various gaps in the data—such as the author's apparent ignorance of the full names of some of the characters in his story, including one whom he interviewed. Part of the difficulty here may be due to the fact that Schryer is an anthropologist writing history and political analysis. His subject matter transcends anthropology, but he retains the subjectivism which the anthropologist's normal research situation makes inevitable. Good book, could have been better.

MARTIN C. NEEDLER

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Small Urban Centers in Rural Development in Africa. Edited by Aiden Southall. (Madison: University of Wisconsin African Studies Program, 1979. Pp. ii + 409. $8.25.)

This volume's roughly two dozen essays on small towns in Africa offer detailed vignettes on mechanisms and effects of change at a neglected level of analysis. Most of the essays are cross-disciplinary, touching in varying degrees on economic, social, cultural, and political structure and trends. Geographically, they span the continent, though Nigeria claims eight essays while only one deals with Southern Africa. The essays vary considerably in focus and quality, but collectively they offer valuable insights to at least three audiences: those interested in rural development, in spatial patterns of development and population redistribution policies, and in political change.

Small towns are often cited as potential catalysts or agents of rural development. Urban and regional planners and those concerned with patterns of migration often view revitalized and modernized small towns as important components in a broader strategy designed to divert migration from the largest cities and encourage less concentrated urbanization. Political analysts sometimes (probably too seldom) focus on small towns as the lowest level of the national administrative and political hierarchies, where rural people contact government agents and services, thereby shaping perceptions of government and—it is to be hoped—government responsiveness to rural needs.

This volume offers no simple generalizations on the role of small towns from any of these three perspectives. The main impression is of immense variation. Most of the towns described are in the 5,000-15,000 size range. Some trace their histories back for centuries, as pre-colonial centers of trade or administration. Others have mushroomed in the past few years, spontaneously responding to economic and political change or (in one pair of cases) deliberately created by government action. Some are thriving on new opportunities—the emergence of tourism on the coast of Kenya, or the establishing of a new university at Nsukka in Nigeria. Others stagnate (for example, a string of bus-stop hamlets off the Copperbelt in Rhodesia) or decline (a town across the lagoon from Lagos, formerly a trans-shipment point but now bypassed by improved transport links). The towns represent a range of economic functions—minor and major market centers, administrative seats, railroad nodes, centers of small industry (the tinsmiths of Awka, Nigeria) or in one case a large industry (Kima in Egypt). Their cultural and religious roles also differ markedly.

Perhaps most interesting to political scientists are the varying roles of these towns vis-à-vis both the broader national context and their own hinterlands. Americans may be particularly prone to idealize the potential political role of small towns as transmission points for rural preferences to reach higher state or national decision makers, and as centers where rural people can meaningfully participate in decisions regarding local development problems. Clearly, however, their capacity to play such roles depends on local history and social and political structure, and on the goals, organization and capacities of the larger political system. Just as small towns cannot promote rural development where national terms of trade for agriculture or international prices present powerful disincentives, so also they cannot represent rural interests nor act as centers for local participation if the systems of which they are parts do not seek or permit these functions. The cases suggest varied patterns: gross exploitation reflecting a profoundly corrupt national structure and partially perverse international links (Zaire); a case where local cleavages and competition, tied into a competitive national political context, produced substantial material benefits for the town and its locality (Ronald Cohen on Biu in Nigeria); another where town notables act as interpreters and buffers between national agents and local rural leaders, while being kept at a bit of a distance by the latter (Nicholas Hopkins on Kita in Mali).

In preparation for the research and conference leading up to this volume, hypotheses were formulated (reproduced in an appendix) attempting to identify mechanisms and conditions under which small towns could effectively promote rural development. The studies themselves, however, prompt Aiden Southall to suggest in his introduction that "Most small towns appear as the lowest rung of systems for the oppression and exploitation of rural peoples" (p. 2). Trends toward centralization and international penetration exacerbate this tendency. Yet it seems to me that many of the cases—including some which the authors present as examples of exploitation—demonstrate instead the mixed benefits and costs of change, and the immense range of local, as well as national and international factors bearing on who gains and who loses. Ronald Cohen's essay strikes a more balanced position, as does Crawford Young's on the changing role of African states and the extent to which dependency theory is useful in understanding the changes. To move beyond sweeping and probably misleading generalities, we need to focus more precisely on mechanisms and conditions under which small towns play various roles. These cases are most valuable for their (mainly implicit) suggestions on such mechanisms and conditions.

JOAN M. NELSON

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Poverty in the United Kingdom: A Survey of Household Resources and Standards of Living. By Peter Townsend. (Berkeley: University of California Press, 1979. Pp. 1216. $37.50, cloth; $15.95, paper.)

Poverty appears on America's agenda when we have depressions, urban riots, or an interested president, but currently it is out of fashion. In Britain, however, the state of the poor is a continuing concern for which each successive government is held answerable. This steadier focus can be credited in large measure to the well-publicized studies of Peter Townsend and the other diligent researchers associated with Richard Titmuss, a professor of social administration at the London School of Economics from 1951 until his death in 1973. In 1952, Peter Townsend attacked the prevailing assumption that the "welfare state" created in 1945-48 had eliminated poverty. Questioning the subsistence poverty standard on which optimism about postwar social progress was based, Townsend proposed a relativist concept as the more realistic measure. *Poverty in the United Kingdom* is the fulfillment of Townsend's work since 1952 in operationalizing that idea.

This mammoth volume reports on the most well thought-out, wide-ranging poverty survey ever done in Britain, which is probably to say anywhere. In 1968-69, a specially trained team administered a 40-page questionnaire to a national sample of 3,260 households, comprising over 10,000 individuals. But the book provides more than a poverty count in one nation. Townsend follows the ameliorist, fact-gathering tradition of Charles Booth, the Webbs, Seebohm Rowntree, and William Beveridge, while extending it; unlike his predecessors, he does not eschew grand social theory. The book's originality lies in its effort to use an empirical survey of the distribution of resources in British society in order to outline a general explanation of the structure of inequality in capitalist societies.

In theory, U.S. and U.K. welfare benefit levels are based on the cost of food and other necessities deemed requisite for physical efficiency and social functioning. In practice, governments adjust those levels in response to prices, wages, and changed opinion. Thus, what is actually a relative, politically determined standard is widely taken as an absolute, scientific standard. But Townsend rejects the simple option of updating such poverty lines and clarifying their ambiguous application. The underlying subsistence rationale is wrong. People are poor, Townsend argues, when they are excluded from the diets, household amenities, educational opportunities, work conditions, and physical environments which are "customary, or are at least widely encouraged or approved, in the societies to which they belong" (p. 31). Poverty could increase as a society got richer.

The survey tries to demonstrate how such a poverty standard derived from conventions can still be established independently of subjective attitudes felt by the poor or toward the poor. The distribution of a wide range of resources enjoyed by the general population was determined, and a rough index of a national "style of living" identified. Income strata were then ranked on each and all resource dimensions according to a "deprivation index." Townsend hypothesizes that when income is severely lacking, participation in society's normal activities declines precipitously. This dropping-off point should serve as the poverty line.

Not surprisingly, the survey found considerably more in poverty—12½ of the U.K.'s 55½ million —when using the deprivation standard than when the government's poverty line was applied. There was evidence in some household categories of an income threshold below which social isolation increased rapidly, although the data were judged inconclusive. Even using the state's own standard, however, Townsend found more poor people than other studies of that period would have predicted.

Of great interest is Townsend's modified class explanation of poverty. Separate chapters describe each of the demographic categories constituting most of the relatively deprived: unemployed, low-paid, and older workers; the disabled; handicapped children; one-parent families; the elderly. Its concentration within these "social minorities" leads Townsend to reject individualistic determinants of poverty, but the poor's dispersion across different ethnic groups and communities makes subculture and area theories inadequate also. Townsend rejects radical and dual market theories, moreover, because they focus primarily on the effects of the labor market and wage system, and neglect the nonworking population.

It is not simply relations to the productive process that determine the chances of poverty, Townsend suggests, but also the influences of hierarchical private organizations, state bureaucracies, professions, associations, and clientele interests in helping "to reproduce but also to modify" (p. 89) the class structure. A complete explanation of modern deprivation must include all institutions that set the standards for the style of living widely expected, and then govern access to the work roles and resources required to meet these standards. By creating standards and allocating privileges and status, these institutions and their rules differentiate the population, consigning some to roles of dependency. Policy constitutes society.

The many different topics and methods found throughout the book's 27 chapters and 13 appendices hold lots more for various sociological, methodological, and policy specialties. Two chapters on the problems in Britain's centralized public assistance system and its local means-tested services, for example, could be read profitably by reformers looking to the federalization of all American welfare. All readers will be moved by the chapter of case narrative revealing the impacts of poverty on specific families.

Townsend and Britain's poverty action groups have influenced governments not because there has been wide acceptance of the idea of relative poverty or its underlying causal theory. They have had influence because they have documented unmet needs under existing welfare programs, using the government's own poverty criteria. The Titmuss group has largely employed the traditional Fabian strategy of publicizing their studies in the London press and engaging in top-level dialogue with government ministers. This approach is sensible, given the obvious difficulties of directly mobilizing statistical categories such as "households with borderline disabled under 65." Any ambitions to mobilize wide political support behind an anti-deprivation theme would also have to confront another brutal fact. As the survey itself revealed, most people defined poverty not in the relative terms Townsend does, but in the way the government does, where the figures are less staggering.

MICHAEL S. LUND

University of Maryland, Baltimore County

The Soviet Union: Looking to the 1980s. Edited by Robert Wesson. (Millwood, N.Y.: KTO Press, 1980. Pp. ix + 288. $25.00.)

This volume, with 16 contributors, takes a look at Soviet domestic and foreign policy with an eye on the future. Most of the authors are well known and highly respected, which on the surface augurs well for an up-to-date, provocative treatment of Soviet politics. The quality of the contributions is disappointing, however. Some chapters are excellent; others are only fair. The more distinguished chapters are those written by Jan Triska ("Soviet-East European Relations"), Joseph Berliner ("Economic Prospects"), Gertrude Schroeder ("Prospects for the Consumer"), Teresa Rakowska-Harmstone ("The Nationalities Question"), and Frederick Barghoorn ("Political Dissent").

Although the book deals with foreign as well as domestic affairs, it is the question of domestic change that ties the contributions, or at least most of them, together. In the words of the editor, Robert Wesson, is the Soviet Union "approaching major change"? (p. vii). The answer that Wesson gives for his contributors is affirmative: "It is hence bold to entertain the thought that there may now be severer strains in prospect, possibly entailing instability of the long stable system. *We* [italics added] are tempted, however, to do so, viewing the visible trends of recent years" (p. 264). This conclusion varies, in fact, from the assessment made by most of the contributors. Four pay little if any attention to the central question, and eight conclude that major change is not in the offing. Only four contributors—Berliner, Wesson, Rakowska-Harmstone, and Andrei Amalrik—see significant change on the horizon, and even here there is a difference. Berliner suggests that the post-succession leadership may seriously entertain a within-system organizational reform, a modern version of the New Economic Policy (pp. 105, 198), whereas his three colleagues predict system change, with Amalrik going so far as to depict the replacement of Soviet "totalitarianism" (p. 258) with "Russian fascism" (p. 255).

The volume offers little that is new. It does, however, assist the reader in recalling the highlights of recent Soviet actions, both domestic and foreign, and it contains projections into the 1980s that for the most part are well argued. The book is a "must" for large libraries, but the return on a capital investment of $25 would for most individuals be marginal.

JOHN H. HODGSON

Syracuse University

Political Culture and Soviet Politics. By Stephen White. (New York: St. Martin's Press, 1980. Pp. xi + 234. $22.50.)

Stephen White seeks to provide a comprehensive view of the evolution of Soviet political culture. Despite the inherent difficulties and some shortcomings in the use of available evidence, the result is a significant contribution to the Anglo-American study of the Soviet system. The major strengths of the book are the clarity of the presentation, the effective effort to relate the Russian experience to the contemporary Soviet situation, the thorough use of other relevant work, and the relating of the political culture theme to the heterogeneity of the Soviet population.

White demonstrates a superior ability to organize a complex body of findings and to minimize dependence on jargon. Such efforts result in a rather concise volume that, nevertheless, covers much ground.

The historical dimension takes up approximately one-third of the book. Although this section is the least valuable to the specialist, the major sociological and political aspects of the tsarist heritage are effectively linked to current Soviet patterns.

The diverse sources used include memoirs, sociological studies published in the Soviet Union, and interviews of Soviet emigres during various eras. The heart of the book is a direct comparison of the findings of Alex Inkeles and Raymond Bauer (*The Soviet Citizen*, Harvard, 1959) and recent surveys of Soviet Jews and other emigres. The results of a burgeoning Soviet survey research literature are also stressed.

Several significant shortcomings relating to the use of evidence need be noted. First, White tends to use surveys of Soviet Jews who had emigrated to the United States and Israel as if this group represented a cross section of the Soviet population rather than a distinctive subgroup. Such studies, including several of White's own, are consistently identified as studies conducted "with Soviet emigres in Israel and the USA" (see p. 96). The term "Jew" does not appear in the book's index and is rarely used in the text. It is doubtful that other researchers would accept his universalization of the Soviet Jewish experience.

Another problem pertains to the use of sociopolitical surveys developed by official Soviet agencies. White makes some efforts to distinguish between credible and dubious published findings. Yet the reader is left wondering about the impacts of varying procedures used by the Soviet researchers as well as presumed official decisions to alter or withhold unwelcome findings. For example, it is noted that not all official surveys are developed with anonymous respondents.

Not surprisingly, White's findings seem strongest when he stays closest to his best sources of data and the specific elements of political culture. An example of this is his conclusion, "There is no evidence of a movement towards more participation and consultation norms, in these respects at least, over a period of major and far-reaching social and economic change" (p. 175).

Yet his broader generalizations do not always seem to be based on sufficient evidence. Mass support for the Soviet "system" is said to coexist with limited commitments to the regime and to Marxist-Leninist values. This conclusion may well be essentially true. Yet additional unassailable evidence is needed to sell this centrist perspective to skeptical readers.

RICHARD SIEGEL

University of Nevada, Reno

International Politics

Communism in Africa. Edited by David E. Albright. (Bloomington: Indiana University Press, 1980. Pp. viii + 277. $12.95.)

This first-rate compendium consists of eight essays, three of which are revised, enlarged, and updated versions of articles published originally in *Problems of Communism*. All contributors are experts who provide valuable information and insights concerning the impact of communism on Africa.

To ensure a degree of coherence, David Albright uses the dynamics of Soviet-African relations as an organizing framework. Such a focus emphasizes the role of the USSR, but it also leaves room for other communist powers' involvement and allows exploration of the roles that noncommunist elements have played in communist successes and failures. Undoubtedly, the Soviet superpower can and does interact with other communist and non-communist states with unmatched intensity. The thesis of the book is that the primary significance of the non-Soviet activities lies in their impact on the enterprises of the USSR in Africa.

In keeping with the focus of the book, each chapter addresses itself to a particular issue bearing upon the dynamics of Soviet-African relations. Colin Legum's assessment of changes over time in the attitudes of African governments and movements toward Moscow seriously underestimates the positions of power and influence the Soviets have gained in the last few years. He fails to take into account the fact that Soviet-African relations have always been determined by Soviet decisions based on the degree of Soviet interests involved. This view is expressed clearly in Albright's study where he concludes that Soviet policy toward Africa depends on "the relative priority that (Moscow) attaches to the continent . . . and the opportunities that Africa presents" (p. 64). T. Wilson's essay emphasizes the effects of the long-standing Russian associations with Africa on the Soviets' recent policies toward the continent. Jiri Valenta's examination of Soviet decision making is based on the intervention in Angola, and supports the view that events, perceived as opportunities, have shaped Soviet behavior more than their behavior has shaped events. Marina Ottaway focuses on the interaction of the Ethiopian and Mozambican regimes with Moscow and the relationship between Marxist-Leninist theory and practice in those countries. Edward Gonzales' study of Cuba's role in Africa on Soviet Africa policy points to the most serious weakness of the volume. There is no discussion of the important role of the German Democratic Republic in Moscow's Africa policy. To ignore Cuba's German communist counterpart in the Soviet penetration of Africa is a serious mistake, especially in view of the overwhelming evidence that has been discovered.

George T. Yu's contribution deals with aspects of Sino-Soviet rivalry in Africa and concludes that Chinese communist influence on Soviet behavior will probably "remain quite restricted in the foreseeable future" (p. 188). W. S. Thompson explores "The African-American Nexus in Soviet Strategy," and draws attention to Soviet concern with the minerals in Africa and the sea lanes around the continent. By being able to cut off shipments of crucial minerals to the West and endangering the flow of oil from the Middle East, the Soviets have from a strategic point of view outflanked the Middle East, which is critical to Western survival. In the last chapter the editor discusses the prevailing instability in Africa and concludes that "the atmosphere on the continent will in all probability be favorable to increased Communist involvement" (p. 233). This, then, is the general impression created by this timely and important contribution to the literature on Africa's role in international relations. Soviet involvement can be expected to continue, and it will probably grow in the future. The implications for American foreign policy in the 1980s are unmistakable.

ROBERT M. BIGLER

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Conflict and Intervention in the Third World. Edited by Mohammed Ayoob. (New York: St. Martin's Press, 1980. Pp. xi + 261. $27.50.)

The Third World has been the principal arena of violent conflict, and the only scene of interstate military engagements, during the past several decades. Largely as a product of the Third World's colonial heritage but partly as a result of contradictions whose origins are independent of the legacy of external dependence, conflicts of interest have outstripped the social and institutional bases for their peaceful resolution. Moreover, it is very likely that the human and material costs of these antagonisms will continue to be heavy in the foreseeable future. Therefore, this book addresses an important topic; whether it does so entirely successfully is debatable.

More often than not, the edited volume contains a collection of essays of uneven quality and of far-flung perspectives. This is not so here and the uniformity of style and of discourse (though not necessarily of political opinion) is truly unusual—no doubt explained by the fact that, of the eight authors, five are associated with the Australian National University (and only one is affiliated with a non-Australian institution). The first and last essays (by T. B. Millar and Mahommed Ayoob, respectively) are predominantly theoretical, while the remaining seven are case studies of the principal modern instances of Third World conflicts, from Cyprus to Indo-China. On the whole, the theoretical pieces are rather loosely speculative and the other contributions, while factually comprehensive, are analytically modest. The main purpose of a case study, of course, is to present an interesting constellation of facts relevant to a specific social entity: but, with fully *seven* such essays, it should have been possible to draw analytically solid and empirically well-grounded generalizations. This, in my opinion, has not been done. The essay which carries the final burden of attempting to achieve this is the editor's concluding piece. What we are presented with is a broad-stroke indictment of the major powers as cynical fomenters of Third-World antagonisms, but the attempt to trace the various causal mechanisms which are involved is startlingly skimpy and gives virtually no recognition to possible rival hypotheses (some of which are suggested by the case studies themselves). It is not that Northern responsibility can be plausibly denied, it is merely that its nature is not nearly as simple as Ayoob seems to feel; in short, the valuable empirical material mustered in the case studies has not been done adequate theoretical justice.

Perhaps the most interesting essay is D. J. Goldworthy's piece on South Africa, in which the author convincingly traces the contradictions of a system which has achieved substantial economic growth by the sort of economic repression which deprives it of the internal talent and markets needed to perpetuate this growth. The international implications of this contradiction are particularly well described.

In conclusion, the book contains a wealth of relevant factual information on some of the politically most volatile regions of the world; it is interesting but not, however, as useful as it could have been were it an empirically more rigorous and conceptually more sophisticated volume.

MIROSLAV NINCIC

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Office Without Power: Secretary-General Sir Eric Drummond 1919-1933. By James Barros. (New York: Oxford University Press, 1979. Pp. xii + 423. $41.00.)

James Barros' *Office Without Power* is the first book to appear on Sir Eric Drummond's tenure as the first secretary-general of the League of Nations. Those who write textbooks on international organization frequently use Drummond as a prototype of the discreet, behind-the-scenes administrator and facilitator of international agreements who eschews public exposure and publicity either for his own positions on issues or for the ideals of the organization's charter.

Drummond adopted this role because he was aware that the great powers in his day would not and could not rely upon the newly created League to solve major international issues. Thus, he sought to be perceived as a neutral international officer who would merit the confidence of *every* League member and who would always be ready to help in international conflicts to whatever extent possible within the principles of the Covenant (p. 263). While the League could never dictate the settlement of disputes, especially those involving major powers, Drummond thought that the organization could provide "face shaving machinery" in conflicts involving prestige and serve as "an enormous safety valve" for the volatile boundary and minority disputes that plagued the first decade of the interwar period (p. 41). Any more grandiose role for the organization would come slowly, he thought.

The great contribution of Barros' book is his absolutely thorough and detailed description of Drummond's behavior within this role conception. Drummond's quiet activity is revealed in detail for every major conflict during the first 13 years of the League's history. Barros' study also makes it clear that while Drummond worked quietly and discreetly he was in no way nonpolitical. He unceasingly gave advice and made suggestions on both tactical and substantive policy issues to great and small powers alike.

Even in his own terms, however, Drummond was not completely successful. He never attained the confidence among such revisionist powers as Germany and Italy that he was able to gain from Britain, the Scandinavian states and the members of the Little Entente. Also, because he had decided to work with the Great Powers, especially Britain, Drummond's efforts were always heavily constrained by the policies of these states. And, of course, there were cases where persuasion simply failed, in Corfu and Manchuria most notably. Drummond, however, was never under any illusions about the limits of his own efforts or those of the League. His tenure was one of many frustrations and disappointments, all of which he felt deeply.

The organization of *Office Without Power* is largely chronological and geographical. After discussing the development of the office of the secretary-general, Drummond's conceptions of international politics and international organization, and his behavior in 1919 and 1920, Barros describes Drummond's activity in separate chapters on the German question, Latin America, Italy and Manchuria. Because of this largely chronological framework, others will have to reorganize Barros' material to answer more analytical questions. For example, if one wants to know what kinds of influence attempts Drummond made to what kinds of powers in what kinds of disputes and how successful these attempts were, one will have to rework Barros' data. But, happily, the data is now there—data that was unavailable or inaccessible before. And for this we have Barros to thank.

Clearly, *Office Without Power* will be the basic reference work on Eric Drummond's tenure in office for a long time to come.

STANLEY J. MICHALAK, JR.

Franklin and Marshall College

Terrorism and Global Security: The Nuclear Threat. By Louis René Beres. (Boulder, Colo.: Westview Press, 1980. Pp. xii + 161. $18.50.)

Louis René Beres is at his best when he describes contemporary political terrorist movements. He correctly states that such groups are engaged in total war against prevailing social, political and economic values and structures. Terrorists are thoroughly committed to the righteousness of their cause and stand ready to sacrifice their lives and the lives of others for the movement.

Nuclear terrorists are different because their actions may, when compared to those of ordinary terrorists, potentially cause a greater degree of damage and suffering. They can steal nuclear weapons, fabricate their own nuclear explosives, or sabotage nuclear power reactors. The nuclear genie has been released, there are no more secrets. Terrorists could intimidate governments by threats, wreak havoc if their demands are not met, and provoke regional and global nuclear wars. In addition, Beres correctly points out the need to restrict the proliferation of nuclear weapons. Clearly, the non-proliferation treaty and fuel controls are not adequate. However, he offers no concrete method of preventing the spread of nuclear explosives.

Beres should have limited his focus to an analysis of terrorism and its nuclear implications. Instead, he offers some extraordinarily superficial views on military strategy and world politics, and then suggests that the world may soon end if the present system of relationships among states is not altered.

Like many of the nuclear pacificsts of the 1960s, Beres' vision of the future is cataclysmic. Nuclear weapons and terrorists, however, are not the root causes of disaster. Realpolitik and the politics of national defense and interest are the true culprits. States must discard such narrow concerns and become conscious of their "planetary identity." There is, according to the author, a desperate need for a new world order to promote cooperation rather than competition. The current system, he believes, cannot last out this century. Beres provides several probable models for a new order, none of which is adequately explained nor explored. Nor, and this is most significant, does he indicate how the possibility of nuclear terrorism will be diminished in a system wherein national interests are redefined and "planetization" is promoted. He assumes, without adequate justification, that a new world order will produce circumstances wherein terrorists will become less dangerous. Beres does not convincingly demonstrate why realism is so awful nor does he give the reader any reason to believe that "planetization" is a viable alternative to the current configuration of world politics.

Unfortunately, the scope of this work does not live up to the promise of the title. Beres' superficial attempt to analyze and reform international politics from the perspectives of nuclear terrorism will impress few academicians and probably no political leaders.

ANDREW C. TUTTLE

University of Nevada, Las Vegas

Deviance, Terrorism and War: The Process of Solving Unsolved Social and Political Problems. By John Burton. (New York: St. Martin's Press, 1979. Pp. xv + 240. $22.50.)

This is a wise and convention-shaking book, nudging us toward a less coercive political science. The thesis is that a shift is taking place in world politics from a "coercive paradigm" to a "needs paradigm." The shift is embodied in a participatory process of "problem solving" that replaces customary forms of "puzzle solving." The motivation for the shift is failure of coercive institutions to satisfy human needs. The task of political science is to interpret the shift for the benefit of all involved so that the transition can be quicker and less destructive. Political science, however, does not prescribe solutions to problems; it assists processes by which parties in conflict identify and accept them.

The "navigation point" to guide political action and analysis is the concept of "universal human needs" that Burton, after Paul Sites in *Control, the Basis of Social Order* (Kennikat, 1973), defines to include needs for consistency in response, stimulation, security, recognition, distributive justice, rationality, meaning, and control. Burton adds the need for "role defence" (protection of acquired need satisfactions) and also stresses need for "identity." Criminals, terrorists, and war leaders are all assumed to share the same needs as their victims.

This line of thought leads Burton to assert a view for which this book is most likely to be remembered. Others have said it in different ways, but perhaps no political scientist has stated so boldly the thesis that when violence occurs—including crime, terrorism, and war—the perpetrators are not to be regarded as "deviant." Rather their behavior is to be taken as evidence that the dominant institutions and structures of a local community, nation, or international polity are "deviant" to the extent that they have failed to satisfy needs of persons affected by them. Therefore the grand task of political science is to help parties in conflict understand this deviance and to work together toward common need satisfactions. Burton rejects the conventional zero-sum objection to this approach and advocates instead positive-sum outcomes in which all gain something.

Constant return to human needs as criteria for analysis and action enables Burton to question assumptions that have led to theoretical, disciplinary, and administrative compartmentalization. For example, Burton questions the reasonableness of the assumption that the problem of communal conflict in Northern Ireland ought to be solved by the British Home Office while that in Cyprus ought to be solved by the Foreign Office.

Overall, Burton's book suggests an incipient paradigm for a political science of human needs. One of the advantages of such a general framework is that it assists identification of opportunities for internal development as well as for external assistance.

Five are apparent. First, the emerging paradigm requires basic and applied theoretical development. Although Burton does not consider the problem of combining consciousness-raising hermeneutics and positivist social science with the clarity of Robert L. Heilbroner's later discussion in *Marxism: For and Against* (Norton, 1980), he appreciates the need for testable and falsifiable propositions. They are still needed, offering a challenge to basic and applied theoreticians. Second, further development of the paradigm should take account of the unmentioned Lasswell-McDougal framework of policy science—and vice versa. This is not to prejudge superiority but to recognize fertile affinity. Third, although Burton, like Lasswell, does not center attention upon the concept of "leadership" in either practical politics or in applied social science, his work is rich with leadership-related ideas scattered in discussions of "power" and "authority" as well as in inadequately indexed references to "leadership" itself (e.g., references to third party intervention as "leadership behaviour," p. 219). More explicit recent political science interest in the concept of leadership can easily be brought to bear, including needed rediscovery of Martin Tarcher's neglected but potentially seminal view of leadership as applied social science in *Leadership and the Power of Ideas* (Harper, 1966). Fourth, Burton's clear commitment to lessened violence needs to be advanced to embrace the social science literature on nonviolent behavior that is beginning to appear, pioneered especially by sociologists such as Gene Sharp, George Lakey, and Severyn T. Bruyn. Fifth, the moral commitments of Burton's human needs scientist and Lasswell's policy scientist both need to be made much more explicit as a scientific problem. It is a myth that we can help others solve life and death problems without controlled awareness of how our own values and needs, including our silences, affect the process.

By questioning assumptions and suggesting new ways out of old difficulties, implying that political science itself may be "deviant" in its capacity to serve universal human needs, Burton's book helps to envision new steps forward in disciplinary advance. It deserves a wide reading among political and other social scientists.

GLENN D. PAIGE

University of Hawaii

The Politics of Grandeur: Ideological Aspects of de Gaulle's Foreign Policy. By Philip G. Cerny. (New York: Cambridge University Press, 1980. Pp. xi + 319. $29.50.)

Why did Charles de Gaulle strive in such a dramatic manner "to upset the flower pot" of international relations during his years as president of the Fifth French Republic? This question, which has never ceased to fascinate students of French and international politics, is the central concern of *The Politics of Grandeur*. Philip Cerny answers the question in an original fashion, not through the introduction of new data but rather through the development of "a new synthesis, based upon a new interpretation of the significance of Gaullist policy" (pp. 2-3).

The first part of Cerny's book focuses on the "ideological roots and purposes of de Gaulle's foreign policy." Four chapters of part 1 deal with the personal, philosophical and structural bases of Gaullist policy; the fifth chapter attempts "to chart a course through the complex waters of generalization" (p. 98), analyzing de Gaulle's policy as an example of efforts to achieve national integration or authority legitimation through foreign policy leadership. The second part examines de Gaulle's "grand design and foreign policy praxis." After briefly describing the structure and development of the grand design, Cerny devotes three chapters to a study of Gaullist treatment of "the Atlantic question," and then concludes with an assessment of the impact of de Gaulle's foreign policy on domestic politics in France.

Cerny's central thesis is that "the purpose of de Gaulle's foreign policy was not the attainment of glory and power for France for its own sake," but rather the creation of "a new and more profound sense of national consciousness" which would transcend traditional French political divisions and thus allow for the maintenance of domestic stability and the legitimation of the Fifth Republic's institutions (p. 4). As Cerny acknowledges, he is by no means the first analyst to contend that "the Gaullist notion of *grandeur* was a significant symbolic force for the creation of domestic stability and a sense of national identity" (p. 6). However, his essay is the first attempt to develop at length the argument that *grandeur* was pursued *primarily* for such domestic purposes. De Gaulle's "international revisionism was a function of his aspirations for the cultural and social integration and development of the French people by means of a common purpose, and not the other way around" (p. 6). Within the analytical framework presented by Cerny, "the acid test" of Gaullist foreign policy "was domestic impact" (p. 90). Viewed from this perspective, de Gaulle's ambitious foreign policy initiatives deserve much higher marks than they are usually accorded. Cerny concludes that de Gaulle's "symbolic strategy" (p. 259) was essentially a success, fulfilling "a vital function of normative integration" (p. 268) by building a consensus not merely on foreign policy but also "quite clearly on the acceptability of the political system as a whole" (p. 263).

As compelling as Cerny's account may seem, it must be said to suffer from two major problems. First, his portrayal of de Gaulle simply cannot be substantiated on the basis of the available evidence. Cerny asks that we "read between the lines of Gaullist rhetoric" (p. 272), but he provides no sufficient reason to ignore the lines which indicate that *grandeur* was valued at least as much for international as for domestic purposes. Second, his images of a de Gaulle chiefly concerned with domestic stability and of a French public successfully integrated into the new system of authority are both difficult to square with the events of May 1968. Was not the May revolt caused at least in part by what Stanley Hoffmann has termed the "subordination of domestic politics to the primacy of international affairs and the requirements of status"? (p. 89). Cerny implicitly rejects this notion, but he provides no alternative explanation and indeed virtually ignores the subject of May 1968 and its aftermath.

Despite its flaws, Cerny's book is definitely a worthwhile contribution to the literature on French foreign policy. Perhaps its greatest virtue is that it shows clearly how and why the "*gaullien* style of politics" in international affairs continues to be "one of the most compelling options of a President of the Republic" (p. 269). Readers of *The Politics of Grandeur* will understand how Giscard d'Estaing's test of the neutron bomb represents not only the progress of French technology, but also the persistent domestic appeal of the foreign policy launched by de Gaulle.

JOHN T. S. KEELER

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The New International Economic Order: A U.S. Response. Edited by David B. H. Denoon. (New York: New York University Press, 1979. Pp. xvi + 346. $20.00.)

In 1976, concerned by the turmoil created in the wake of increased oil prices and challenges from advocates of a New International Economic Order, the United Nations Association of the United States (UNA-USA) gathered together a group of distinguished Americans for the avowed purpose of evaluating claims made by the LDCs and to make policy recommendations on particularly promising responses. This book is the technical report of the scholars commissioned to carry out these studies. The book is divided into three parts (trade, commodity policy, and capital flows) which correspond to the three panels set up by the UNA-USA Economic Policy Council. The Economic Policy Council, listed in an appendix, reads like a "Who's Who" of representatives of U.S. interest groups. And therein lies a major problem with the book.

The Economic Policy Council decided that the project would have two major foci: proposals for restructuring the international economic system which yield *joint* gains for developed countries and LDCs and a package of recommendations which would respond to these challenges but yet be sufficiently *pragmatic* to be of use to the U.S. Congress and executive branch. Since Congress and the executive branch have been notoriously hesitant to confront many of these ticklish issues, the project faced severe limitations from the beginning. The result, written almost exclusively by economists, is heavy on models and light on worthwhile suggestions. The political aspects of a very important set of international political issues are almost entirely ignored.

The book is a collection of technical essays and it is difficult to characterize the result as anything other than mixed. It wanders over a great deal of territory and hits upon a number of topics, but gives no good reasons for the selection of some and elimination of others. And, aside from a very fine introduction by the editor, there is very little that integrates the essays.

The best section of the effort deals with commodity policy. Willard Cochrane does an excellent and complete job of assessing the international grain market and analyzes proposals for alleviating market fluctuations and potential future shortages through creation of a world grain reserve. Among the ideas found in his four pages of excellent policy suggestions is a scheme that would commit importers and exporters of grains to hold grain reserves of at least 70 million tons; thus Cochrane walks a very delicate line between those who advocate no organized reserves and those who argue for an international organization to do the same thing.

The chapters by Smith, who deals with the tin market, and by Woods, who concentrates on the world aluminum/bauxite market, are likewise complete and are highly recommended reading for those interested in international commodity markets. Smith argues that tin is of little economic significance to the United States; the importance of U.S. membership in the Fifth Tin Agreement is more symbolic and less economic than might be supposed. He urges that the U.S. play a constructive role in the organization since it is a pretty much cost-free method of demonstrating willingness to collaborate with exporters of commodities. The selection by Woods is more notable for the completeness of the analysis than any suggestions that he makes; it is essential reading for anyone interested in the aluminum market.

The rest of the chapters in the book are somewhat disappointing because, in general, they say nothing very original and have become outdated because of better work more recently published.

The section on trade is unexciting and the relationship of large sections of these chapters to demands of advocates of a New International Economic Order is never made clear. Thus the authors tell us that computer models indicate that multilateral tariff reductions reduce prices in all countries and that the overall effects on employment are quite modest. But the effects vary among industries. In the United States, for example, multilateral tariff reductions hurt iron and steel, textiles and apparel, while footwear, electrical goods and transport would be helped by such a move. This, of course, creates political problems which extend beyond the scope of the book. The section on capital flows concentrates on the debt problems of less developed countries in light of the first massive increase in petroleum prices (the impact of the 1979 increases is not covered). The authors attempt to remain analytical, and consequently say very little that is new or original about the debt problem. Little matter, since this section has been superseded by several more complete books on the subject.

In summary, this uneven book unwittingly illustrates the general paralysis of United States policy in response to demands for a New International Economic Order. The council's mandate seems to have been to look at these issues but not to suggest anything that would offend anyone. And as is true of U.S. policy, the result is timidity. There is very little that is new or creative either in analyses or policy suggestions, and again, like U.S. policy, the result is intellectual bankruptcy. As a U.S. response to demands for a New International Economic Order, the book is lacking. If a vote for creativity were taken at this point, the NIEO would certainly win round one.

DENNIS C. PIRAGES

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The Middle Eastern States and the Law of the Sea. By Ali A. El-Hakim. (Syracuse, N.Y.: Syracuse University Press, 1980. Pp. xv + 293. $27.00.)

The Middle East has long been a turbulent region, seemingly lurching from one crisis to another and demanding our almost constant attention. The region has not, however, produced comparable drama in marine affairs except for sporadic threatened and actual interruptions of navigation. Nevertheless, the importance of its petroleum, waterways, fisheries and votes at the Third United Nations Conference on the Law of the Sea (UNCLOS III) make necessary a detailed and authoritative analysis of its role in the development of the Law of the Sea. This book is an important contribution toward such an analysis.

The book is organized into two parts. The first covers general practice and policies with sections devoted to existing claims to offshore jurisdiction and to trends as reflected at UNCLOS III, including discussions of positions taken by states of the area at the conference and of recommendations of the Arab League. Part 2 covers Specific Regional Issues: the "legal problems of offshore boundaries in the Arabian Gulf," "legal status of the Gulf of Aqaba and the Straits of Tiran," and "legal aspects of the Red Sea hot brines and other metalliferous mud deposits." The first two of these topics get very detailed treatment, the third much less, but still more than can readily be found elsewhere. The final paragraph of the four-page general conclusions is worth quoting (p. 192):

> In conclusion, while there is much to be said for regional cooperation and agreements, it is to be hoped that a universally acceptable system of cooperation will supersede those regional arrangements. It can be seen from the foregoing chapters that, in so far as a coherent Middle Eastern point of view emerges, there is great potential for reconciling it with that of the wider international community.

This commendable universalist view is contradicted, however, by the author's parochial view of anything regarding Israel.

The book contains seven appendices, mostly texts of relevant national legislation; a helpful list of abbreviations; extensive and most useful notes; a selected but still impressive bibliography and a good index. There are 17 simple maps, many of which lack legends to explain the symbols used. The writing is generally smooth and careful, a good blend of law, economics, politics and geography, with few typographical errors and a minimum of jargon. It does have a serious weakness, however.

El-Hakim does not include Israel among "the Middle Eastern States" and scarcely mentions Israel except in connection with the Gulf of Aqaba and the Straits of Tiran, to which he devotes nearly 20 percent of his text, even though Israel has played a significant role at UNCLOS III and is a major maritime power in the region. His agreement with the Arab position on the Aqaba-Tiran issue is understandable, but not his use of the name "Palestine" where "Israel" should be on two maps. On one of them (p. 134) he even shows a number of tiny settlements in Sinai and Saudi Arabia but only a blank space where the Israeli port city of Eilat should be!

In his introduction, El-Hakim stresses that his book is not comprehensive, so omission of certain topics is not necessarily a fault. While the book is not nearly as complete as the loose-leaf series published by Oceana on Latin America, Eastern Europe, Western Europe and North America, and East Asia and the Pacific Islands and the Law of the Sea, it does nevertheless partially fill a gap left by this series and, despite the weakness noted above, is a most welcome addition to the literature.

MARTIN IRA GLASSNER

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America and the Third World: Revolution and Intervention. By John L. S. Girling. (Boston: Routledge & Kegan Paul, 1980. Pp. xii + 276. $25.00.)

John L. S. Girling of the Australian National University, earlier a member of the British Foreign Office research staff, has written a difficult but important and engrossing book. It is of a genre of which we need more: thorough, painstaking and penetrating analysis—not the mere recounting or judging of the purposes, means and results of U.S. foreign policies. At the same time, Girling's own views and conclusions illuminate the book.

The first 100 pages of this succinct work coolly scrutinize the motivations and structural contexts of Third-World revolutionary movements, emphasizing throughout the four "pivotal" strata Girling identifies in "underdeveloped" social structures: "peasant dependence, radical rootlessness, military caste loyalties and either bourgeois absorption in or management of the system" (p. 100). Girling finds neither the pluralistic, patron-client or neo-Marxist (dependency) models satisfactory as "global" theories of revolution, although the last of these, without being necessarily predictively powerful, probably best explains Third-World revolutionary or pre-revolutionary conditions. Nor does he believe that a single global theory of revolution is likely to emerge: the principal intellectual barrier is the incapacity to reduce voluntarism (the factors of will, personality and chance) to order within the limits of indeterminacy. This part of the book is thoroughly documented by resort to almost all the existing secondary source literature. Followers of Tilly, Gurr, Huntington, Johnson and others, including those of dependencia writers like Bodenheimer, will find here a stimulating critique of their thoughts.

In the second 120 pages, Girling turns strongly condemnatory as he mercilessly flails U.S. follies in the Third World, such as our experiments with the electronic jungle battlefield, the cumulative effects of napalm bombings, or the techno-managerial approach of the counter-insurgency school. Social scientists engaged in such nonsense as Project Camelot or later efforts at rationalizing counter-revolutionary excesses come in for their share of the blame. This part of the book relies perhaps somewhat too heavily on secondary sources and occasionally on dubious press accounts. One might also wonder whether Girling is right in depending as much as he does on the pronouncements of American leaders as authoritative sources, although one is at the usual loss in suggesting what else the scholar might do, even if more of the secret record of policy making were at hand. Alas, U.S. actions in most of its interventions in the Third World accord well with the globalistic pronunciamentos set forth in the speeches of Washington policy makers.

The heart of Girling's thesis, which I acccept wholeheartedly, is that globalism does not work. This is so whether the global approaches are conceptions "from above," as in the containment policies of the earlier postwar administrations, or the interlinked credibility or modified balance-of-power policies of the later ones; or manipulations "from below," as in the specific (overt or covert) actions employed to bolster faltering elites in Third-World countries or to reduce the strength and appeal of their present or potential revolutionary opponents. The "push effect" that results from promoting the global environment in the direction of a world order desired by the U.S., one presumably conducive to enhancing fairly narrowly perceived U.S. security and economic interests, combines with the "pull effect" exerted by generally unpopular, unstable and threatened Third-World elites to draw the superpower into successively escalating stages of involvement. These produce agonizing and morally flawed interventions, which ultimately founder on their incompatibility with the democratic American domestic value structure. Such was certainly the case in Vietnam, as Girling well demonstrates, but he also documents an almost endless list of similarly escalating engagements and involvements elsewhere, from the Middle East to Chile, from Cuba to Thailand and Africa. The alternative to globalism is strategic disengagement (in the mode envisaged by Earl Ravenal) and a more particularistic approach to the Third World and its turmoil. But Girling is not sanguine that it will soon be adopted by the U.S., and he sees the other superpower engaged in a roughly similar search for global security and power, equally troubling to the prospects of world peace and socioeconomic justice in the Third World.

Girling forecasts a world of growing instability and fragmentation, providing "greater—not less—opportunity and 'necessity' for patrons to intervene in the troubled affairs of clients in order to safeguard their (patrons') interests from the threat of a takeover by rival interests. And there will be more rising patrons—not less—with both the capacity and the will to intervene" (p. 207). Short of a radical transformation of the perceptions of a desirable world order, unlikely to be achieved by either superpower, Girling finds the prospects grim indeed. And in this, I am forced to agree with him.

PAUL M. KATTENBURG

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Territorial Asylum. By Atle Grahl-Madsen. (New York: Oceana Publications, 1980. Pp. xvi + 231. $28.00.)

At this writing, issues of asylum are reported regularly in the newspapers to inform readers of every walk of life of the suffering of their fellow man. Thousands of Cubans jeopardized their future if exit were not granted by Castro, and a refuge offered in countries willing to take them. Asian boat people continue to leave their homes in the hope that some country will offer them asylum and life. There is the daily exodus of Chinese who try to find temporary or permanent asylum in Hong Kong. Many Haitians try to escape their homeland in pursuit of a safe haven. During the Moscow Olympics, stringent security measures were taken to curtail any opportunity of Russians and foreigners to seek asylum.

Against this background, I have read and reviewed *Territorial Asylum* by this distinguished Swedish scholar whose works include his magnum opus, *The Status of Refugees in International Law* (Sijthoff, 1966). This latest book provides those interested and working with the subject a valuable study on the present state of the law of asylum.

In the first 43 pages, Atle Grahl-Madsen presents an abbreviated account of the concept of asylum, existing rules on the right of states to grant asylum, an account of the municipal law on asylum as practiced in 15 states, and non-extradition and non-refoulement principles in international law. Grahl-Madsen ably discusses the contents to be included in any convention on territorial asylum; analysis involves potential provisions and their impact in regulating interstate relations and effects on the rights and duties of individuals.

After a brief review of the reasons for the failure of the 1977 United Nations Conference on Territorial Asylum held in Geneva, Grahl-Madsen presents his thesis for a protocol relating to territorial asylum. As he concedes, this approach may not bring international refugee law a great leap forward, but it is a step in the right direction. I find the author's analysis and suggestions for the proposed protocol balanced and well thought out.

The remainder of the book contains many documents pertinent to the subject—that is, all agreements, treaties, resolutions and international expressions, as well as individual drafts of a convention and protocol. Aside from the obvious merit of having many of these references in one book, the differing viewpoints in the drafts can be studied and left for further individual reflection and argument.

A list of abbreviations, index and bibliography are helpful to the reader. With respect to the bibliography, the author includes only a partial list from his earlier opus aforementioned. This work stands on its own merit and the bibliography could have been easily expanded to assist the researcher.

DANIEL C. TURACK

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The Western Alliance: European-American Relations Since 1945. By Alfred Grosser. Translated by Michael Shaw. (New York: Continuum Publishing Corp., 1980. Pp. xxiv + 375. $19.50.)

The time is ripe for a fresh look at the Atlantic Alliance. The economic crises of the 1970s and the uncertainties about American leadership have dated the definitive studies of the 1960s. Alfred Grosser, whose work on French and German foreign policy is unsurpassed in Europe, is a logical choice to tackle the complexities of the past decade.

A new analysis of European-American relations must disentangle continuity from change. While American analysts have concentrated on changes in Atlantic relations and particularly the diminished position of the United States, Grosser stresses continuities. The realities of mutual dependence for security, a commitment to pluralist regimes and economic interdependence dictate a loose alliance framework binding Western states together. However, competing perceptions of those realities held by leading actors guarantee a legacy of alliance disagreement and friction. National perspectives are a product of postwar identity problems. Britain and France had to adjust to the loss of empires; Italy had to cope with internal conflicts and cleavages; Germany sought to recover both its European and international legitimacy. Grosser is at his best when examining alliance problems from different national points of view.

Unfortunately, Grosser is not always careful to distinguish between problems of perception and conflicts in interest. The German desire to base European security on nuclear deterrence and the

American preference for a more flexible conventional and nuclear response is a difference based not on misperception but on conflicting interests defined by geography. One suspects that interest rather than perception (while the latter may influence the former) will prove the more effective analytical tool in any full accounting of alliance differences.

To stress continuity is to minimize change. For Grosser, the only important change is change in alliance structure. That structure changed only between 1947 and 1949 when Germany and Italy joined with the West and the Soviet Union shifted from ally to common enemy (p. 322). By defining change in this way, Grosser misses the opportunity to explore and explain the many changes that have taken place within the existing alliance structure. Because Atlantic boundaries have always been broad and flexible, changes that have taken place within the framework of the alliance often have been as important as changes in the framework itself.

Alliances, like constitutions, are the product of time and place. Their long-term usefulness is determined by the ability to adapt to a changing world. It is this rich legacy of adaptation that Grosser fails to address. In the 1960s and 1970s, three challenges have prompted extensive adjustment. In security affairs, the slow and uncertain accommodation with the Soviet Union and Eastern Europe has made security both less important and less manageable in alliance politics. What dominated the alliance agenda in the 1950s must now share top billing with economic affairs. While Washington prefers bilateral diplomacy with Moscow over SALT and multilateral negotiations to reduce conventional forces in the heart of Europe, alliance partners feel excluded from strategic discussions that affect them most and resent MBFR negotiations that serve as a reminder of Washington's flagging European commitment.

A second challenge has grown from the economic structure of the alliance itself. The liberal trade and monetary regimes have forged an economic interdependence in the 1970s quite unlike the dominant-dependent relationship of the 1940s. The interlocking of economies has followed the reduction of barriers to trade, free convertibility of currencies and the extensive foreign activities of banks and corporations. However, the management of this interdependence has been complicated by the active intervention of pluralist governments in their national economies. All too often, the demands of domestic economies have clashed with the constraints of interdependence. Where domestic priorities have prevailed, as has so often been true in the United States, Atlantic crises have followed. The attention given to the management of economic matters in Atlantic relations is the major change of the 1970s and as such receives far too little attention from Grosser. Clearly, he is uncomfortable with matters of money and trade which are squeezed in where chronologically appropriate but without sufficient background and explanation.

The third challenge to the alliance, the quadrupling of oil prices in 1973-74 affected the alliance in two important ways. It demonstrated a new American vulnerability to international economic forces that reduced an important alliance asymmetry that had permitted the Americans a posture of "benign neglect" in the late 1960s and early 1970s. Moreover, it internationalized those economic affairs that had been the exclusive domain of the Western Alliance. Not only OPEC but other Third World States and Japan now participate actively in negotiations to solve common problems. Grosser's neglect of these alliance dynamics makes *The Western Alliance* a book much like those that have preceded it. While its four parts treat the 1940s, 1950, 1960s and 1970s in turn, the emphasis is on the first two decades. Grosser handles this early material with a steady hand but, except for some interesting personal insights, there is little that is new. In parts 3 and 4, where there is considerable potential for new insights, the work is much less carefully done. The organization is unclear, the criteria for including some topics and excluding others is never made explicit and analysis seldom moves beyond impressions.

Sorting out the complexities of Atlantic relations is not an easy task. Simple generalizations are inadequate where security, economic and political affairs increasingly mingle and mix. Grosser has given us a useful rendering of the realities of the 1950s but not the new synthesis of the 1970s.

THOMAS L. ILGEN

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Regionalism Reconsidered: The Economic Commission for Africa. By Isebill V. Gruhn. (Boulder, Colo.: Westview Press, 1979. Pp. xiv + 154. $20.00.)

What function should regional organizations perform in the years ahead? Isebill Gruhn, noting a decline of interest on the part of writers in regional integration as theory and as process, argues strongly and cogently for a redefinition of Economic Commission for Africa (ECA) activities to bring it into line with the "developmental" concerns of governments. Gruhn's thesis is clearly stated: The earlier focus of African-inspired region-builders upon the transfer of resources to achieve economic development deals inadequately with the equally imperative needs for the manpower skills, institutions, and infrastructures "to convert capital into human welfare and societal development" (p. 26). Although Gruhn gives little background evidence to support her assertions on the earlier work, her general point on directing ECA's attention toward the facilitation of African regionalism is well taken. In particular, Gruhn's linking of ECA's regionalist role to the current effort to fashion a New International Economic Order (NIEO) gives this volume considerable policy relevance.

Gruhn's account of ECA organizational failures in the 1960s and 1970s explains much of the reason for Africa's overall lack of interest in this international agency. A variety of factors are set forth to describe ECA's inability to gain an important place for itself in the new African order: its insufficiency of focus and purpose, limited organizational capacity, low budgets, lack of African identity, dependence on foreign donors and personnel, and poor coordination with African governments.

Yet, for all these deficiencies, Gruhn stresses the need to remain fully apprised of the organization's contributions. These accomplishments are grouped under three broad headings. First, the ECA functioned as an arm of the international development assistance community. It generated and distributed data, established national statistical services, organized data banks, trained administrators and skilled personnel, and provided consultancy services. Second, the commission acted as a facilitator of interstate integration. It arranged inter-African conferences and contributed research and preparatory work leading to the establishment of such organizations as the African Development Bank and the Economic Community of West African States. And third, the commission set up its own subregional offices (most notably, the Multinational Programming and Operational Centers) to deal with interstate activities, and, ultimately, to serve as focal points for further subregional integration. For Gruhn, ECA's second role was its most significant, and, when outlining a program for ECA reform, she contended that it was the area which could most profitably be emphasized.

This brings us to the linkage envisaged between ECA and NIEO—the most intriguing aspect of the book. Gruhn—correctly, I feel—depicts ECA as a low-profile body, lacking focus as well as African roots. By tying its fate to NIEO, especially with respect to its integrationist role, she seeks to breathe new life and purpose into this agency. *Regionalism Reconsidered* thus changes tone toward the end, moving from analysis to prescription. "In short," she writes, "the time is now ripe—which it was *not* in the '60s—for African states to make arrangements that facilitate their interaction and allow them to maximize the utility of limited resources" (p. 119).

To grant that the time is now right for African integration efforts is not to demonstrate conclusively that this was not the case before. Certainly the advent of NIEO has altered the context within which regional integration must be discussed at this juncture—and Gruhn's insights are helpful here—but generalizations on earlier processes deserve an equally careful examination against the context of their times. This point is not meant to detract from the usefulness of this work, but mainly to express a wish for more—more about the realistic choice options of the past, more about the environment of contemporary choice, more about the decision processes at both the state and interstate level, and more about the ECA as a part of the larger United Nations operation. Isebill Gruhn deserves credit for moving us forward, but much remains to be written on this topic.

DONALD ROTHCHILD

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Why the Vietcong Fought: A Study of Motivation and Control in a Modern Army in Combat. By William Darryl Henderson. (Westport, Conn.: Greenwood Press, 1979. Pp. xxix + 163. $17.95.)

Why the Vietcong Fought is a narrowly focused examination of the organizational strengths of the Vietnamese People's Liberation Army (PLA) which enabled it to endure under the weight of massive American firepower introduced into Vietnam from 1965 onward. The author, a former U.S. Army battalion commander in Vietnam, undertook this study to discover why the PLA was able to endure, maintain its cohesion and remain on the battlefield while the U.S. Army was showing signs of the disintegration of small-unit cohesion.

The main sources for Henderson's study consist of 61 K-series interviews of PLA defectors and prisoners conducted by the RAND Corporation in 1965-67 and captured Vietnamese documents published as part of the *Viet-Nam Document and Research Notes* series. The K-series of interviews, named after the project's director Konrad Kellen, forms part of a 20-project study, totaling 2,400 interviews, undertaken by RAND. These materials were supplemented by the author's personal experiences and observations which include, most interestingly, private discussions with Warsaw Pact officials (p. 24).

Building up on this core of primary material, Henderson also incorporates the work of other specialists such as Douglas Pike (on the Vietcong) and Alexander George (on the Chinese PLA). There is one important omission, however. Although Paul Berman's 1970 Ph.D. thesis ("The Liberation Armed Forces of the NLF: Compliance and Cohesion in a Revolutionary Army") is listed in the bibliography and mentioned briefly in the text (where it is curiously termed a "recent study" on p. 96), no mention is made of Berman's book, *Revolutionary Organization: Institution-Building Within the People's Liberation Armed Forces* published in 1974. This omission is unfortunate because Berman's work, which also draws on the RAND interviews, is the most comprehensive and analytically satisfying account yet published.

Henderson attempts to answer the question why the PLA fought so well by examining the relationship between the three-man cell, the primary unit of the PLA, and the larger PLA organization, which he terms the secondary group, at the squad and platoon levels. In discussing the linkage between these two groups, Henderson rightly stresses the crucial role of the cadre-leader. The cohesion of the three-man cell is attributable to various factors: the cell's ability to cater to the material, social and psychological needs of its members, the formation of a "buddy relationship" among its members, and the fact that surveillance, control and criticism/self-criticism functions are performed within the cell.

The position of the cell within the PLA is also conditioned by various characteristics of the secondary group. Henderson discusses six cultural variables: nationalism, group orientation, orientation towards hierarchy, "face," status and fatalism. The role of the cadre-leader is crucial in linking the three-man cell to the larger organization. Henderson's discussion of the cadre-leader, which occupies over a third of the text, constitutes his main contribution to our understanding of motivation and control within the PLA. To quote Henderson directly,

> The cadre-leader, by linking the primary group to party/PLA goals, was seen as crucial to the accomplishment of the overall success of party/PLA goals, was seen as crucial to the accomplishment of the overall success of party/PLA objectives in South Vietnam. In order to succeed in his three roles of party agent, primary group leader, and chief of surveillance, the cadre-leader had to successfully: (1) transmit party goals to the three-man cell; (2) be dominant within the three-man cell and above; (3) conduct surveillance of individual and group performance to detect deviance and insure compliance with party/PLA goals; and (4) indoctrinate "good communist soldiers." To accomplish these functions, the cadre-leader drew upon the bases of power described [i.e., "reward," "coercive," "legitimate," "expert" and "referent" power] and exercised certain control devices such as criticism, surveillance, and isolation (p. 121).

During the 1965-67 period this system basically worked, according to Henderson, as "there was no widespread disintegration and there was no mass defection of platoon-size or higher units" from the PLA (p. 117).

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The Third World Reassessed. By Elbaki Hermassi. (Berkeley: University of California Press, 1980. Pp. 223. $15.00.)

Elbaki Hermassi, a sociologist, argues that "no serious attempt has been made to distinguish the sociological study of social structure from the sociological analysis of development" (p. 10). To promote the latter, he has written this series of essays, much of the validity of which comes from his own Third-World background (Algeria). The writing is stimulating and expository in form, designed to challenge Western theories of development. Instead of a new theory, however, Hermassi calls for inductive thinking "which puts theories on their heads" (p. 12). Although he denies having a paradigm, he seems to be guided by raw empiricism in which every datum seems equal to all other data.

Hermassi devotes much attention to the psychology of the alleged trauma produced by colonialism. Because of psychic trauma, he sees basically pathological societies which have little to offer their people. This perspective, he argues, flies in the face of Western scholarship which he consequently rejects by questionable generalizations in a work which otherwise condemns generalizations. Europe in its colonial domination "did not hesitate to destroy any form of humanity in order to shape the world in its own image" (p. 2). Western scholars "have failled to realize that no problem can be studied apart from its relations to others" (p. 13). Westerners think in terms of Progress, Evolutionism and Development, which guide modernization theorists on one hand and the neo-Marxist theorists of dependency on the other. Each fails to see that change is dependent on a "unique constellation of highly disparate events" which "can thus hardly be amenable to paradigmatic thinking" (p. 12).

Hermassi's corrective is a sociological analysis of development. Such an analysis need not destroy "Western concerns for rigor and validity" but would add an awareness of "Third World sensitivity to relevancy and immediate problems" (p. 12), as well as a "world-historical" perspective (p. 43). A sociological analysis would reveal that Third-World societies are divided societies; that they are not changing uniformly; and that in reality no development has occurred, whether the elites use self-reliance, capitalism, socialism, or international co-optation—"everything is still just about as it was" (p. 174). Furthermore, we must now reject the "catching-up" fallacy (p. 192) and recognize that the Third World can never be a high-productivity, high-consumption area. Knowing these things requires us to shift our research on Third-World societies from the general approach to the study of specific societies and "the particular historical conditions under which they confront the processes of national formation and transformation" (p. 200).

Despite these conclusions, Hermassi believes that something called development is possible to a limited extent (pp. 177, 185). He argues that the lot of Third-World peoples can be improved if the rich countries change their life styles (p. 192). Conversely, he also argues that "it is futile for Third World countries to blame the international system for their own troubles and to keep postponing their assumption of full responsibility for their own development" (p. 92).

This book is filled with provocative and often heuristic comments, as well as some infuriating ones. However, it will frustrate many political scientists. Although he argues for the "centrality of politics" and the "primacy of the state" (p. 201), he does not deal at all with the stuff of politics—who gets what, how values are allocated, what structures and functions are important, how political institutionalization occurs, the resolution of political crises, governors versus governed, or the meaning of *political* development.

Another frustration I felt stems from the fact that a "world-historical" perspective, which the author admits requires a long-term view, is missing from the essays themselves. A long-range view tells us that in all Great Revolutions (technological at their base) there will be cultures which come in last, that are furthest from the characteristics which led to the original changes, that are most resistant to change, and that are forced to change as secondary adapters rather than pristine ones. A long-range view tells us that societies change, and a mere generation is insufficient time to say what Third-World countries can or cannot do.

Finally, Hermassi's insistence that Third-World development should be studied without reference to the rest of the world is made without his ever explaining how developing societies (even as he sees them) can be evaluated without at least some notion of what developed ones are like.

CLAUDE S. PHILLIPS

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The Fall of South Vietnam: Statements by Vietnamese Military and Civilian Leaders. By Stephen T. Hosmer, Konrad Kellen and Brian M. Jenkins. (New York: Crane, Russak, 1980. Pp. 267. $14.50.)

This report summarizes oral and written statements by 27 former high-ranking South Vietnamese military officers and civilians about their perceptions as to the reasons for the collapse of South Vietnam. While they found much to criticize in their own behavior and institutions, the dominating factor in the debacle was the shifting role played by the United States in Vietnam.

The report focuses on the later stages of the war, first reviewing the period ushered in by the January 1973 Paris Agreements and then dealing with collapse, beginning with the loss of Phuoc Long in III Corps north of Saigon in early January 1975.

One major complaint was that the United States had saddled them with a military organization and tactics which were ill suited to combat the North Vietnamese and could not be maintained after the United States withdrew its forces and cut back the level of its military assistance.

The Americans were also criticized for the related Vietnamization program, both for starting it too late and for attempting to train the Vietnamese army in the image of the American forces. Initially, the Americans tended to push their allies aside, convinced they had all the answers. Only later did they attempt to put the conflict burden on the backs of the Vietnamese.

The Paris Agreements were also cited as a major source of the debacle, particularly these disadvantages:

> (1) the in-place cease-fire, which made its area of control vulnerable to Communist "land grab" tactics and made military difficulties for the GVN; (2) the fact that the Agreements left North Vietnamese troops in South Vietnamese territory; and (3) the establishment of a Committee of Concord and National Reconciliation, which gave the Communists too much legitimacy (p. 31).

They also were disturbed by the tough pressure exerted by President Nixon on Thieu to sign the agreements. They found even more pernicious the fact that violations of the Agreements were tolerated by the United States.

The South Vietnamese officials also perceived an almost callous abandonment of South Vietnam, particularly during the 1974-1975 North Vietnamese offensive. Paradoxically, this was mixed with their misplaced confidence that the United States would "do something" if the enemy seriously endangered their national existence.

These perceptions have been endorsed by responsible Americans, including U.S. Defense Secretary Melvin Laird (in "America's Principled Role in World Affairs," in *Detente and Defense,* ed. Pranger, American Enterprise Institute, 1976):

> As a member of Congress, I criticized the Americanization of the Vietnam War. As secretary of Defense, I had to win support from Congress for the de-Americanization of Vietnam through the initiation of novel assistance measures.

Concerning U.S. abandonment of South Vietnam in 1975, Laird stated:

> America's convenient neglect of its solemn pledge to guarantee the security of South Vietnam cannot be excused. No long-term goal was served by ignoring the enforcement of the Vietnam accords by shrugging off arms build-ups prior to the North Vietnamese offensive of 1975.

As ambassador to Thailand in early 1975, I saw pictures showing the Ho Chi Minh Trail being transformed into a major turnpike on which vast military stores were being moved south for the final assault. I had to justify to the Thai government the continued presence of B-52s and other combat aircraft on the Thai territory by claiming that the United States would strike if there were a major violation of the Paris Agreement by Hanoi. I knew such a response was highly unlikely. But the Thais as well as the South Vietnamese still believed we would not abandon what we had already spent in blood and treasure to defend.

The failure of Congress to appropriate the "supplemental funds" in early 1975 was the beginning of the end. Supplies of rice, petrol and ammunition began to dwindle. President Thieu ordered withdrawal from I and II Corps because of reduced U.S. assistance. Even with the best of troops a retreat in the face of an aggressive enemy is the most difficult maneuver to execute in war. It is no wonder the largely immobile South Vietnamese units failed this test and disintegrated in panic. The fall of Saigon became inevitable by early April.

The report concludes on this somber note:

> The destiny of South Vietnam, in the final analysis, was regarded to have rested in the hands of others—and in this sense was a matter of fate, which, after all, is a concept deeply embedded in Vietnamese culture. And, as one respondent concluded, "Fate was not on our side." (p. 200).

In retrospect, the American failure in Vietnam can be traced to the absence of cultural empathy and the arrogance of the "Best and the Brightest." A realistic evaluation of the American role in Vietnam is more than ever essential. This book will prove a useful starting point.

WILLIAM R. KINTNER

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Conflict and Violence in Lebanon: Confrontation in the Middle East. By Walid Khalidi. (Cambridge, Mass.: Harvard University Center for International Affairs, 1980. Pp. 217. $12.95, cloth; $6.95, paper.)

The Harvard Center for International Affairs has a distinguished history of publishing a high-level series of books on international politics and conflict. Unfortunately, CFIA's first—and one hopes last—misjudgment seems to be the publication of Walid Khalidi's *Conflict and Violence in Lebanon,* which is no more than a political pamphlet and tract written by an obvious propagandist and advocate.

The fact that the author is a Palestinian professor of political science at the American University in Beirut does not necessarily lend respectability to his work. The book on the conflict in Lebanon contains 122 pages (minus appendices, chronology and glossary), but only 20 pages actually deal with the Lebanon Civil War, another 24 pages with the actors in the conflict and only four with the Palestinian actors. By contrast, there are over 50 pages dealing with the hostilities and the aftermath of the Israeli invasion of March, 1978.

It is a fact that one of the key elements in the problems and struggles of Lebanon is the emergence of the Palestinian Liberation Organization as a power broker and as one of the chief destabilizing elements in Lebanon. Yet the PLO's emergence in Lebanon, its motivations, its military and political strategies and its aims are barely mentioned, and certainly are not analyzed or explained in depth. It is a remarkable sin of omission akin to writing a history of Islam without mentioning Muhammad.

The theme of Khalidi's extended political pamphlet is that the real nemesis of Lebanese stability are not the Palestinians but the Israelis, whom he paints as dedicated to destroying Lebanon and bringing about partition. It should not be surprising that Khalidi, a Palestinian intellectual and academic tainted by three decades of anti-Zionist propaganda and warfare, should take this turn in his book. It would have been much more profitable for CFIA to commission a younger, new-generation Palestinian to write a penetrating study of the Palestinians, their problems, aspirations and goals, rather than waste good paper on this peculiarily hollow and limited work.

The major chapter in Khalidi's book focuses on the Israeli invasion of Lebanon. This chapter is tendentious, inaccurate and rife with sins of omission and commission. The author contends, "One specific political objective of the Israeli strategy would seem to have been the disruption of the Shtaura accord of July 1977, and therefore the entire infrastructure of the Sarkis-Syrian PLO consensus on the interpretation and implementation of the 1969 Cairo Agreement. . . . The Shtaura accord had at least the merit of imposing a freeze on Palestinian operations across the border. Given its centrality to the slowly emerging Lebanese consensus on national reconciliation and the reconstruction of a national army, as well as its vital importance in the whole range of Lebanese-Syrian-Palestinian relations, the Israeli objective can only have been to throw everything into turmoil again" (p. 126).

This assertion is obviously off the mark and not proven. If the Israelis wished to "throw everything into turmoil again," their strategy would not have been the conquest of southern Lebanon but the annihilation of the PLO's army, a strategy that involved Syria in the North. The author reluctantly admits that the tacit Israeli-Syrian alliance helped to cripple the PLO in 1978.

Khalidi has difficulty in making up his mind just what the Israeli strategy is: containing the PLO, containing terror in Israel, the conquest of South Lebanon, the partition of Lebanon or a response to the challenge of the Syrians. One has to assume that these cannot be complementary goals and not all of them can be Israeli goals. It should be noted that Israel is interested in a stable Lebanon, replacing its conflict with Syria with peaceful borders, not because it holds great love for Lebanon, but because it wants nothing to do with a fourth front on its borders. Thus, it is because of the tacit Israeli-Syrian bargaining that Lebanon is still intact and in one piece. Khalidi politely dances over the implication that Lebanon is in fact, if not formally, under Syrian military protection and hegemony.

Khalidi peremptorily dismisses the Syrian role in the Lebanese conflict in less than nine pages. And, without evidence, and contradicting himself, he arrogantly imparts a great deal of responsibility to the United States.

Conflict and Violence in Lebanon is not a study of conflict and violence in Lebanon. It is clearly a pamphlet, bewailing Palestinian woes, decrying Machiavellian U.S. strategies, and belaboring the arch-villanous Israelis. There is probably a lesson to be learned here, not because of what Khalidi has written but because of what he has *not* written. The lesson demonstrates the need for an authoritative, insightful, and impartial study of the real conflict in Lebanon, a study that will include an incisive look at the PLO presence and tactics in Lebanon. What Khalidi has accomplished is to point out the dearth of information and the large amount of misinformation that exists in this area of study.

AMOS PERLMUTTER

American University

The Origins of the Cold War in the Near East: Great Power Conflict and Diplomacy in Iran, Turkey, and Greece. By Bruce R. Kuniholm. (Princeton, N.J.: Princeton University Press, 1980. Pp. xxiii + 485. $27.50, cloth; $10.50, paper.)

These are fitting days in which to give heed to an expert rehearsal of the action in the Near and Middle East theater at the onset of the First Cold War. Bruce Kuniholm provides that record in a tome of vast learning and detail. It is diplomatic history written mainly for diplomatic historians, a massively footnoted reference work more than an analytical synthesis. But there is an awesome wealth of evidence here available also for others to mine about the origins of containment.

Kuniholm's own approach is post-revisionist, nonpolemical. He rebuts revisionist interpretations of his specialty (Greece, Turkey, and Iran) but readily concedes utility to much of the revisionist writing about other (especialy later) cold-war conflicts. His principal theme is "the fact that American policies [in the region] emerge from a framework of traditional European power politics, [and] traditional interests along the Northern Tier. . . . American-Soviet rivalry gradually supersedes the earlier Anglo-Russian rivalry in the region" (p. xvi). Kuniholm provides background on hundreds of years of Czarist expansion and the Great Game between Russia and England and treats all of World War II thoroughly; likewise, his ensuing coverage of 1945-46 Iran, Greece, and Turkey is impressively searching and professional. To be sure, 1947—the Truman Doctrine itself and the Greek-Turk aid program—gets skimpy treatment by comparison; but that can be somewhat excused by the greater availability of other monographs on that particular crisis.

What remains troubling is the rather uncritical ease with which Kuniholm glides into the view that "the policy of containment in the Near East was a realistic and pragmatic policy. . . . If one were to find fault, it would have to be in its rationalization [ideologization, globalization], in the legacy of that rationalization, and in analogies engendered by the policy's success in the Near East" (p. xx). Kuniholm's own assumptions are matter-of-factly geopolitical. Americans came to "recognize" their country's interests in the region as being "synonymous" with Britain's (p. 382); the resulting policy was "well-founded" (p. 431). He assigns the credit mainly to State Department careerists. (Perhaps the availability of documents skews the evidence in the latter direction, but it remains impressive—though regrettably not balanced by any thorough, respectful treatment of Congress and public opinion; even the presidents get rather short shrift whenever it appears that their own naive idealism or domestic political considerations might have been delaying their acceptance of geopolitical imperatives.)

Kuniholm apparently sees two of these imperatives as ultimate. In my view he presents them too uncritically. He accepts without much question (a) that America did need Middle Eastern oil and (b) that, but for containment, communist pressures (internal and/or external) would have thoroughly destroyed Greece, Turkey, and Iran as buffers on the north.

Kuniholm is able to prove convincingly that the pressures were intense. Yet in Greece (as he admits) the Russians were generally honoring their October 1944 spheres-of-influence deal with Churchill (even if the Yugoslavs, Albanians, and Bulgarians were not). And in Turkey what the Russians were demanding was eastern provinces that had already been theirs from 1878 to 1921, plus bases at the Dardanelles. Moreover, in Iran, they "would undoubtedly have accepted a division of [the country] into spheres of influence" (p. 204); the British of course were peaceably retaining anyway their own sphere of influence in the oil-rich south. Kuniholm's sense is that Turkey and Iran could not have survived the Russian nibbles without becoming Soviet satellites; hence they would have been fully eliminated as buffers for the oil of Iraq and the Persian Gulf. This may be true (or perhaps Soviet-American friction over rump regimes would have become a major aggravation in the impending cold war anyway)—but alternative scenarios deserve more consideration than Kuniholm offers.

More fundamental is Kuniholm's neglect to spell out with sufficient thoroughness just why Persian Gulf oil was considered so crucial for America in the mid-1940s and why he concurs in this opinion: was it directly crucial, or just indirectly because it was already so important for Britain and Britain was important for America? Was it crucial to have the oil ourselves, or to keep Russia from having it? If the latter, did we aim to keep their economy weak, or to ensure that they would never have a spigot to turn off against us? And was the implicit objective one of forestalling the natives, not just the Russians—i.e., preserving cheap royalties, not just access per se? How many of these arguments were made justifiably, at the time? Thirty years later one could see clearly that the OECD countries were enabled to float onward to prosperity in the 1950s and 1960s on a sea of cheap Middle Eastern and North African oil—yet in the process they all became excessively addicted, much more so than even Britain had been in 1945. Today the withdrawal symptoms have become so agonizing that the mid-1940s definition of America's national interests as including "friendly," buffered governments along the Persian Gulf—whether wise or unwise then—has become self-fulfilling and hence is probably wise today. But its being wise today does not make it wise then. These are difficult questions that one wishes Kuniholm had pondered in his otherwise masterly book.

H. BRADFORD WESTERFIELD

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Latin America, the United States, and the Inter-American System. Edited by John D. Martz and Lars Schoultz. (Boulder, Colo.: Westview Press, 1980. Pp. xvi + 271. $24.50.)

This collection contains original essays—several coauthored—by 14 social scientists, predominantly from political science, all of whom were either students or colleagues of Federico Gil at the University of North Carolina, Chapel Hill. The book is intended to honor Gil and the contributions he has made over the years to the understanding of Western hemispheric relations. Toward that end, the collection succeeds reasonably well. The essays are intelligent and thoughtful, in a contemplative and sometimes analytic manner. But as a group the essays simply don't constitute a coherent, let alone systematic, survey of the subject. The topics, while often timely, seem arbitrarily selected, ranging so broadly in subject matter, scope, and not least of all, style and organization, that it is difficult to imagine any reader finding the collection totally useful or even interesting.

The editors make a valiant effort in the introduction to identify four basic themes: the dominance of the United States, the role of humanitarian values (e.g., questions basically of human rights), the critical importance of economic variables, and something they term "the more things change. . ." (p. 12), which essentially translates into the familiar ebb and flow of U.S. interest in Latin America. These themes do indeed appear throughout the collection, but rarely in organizationally separable or conceptually distinct ways. What seems to emerge is the perhaps obvious assertion that understanding the inter-American system requires a knowledge and sensitivity to Latin America and the United States, the values and perspectives which separate these regions, and the reactions within each created by their historical relationships.

The first of the three sections of the collection, titled somewhat amorphously "Perspectives on Inter-American Relations," addresses some of the traditional and contemporary overviews (paradigms) of inter-American relations, specifically a liberal-developmental model, dependency, and what Steven W. Hughes and Kenneth J. Mijeski in their chapter call "organizational ideology," best represented according to them by "the preliminary efforts of Guillermo O'Donnell" (p. 25). They conclude by arguing for tolerance and scientific pluralism, and, acknowledging their debt to Paul Feyerabend, "methodological anarchism." John S. Gitlitz and Henry A. Landsberger provide a workmanlike and considered review of dependency as the concept has been applied to Latin America, again urging restraint and moderation, and admonishing its advocates to consider, "Is evil perhaps not a good deal more widespread than capitalism?" (p. 69). Gustavo Lagos and Alberto Van Klaveren point toward a diversification of Latin America toward closer relationships with Europe, Japan, and Third-World Nations, and less dependence on the United States.

The second section of the book, "United States-Latin American Relations: The Shape of Hegemony," contains a useful if somewhat elementary chapter by Kenneth M.. Coleman ("The Political Mythology of the Monroe Doctrine: Reflections of the Social Psychology of Hegemony") and a discussion by John D. Martz of the important if sometimes implicit conflicts between Latin America and the United States in interpreting the idea of democracy and its underlying values. The section also contains an excellent review by Enrique A. Baloyra of Cuban-U..S. relationships in the seventies, a good, old-fashioned blow-by-blow analysis followed by some provocative questions regarding future relations between the two countries.

The third and final section of the book presents three chapters dealing contemporarily and historically with U.S. attitudes toward human rights in Latin America, including a detailed account by Joseph S. Tulchin of the impact of U.S. human rights policy in Argentina. Collectively, these chapters identify many of the dilemmas resulting from current U.S. policies, and the important historical antecedents that promoted them.

Had this book gone through another editorial draft, with greater attention to matters of organization and style and more integration among the contributions, it would probably have been improved to the extent possible for a project of this kind. I assume the exigencies of publishing the volume precluded such revision. If the result is less than ideal, the book nevertheless makes some useful if random contributions.

RONALD H. MCDONALD

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Threats, Weapons, and Foreign Policy. Edited by Pat McGowan and Charles W. Kegley, Jr. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. 324. $20.00, cloth; $9.95, paper.)

This volume, dedicated to the memory of John Gillespie, is the fifth in the series of Sage International Yearbooks of Foreign Policy Studies. It is the second in the series which has focused on a specific aspect of foreign policy and, like all others in the series, it seeks to maintain a comparative perspective.

The editors wish in this volume to bridge the gap between "scientific foreign policy studies" and "national security/defense studies." To do so, they bring together nine articles and a bibliographic review by a selection of analysts at research corporations in the Washington area and academics. One of the interesting features of the book is the consistency of methodology and substantive interests of the two author subsets—it makes one almost wonder exactly what gap is being bridged. The pieces almost uniformly reflect serious scholarship and make important contributions to our knowledge.

The chapters fall fairly clearly and are placed into three sections: threats and foreign policy, weapons and foreign policy, and modeling arms races. Although there are clearly other topics of interest at the intersection of foreign policy and defense studies, such as various aspects of military conflict, disarmament negotiations, or policy-making processes, the subjects chosen are of great importance and interest. It is good that the editors have somewhat restricted the scope of issues examined because it does allow some comparison and contrast of articles.

As in all edited collections, the contributions vary in quality and impact. Articles by Farid Abolfathi on U.S. military spending and public opinion, by Charles Kegley, Gregory Raymond and Richard Skinner on bases for nuclear armament decisions in various countries, and by Michael Wallace on superpower arms spending will be of special interest to many; they seek to bridge a more important gap than that noted above, but one which is only touched upon by the editors, namely that between scientific research and policy relevance.

That point brings us to one of the least satisfactory aspects of an otherwise well-selected set of solid, scholarly pieces. After finishing the collection, presumably most readers are going to ask exactly what they know that they did not know before they began. A variety of individual insights and conclusions come to mind, but any organization of them will need be provided by the reader. The editors can hardly be expected to provide a theory of foreign policy. They could, however, have helped the reader to collect the individual insights and conclusions and to sort them in terms of theoretical and/or policy implications. The volume is considerably coherent in methodology and subject matter, which is not always true of edited collections, and provides a base upon which one could build further.

More generally, it appears to be time in the life of this valuable series for the editors to step back and review progress. That review should definitely not be in terms of the number of scientific and comparative foreign policy articles published in their volumes or elsewhere (as in their annual bibliographical essay) but in terms of the knowledge gained and systematized. By identifying a structure of learning the review could also go far in pinpointing weaknesses in it. This is, in fact, the direction in which the editors are moving, and perhaps the sixth volume will provide the needed review and overview.

BARRY B. HUGHES

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Terrorism and Hostage Negotiations. By Abraham H. Miller. (Boulder, Colo.: Westview Press, 1980. Pp. xvi + 134. $16.00.)

This book, one in a series published by Westview on terrorism, is devoted to comparative analysis of past "barricade and hostage" situations, and the devising of effective strategies for coping with a feared spread of international political terrorism to American soil. Drawing on case materials and interviews with high-level decision makers in the U.S. and abroad who have been involved with past hostage episodes, it was written just a bit too early to incorporate the recent events in Teheran, London, and Bogota, which in combination might have provided valuable, albeit prescriptively ambiguous, additional lessons.

Abraham Miller's work is pegged at two somewhat distinct levels, seeming to reflect, respectively, his background as a professional political scientist and his recent experience as a visiting fellow with the National Institute of Law Enforcement and Criminal Justice. On one level, there is a wide-ranging though succinct discussion of terrorism as an aspect of contemporary international relations, ramifying into subjects such as: the role of ideology in defining terrorist movements, the UN's handling of the issue (it recognizes only Western "colonial" terrorism), terrorism as a stage of revolutionary warfare, the connection of terrorist groups to major power sponsors, terrorism as—in part—a creation of the mass media, and so forth.

With this as background, Miller then proceeds to focus more narrowly on the tactics applicable to coping with one type of terrorist operation, that involving the taking of hostages. The subject is competently handled, though at some points virtually on the level of a "how to do it" manual for "SWAT" teams which might, for better or worse, perhaps be of more interest to a police trainee than to a student of international affairs. Discussed here are recruitment and training of personnel, tactical procedures in hostage situations, the use of aerobics and isotonics in producing physically fit police assault squads, and the pros and cons of forming elite SWAT teams for counter-terrorist operations.

Miller is clearly concerned that the virtual immunity to international terrorist activities enjoyed by the U.S. up to now within its own borders may not last indefinitely, and that we must be prepared —politically and operationally—for the eventuality of our own "Maalot." Generally—and this is his central thesis—he asserts that the experience of Americans and others with *non-political* hostage situations (for instance, those arising ad hoc from ordinary bank robberies), the vast majority of which have seen happy outcomes, are more applicable than is commonly assumed to those with international dimensions. Using the bizarre Hanafi Muslim episode in Washington as a bridging case between the essentially political and nonpolitical, Miller recommends predominant reliance on protracted, patient negotiations in lieu of the more "macho" assault tactics which he notes have cost more lives than the actual killing of hostages by terrorists. In analyzing the frequent successes in such negotiations, he discusses the cumulative impact of the captors' weariness, development of "transference" between them and their captives, the ritual value (to the captors) of the negotiations themselves, and the assertedly normal absence of nihilistic suicidal intent by the terrorists.

As Miller is himself well aware, this prescription runs head-on into a contrary conviction—most tenaciously held in Israel but also by some in the U.S.—that a tougher "surrender or die" posture may *deter* still more frequent terrorist actions. Whereas he claims, however, that such an Israeli policy (and the formally stated U.S. position of "non-negotiation") have not in fact deterred further attacks, others might rebut that it has helped restrict the problem to manageable proportions; in Israel, such things have not yet seriously escalated. Miller should probably also have stressed that the Israeli and European (and American) situations are here fundamentally distinct. On the one hand, one is dealing with a real threat to a nation's existence; on the other, with the kind of spectacularly irrelevant, if sometimes deadly, mischief of a Baader-Meinhof Gang which, not surprisingly, produces vastly different perspectives on deterrence, the "expendability" of hostages, the long-range implications of capitulation to demands for release of prisoners, and so forth. More difficult to analyze is the question of whether the U.S., as a superpower and with a perhaps far less cynical and jaded public opinion than was true of France when it released Abu Daoud for oil and an arms deal, could so easily absorb the humiliation of capitulations which have come to appear near-routine in Western Europe.

One might wish that some other important themes merely bruited in this book had been elaborated upon—for instance, the effect of Arab oil leverage and of some terrorist groups' links to the USSR upon the outcomes as well as the origins of hostage situations. (The latter question is ably discussed by Charles Horner in "The Facts About Terrorism," *Commentary,* June 1980.) Then, too, Miller seems to nibble at the bare edges of a very touchy matter he presumably dares not make explicit, in gently taking the Carter administration to task for not regarding terrorism as a sufficiently serious problem, and for not taking the lead in combating it. In making this charge indirectly, Miller discusses the works of former ACDA Chief Scientist Robert Kupperman, who is portrayed as a voice in the wilderness, one apparently dismissed as an alarmist by administration leaders because of his stated anxieties about possible future terrorist use of weapons of mass destruction. Vaguely hinted here is the suspicion that the administration's low-key handling of the terrorist issue may be less a matter of its being perceived as a mere bagatelle than of its reflection of the administration's broader Middle Eastern policy concerns. Saudi Arabia, after all, is a major financier of the PLO, and the latter is also closely linked to Iraq and Algeria, two radical nations which have been quietly but persistently wooed by Washington in recent years. This book could, hence, perhaps have been enlivened by a franker discussion of the "Europeanization" of America's terrorism policy by analogy with the vaunted "Finlandization" of Europe.

ROBERT E. HARKAVY

Pennsylvania State University

India and Africa. Edited by R. R. Ramchandani. (Atlantic Highlands, N.J.: Humanities Press, 1980. Pp. xiii + 314. $15.00.)

This volume contains 15 of the 17 papers presented at an All India Workshop organized under the auspices of the Centre of East African Studies at the University of Bombay in October 1977. Almost all the contributors are Indian academics, although there is some representation from India's diplomatic service. The papers are separated into three categories: (1) Historical and Political Dimensions; (2) Indians in East Africa; and (3) Economic Dimensions. All the papers, however, follow a similar theme: India and Africa are inextricably intertwined, reveal age-old associations, share a common imperial/colonial experience, identify with nonalignment, are eager to grow and prosper, and are determined to control their own destinies. The book is in some ways a clear and unequivocal statement that India is an emerging great power, and considers Africa, particularly sub-Saharan Africa, an important region in its expanding national interest. A complementary theme therefore focuses on the legacy of European colonialism and the more contemporary efforts by external powers, especially the United States, to influence and dominate the struggling people of Africa. There is throughout the volume either the explicit or implicit conclusion that India's relations with Africa are noble, salutary and beneficial, whereas the purposes and intentions of the Western nations are at best suspect and more than likely insidious.

Although the papers for this volume were prepared more than three years ago, no effort has been made to bring them up to date. Consideration is given to superpower activity in the Indian Ocean but the data base is very limited and the analysis thin and lacking in balance or objectivity. Mention, for example, is made of the conflict between the Somali Republic and Ethiopia on the Horn of Africa but little attention is given to the nature of the conflict. It is also surprising that Satyavati Jhaveri, who is so sensitive to a renewal of big-power maneuvering on the African littoral of the Indian Ocean, refuses to examine the role of Cuban forces in West and East Africa. To suggest, as he does on p. 90, that Castro was only interested in bringing hostilities between Ethopia and Somalia to an end and to induce the "contending areas to form a federation" is betrayed by subsequent events. The creation of a Marxist state in Ethiopia and infiltration of the Ethiopian government by Soviet, Cuban and East German "advisors" as well as the pressure on Mengistu Haile Mariam to fully communize the political system in the country indicates that the Kremlin and Fidel Castro were not simply interested in helping the Ethiopians overcome the "tyranny" of Haile Selassi. The author, however, sees no purpose in pursuing this matter. Rather, Indian concern over American activity on the Indian Ocean island of Diego Garcia is highlighted and given unusual prominence.

Appa Pant's study is similar. He welcomes the assertiveness of OPEC and implies that the industrialized countries at last will be forced to pay the price for their extravagance and "intolerable" burden that they have placed on the "world community" (p. 30). At no time, however, does he examine the impact of OPEC pricing policies on the petroleum-deficient Third-World countries. Moreover, his nonalignment statement does not even venture into an analysis of the reputedly nonaligned, such as Cuba, to carry forward the policies and programs of the Soviet Union, e.g., in Angola and Ethiopia.

Jayanti Patel's long article comparing the Indian political party system with African political party systems is perhaps the most scholarly contribution in the book. Patel takes extraordinary pains to explain how the Indian system was influenced by the European liberal tradition that rests on pluralism and why the Africans were more inclined to follow "Socialist" Europe, the belief in class struggle and revolutionary party organization. Africans more so than Indians, argues Patel, were required to resort to arms in achieving their independence. Thus they were more likely to "create parties with monolithic and penetrative character appropriate for launching an attack upon the colonial regime" (p. 133). Patel is hardly sanguine about Africa's chances for developing competitive party systems. The one-party system, the administrative state and the mobilization model appear to be more in Africa's future than India's more free-wheeling and open political experience.

R. R. Ramchandani has written an interesting article on the role and future of the Indian community in East Africa. He notes that the Indians went to Africa, essentially as indentured labor, to work on projects designed by the European colonizers. Many remained on and their community swelled so that at the time of independence for the peoples of East Africa they represented a notable, often important economic concentration. The black African population, however, did not look kindly on their Asian neighbors. In the years that followed independence, and no longer protected by European power, the Asians found themselves in the precarious position of neither being welcomed in Great Britain nor of being permitted to stay on in their African homeland. Their flight to India and the lukewarm, if not unsympathetic treatment in their historic country of origin is not one of India's noblest moments. Ramchandani attempts to explain this inhumane treatment of the Indians of East Africa. He notes the contributions to East Africa's development made by the Asian community but says the rampant xenophobia in the region negates their good works. Ramchandani cautions against retaliation in any form. India, he suggests, must ingratiate itself with East Africa and the larger continent. In this regard, India must view its overseas community as something of a liability. Indian assistance to East Africa, however, is not ruled out. In fact, the author believes that India's large pool of trained personnel can be of substantial utility to the people of Africa. And with a positive approach to economic and technological cooperation, India will not only win the confidence of the Africans, but make life more bearable for those members of Africa's Indian community who remain there.

Additional articles in this book examine Indian communities on the Indian Ocean island of Mauritius, the role of Asians in Kenya's independence movement, as well as Indian contributions to Africa's quest for equality, its need for technology and overall cooperative endeavors. The book concludes with a unique study comparing urban growth in India and Africa.

The reader will find a mixed bag in this collection of articles. The perspective, however, is a rare one and there is no ignoring the insights the book provides into contemporary Indian thinking about their country's role in an expanding and more demanding world.

LAWRENCE ZIRING

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The European Parliament: The Three-Decade Search for a United Europe. By Paula Scalingi. (Westport, Conn.: Greenwood Press, 1980. Pp. x + ° 221. $18.95.)

The European Parliament has not attracted a great deal of scholarly attention until quite recently. It is hardly surprising that the subject has not been popular, since the European Parliament was for many years a nominated body with only a very modest, advisory role in European Community affairs—in fact, not a parliament at all. The change came in June 1979 when, for the first time, the Parliament was elected by direct, universal suffrage in the nine members states of the European Community. At the time, many viewed this as a development of some significance in the history of European integration. Paula Scalingi's informative book is in some ways a chronicle of how, after 30 years of effort and compromise, this milestone was reached.

As a chronicle, *The European Parliament* is very successful. It is meticulously researched and packed with information from many different sources on both sides of the Atlantic, many of them rarely found in English-language works. Unfortunately, however, the author does not go much beyond the historical approach and thus the potential for analysis, explanation and speculation is for the most part lost.

Scalingi suggests both in the subtitle of the book: "The Three-Decade Search for a United Europe" and in the preface that there is a correlation between the fortunes of the Parliament and moves in the direction of "political integration." If this is indeed the case, are we then to assume that the European Community is holding to the aims originally enunciated in the Treaty of Rome and that the Parliament, despite its lack of formal powers, has become an important building block in some form of political community? There is not much evidence to bear this out. In fact, the direct elections decision came at a time when many observers both inside and outside the Community were suggesting that things were going badly. Nor is there any solid evidence provided to support Scalingi's thesis that there is a "predictable cyclical pattern that has determined the course of European unification and the development of a real European Parliament" (p. x).

The whole question of the nature of a "real European Parliament" is a fascinating one that deserves thorough examination. The Parliament as it has existed until now fits no institutional category. It is widely recognized that it has greater significance than other international assemblies and less than the assemblies of truly federal systems of government. Can its limited powers and impact be explained by internal factors such as the absence of a constitutionally established government at the Community level, or is it necessary to look further for an explanation, perhaps to the decline of parliaments throughout Western Europe?

In many ways the European Parliament is an institution in search of a role. It has all the trappings of a political body and yet partisan battles are rarely fought out on the floor of the house. Indeed, distinct party groupings were present from the beginning yet one looks in vain for the development of the kinds of adversary relationships that exist between members when they sit in their national parliaments. Direct elections have only crystallized the party structure and 1979 even saw the emergence of transnational party platforms. All of this notwithstanding, the observer cannot help feeling that the European Parliament may now look more like a "real parliament," but that is as far as it goes.

By confining herself almost exclusively to a documentary treatment, the author of *The European Parliament* has lost a golden opportunity to provide the student of European integration and parliamentary process alike with much needed insights into some substantive theoretical problems.

GLENDA G. ROSENTHAL

Columbia University

To Augur Well: Early Warning Indicators in World Politics. Edited by J. David Singer and Michael D. Wallace. (Beverly Hills, Calif.: Sage, 1979. Pp. 308. $17.50, cloth; $8.95, paper.)

This book belongs to the growing class of edited volumes devoted to examining aspects of international politics from a quantitative perspective. Essentially, it offers a set of ideas about warning indicators: deciding which ones are important and how to measure them; what particular concepts they represent and the form describing their abstract-to-concrete linkage; and overall, how conceptually clarified, numerically specified variables permit the analyst to account for and predict the events and activities characterizing world affairs.

Nine papers and a composite introductory piece are brought together around two central topics: early warning indicators and contemporary forecast methodology. The volume includes "only original unpublished work that reported genuinely reproducible, policy-relevant findings, and reflected a high standard of data-generation and analysis" (p. 13). Following an unwritten rule, but a growing custom in works of this nature, the volume divides rather neatly into sections of empirical applications that use in turn, the systematic approach, a national attribute's framework, and analysis focused on national behaviors.

With respect to substantive concentration, no particular problem area dominates. A variety of issues is treated in these pages, among them: alliances, international and domestic conflict, nuclear proliferation, crisis perception, and foreign policy orientation. Obviously, these topics are important ones befitting international politics, but it is not clear to the reader just how the chapters are intended to produce a composite, comprehensive, or assembled picture that captures a fresh look at the field. This is not the avowed purpose of the collection, however, and the editors refrain from suggesting any plan of substantive cohesiveness across topical domains addressed here.

The book is accurate, I think, in its *diagnosis* of intellectual, theoretic, and policy-making problems that face those charged with anticipating and comprehending world events. Michael Wallace's lead essay, "Early Warning Indicators from the Correlates of War," states:

> The past two decades dramatically exposed the inability of traditional "experts" in the world politics field to predict even the rough outlines of the changes taking place in the global system. The list of their failures is legion: the outcome of the war in Indochina, the renewed strategic arms race that threatens one more, the fall of a large fraction of Third World civilian regimes to military forces, skyrocketing energy prices, and—as a partial corollary of this last—world-wide economic "stagflation" with its attendant monetary and trade rivalries. All of these (and more) either "couldn't happen" or were largely unforeseen by the keepers of the temple of conventional wisdom. Indeed, if our political and economic soothsayers were baseball players, their latter-day batting average would see most of them instantly sent down to the minors.
>
> At first blush, one might suppose that these failures would have a silver lining for peace researchers and other quantitative social scientists. Since armchair experts have been so often discredited, the way is surely open for those who advocate the application of scientific rigor to the tasks of forecasting and planning. But this has not taken place; rather the reverse has occurred. In recent years, the apparent resistance of policy makers to new intellectual approaches has, if anything, grown. This seeming paradox is, I believe, largely explained by reference to three factors, two relatively transitory phenomena, and one underlying, existential dilemma. These are, in order: (a) the rather small number of findings in the world politics which can readily and safely be used by either policy makers or counter-elites; (b) the "overselling" of the predictive power of social science that has occurred in recent years; and (c) inherent limitations on the impact *any* scientific knowledge can have on policy-making (pp. 17-18).

With some hesitation, it might be said that the articles, in sum, present a *prognostic* litany of these problem factors noted by Wallace. This is accomplished by showing corrective pathways to circumvent a specific obstacle (e.g., how to use research findings appropriately in a policy context); or at the very least, the papers provide commentary to help us understand why the current design and management of quantitative aids in public policy analysis is non-optimal with respect to information on early warning signs.

The papers are built around several major data collections extant in international relations and contain, as well, some infrequently used analytic techniques for sifting through and finding patterns in vast amounts of information. (Two of the nine have methodological appendices, one on the adaptive systems algorithm and procedures, and another on the Partial Least Squares model.) Authors and papers include: Wallace, "Early Warning Indicators from the Correlates of War Project"; Siverson and King, "Alliances and the Expansion of War"; Dewitt, "Breakpoints and Interstate Conflict"; Wilkenfeld, Hopple and Rossa, "Socio Political Indicators of Conflict"; Gurr and Lichbach, "Forecasting Domestic Political Conflict"; Meyer and Brewer, "Monitoring Nuclear Proliferation"; Sullivan, "Foreign Policy Articulations and U.S. Conflict Behavior"; Phillips and Rimkunas, "A Cross-Agency Comparison of U.S. Crisis Perception"; and Chan, Kringen and Bobrow, "A Chinese View of the International System."

How does one evaluate this "book" when the topics and techniques cover such broad ground? I have chosen only two issues for appraisal in light of Wallace's characterization: (1) Are there any significant generalizations or theoretic ideas to provide insight on the early warning question? Yes, indeed. Each piece expresses in its title a hint of the conclusions and general points to come. There are specific results too numerous to be given here. Among the noteworthy ideas with a touch of novelty or profundity are these: Dewitt's notion in "Breakpoints and Interstate Conflict" (more on the novel side) of a *breakpoint,* which he defines as an indication of fundamental change in the dynamics of international relations. His analysis of retrospective forecasting using macro-level variables to track major state activity culminating in World War I appears as a useful, scholarly exercise. Just how breakpoint possibilities could be monitored into the future, however, is unclear. Meyer's and Brewer's work in "Monitoring Nuclear Proliferation" is profound and clever. They lay out existing procedures for monitoring nuclear weapons proliferations, point to the inherent limitations to them, and then present a conceptualization for warning or assurance signals using technological capability, motivational profiles, and technological process monitors of nations. The scheme is diagrammed with useful figures and accompanying verbal descriptions. It is the best piece of the lot partly because the early warning conceptualizations could be applied to a variety of substantive areas beyond the *n*th country problem.

(2) What should policy makers expect from the analyses found here? Overall, the answer is that quantitative, computerized devices called "models" are highly intuitive representations of

the world, and are rightly approached as analytic perspectives that may or may not capture important features of a given problem. The aim of quantitative assessment is to set a framework to enhance and extend judgment by bringing together scientific objectivity and subjective interpretation so that a decision maker can understand explicitly the basis for action and attendant underlying perceptions of the situation. Each of these articles in its own way, provides support for a limited view of the impact of the discoveries of social science on policy choices. Numerous suggestions are made regarding monitoring impending, undesirable developments on the world scene. No method is foolproof or final. In short, there is no single, best way to track and watch the omens that may befall us, but some good ideas have been brought to light.

KAREN A. FESTE

University of Denver

Power Politics and Southeast Asia. By Lalita Prasad Singh. (Atlantic Highlands, N.J.: Humanities Press, 1979. Pp. xi + 208. $12.50.)

A special focus on the international crises of the moment is a prominent and even necessary characteristic of the foreign policy operations of all the countries of the world. The United States, however, seems to be particularly prone to exaggerate the immediate issues, frequently almost to the exclusion of all other aspects of international affairs. Thus, as this review is being written in early June 1980, the core issues (although fading) are U.S. hostages in Iran, Russians in Afghanistan, and the Olympic boycott. As for Southeast Asia, if we discount the recent drama over the "boat-people" refugees from Indochina, we might well conclude from our popular as well as more academic literature that the fall of Saigon in 1975 was for Vietnam as well as Southeast Asia in general a fall not only from grace but also from earth. In this context, Lalita Prasad Singh's overview of the foreign affairs of the Southeast Asian states should be an important book.

Part 1 (The Great Powers and Indochina) includes separate chapters on Vietnam, Laos, Cambodia, and the ICC (the truce supervisory commission established by the Geneva Accords of 1954). Part 2 (Regional Power Politics) focuses on the interaction between and among Indonesia, Malaysia, and the Philippines, while part 3 (New Trends and Developments) covers Sino-Soviet rivalry in Indochina, developments in Thai foreign policy and in that of Malaysia, and the prospects for regional neutralization and economic cooperation and organization. The concluding chapter raises the issue of "two Southeast Asias"—Indochina on the one hand and the Association of Southeast Asian Nations on the other—and their relations with outside powers. Pertinent international documents are included in seven appendices.

The central topic is described by the author as "Great Power rivalry [i.e., between the United States and the Soviet Union] for a sphere of influence in Southeast Asia and the response of the regional states to external intervention. . ." (p. x). In fact, however, the study is broader than this in that substantial attention is devoted both to the roles of India and China in the region and to the increasingly important bilateral and multilateral relations within the region itself.

Although this is a timely book on a topic of continuing importance, it is out of balance in a number of instances. For example, the reader receives little information about U.S. purposes in Vietnam or why failure occurred. On the other hand, the study offers more than enough detail about Sukarno's "Crush Malaysia" campaign and other aspects of Indonesia's foreign policy in the 1960s prior to the fall of Sukarno.

In a more general sense the dominant focus of the book overall is the 1950s and, particularly, the 1960s with rather skimpy coverage of more recent events. This criticism notwithstanding, Singh's attempt to take a long-range view, a view which is frequently not taken by the general public, the practitioner, or even the academician, is most helpful in placing the present into a meaningful context. Moreover, the discussions of particular aspects of this historical context are generally succinct and quite good. Examples of these are the author's treatment of the Soviet perception of Indochina in the immediate post-World War II period, the context and explanations for Singapore's inclusion and then later expulsion from Malaysia, the reasons behind Thailand's Northeast "problem," and the issues involved in the Sino-Soviet rivalry in Indochina.

Although this study offers little new and compelling for the technician, it nevertheless is a useful and competent wide-ranging treatment of the major issues involved in the international affairs of the Southeast Asian states. Because of this, it should receive substantial attention.

JOSEPH W. DODD

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The Joint Organizations of Canada and the United States. By William R. Willoughby. (Buffalo, N.Y.: University of Toronto Press, 1979. Pp. xi + 289. $25.00.)

In his research extending over more than three decades, William R. Willoughby has established himself as an astute and prolific scholar of Canadian-American relations and he continues his valuable contributions to the literature in this book. Employing a rich variety of sources including government documents, archival materials, and personal interviews and communications, Willoughby has constructed a record of the unique practice of Canada and the United States to use joint organizations to deal with myriad policy issues transcending the international boundary.

Willoughby's account is a sharp descriptive overview of the evolution, responsibilities, problems, successes, and failures of these joint organizations. He does not hesitate to offer praise or criticism where appropriate, both to the organizations and to the governments. Although other scholars have examined these joint organizations, Willoughby has effectively considered the important lessons and activities of each one and has probed the relationship of the organizations to the existing condition of general Canadian-American relations.

He emphasizes that although the joint organizations have made important contributions to the facilitation of bi-national relations, they are a supportive method of communication and problem solving between the two governments on the issue, but not the sole method. Some of the organizations, especially the more technically oriented ones such as the International Joint Commission or the various fishery commissions, are very active, but others, such as the Ministerial Committee on Trade and Economics, have fallen into disuse. Recently both governments have on occasion adopted a policy of using ad hoc committees or discussions to deal with certain issues that might properly be referred to one of the joint organizations. Willoughby asserts that this development does not represent a vote of no confidence in the joint organizations but rather a recognition that certain pressing issues (especially those involving matters of high policy) may be more effectively handled by direct and focused diplomacy.

It is clear from Willoughby's account that the joint organizations have always functioned within an existing political environment. An aspect of this environment is the conviction by the leaders of both states that the bilateral issues should be resolved as harmoniously as possible and to the mutual benefit and accommodation of both states. Another is the awareness that among the two states there are substantial differences in economic capacity, military power, and international responsibilities that inevitably have an impact on their bilateral relations. Because of this disparity in power and capacity, Canadians have always been alert to the danger of being consumed or overwhelmed by a well-meaning but likely insensitive neighbor. Consequently, Canadian participation in joint organizations has frequently involved a delicate balancing act of promoting mutual cooperation and accommodation and at the same time seeking to protect Canadian sovereignty, identity and independence. The controversy over Canadian participation in NORAD is an example.

It is equally clear that the use of the joint organizations has not served to resolve all of the controversy or differences surrounding a particular issue. Indeed, agreement within the joint organizations has occasionally been followed by sharp disagreement. In addition, the Canadians have sometimes expressed their unhappiness with unilateral American action in situations wherein bilateral consultations through joint organizations would appear to be warranted and expected.

Although specialists in Canadian-American relations will be familiar with the various joint organizations that Willoughby studies, they will be rewarded by his thorough research and clear assessments. Non-specialists who are interested in the types of institutional arrangements that states can create to deal with mutual problems should place Willloughby's book at the top of their reading list.

DON C. PIPER

University of Maryland

Compliance and Public Authority: A Theory with International Applications. By Oran R. Young. (Baltimore: Johns Hopkins University Press, 1979. Pp. x + 172. $12.95.)

This book is important because it tackles one of the most fundamental questions confronting policy makers in international politics: can compliance with international agreements be obtained in decentralized social systems with no central authority? Oran Young questions the related arguments that the major problem with the international system is the absence of centralized institutions of government and that international law is not really law because of the absence of formalized enforcement mechanisms. Based on an analysis of two case studies, the Partial Test Ban Treaty (PTB) of 1963, and the International North Pacific Fisheries Convention (INPFC) of 1953, Young's conclusion is clear: despite much past thinking, compliance can be gained in decentralized systems. Compliance with the PTB shows an "impressive record" (p. 62) and has been "excellent to date" (p. 66), and for the INPFC, compliance with the prescriptions "is relatively high on the part of all Contracting Parties" (p. 87).

Although Young demonstrates that there has been compliance in both cases, the strength of his conclusion is facilitated only by his overlooking several problems. First, Young overemphasizes the importance of any manifestation of compliance behavior. The lack of centralized institutions, however, has rarely been equated with total anarchy; few have contended that compliance with either written or tacit agreements is totally absent from the international system.

In addition, although Young's two case studies provide convincing illustrations of compliance among signatories, they also provide ample instances of noncompliance. We find, for instance, that the "general objectives" of the preamble of the PTB have not been fulfilled, that there have been large-scale increases in underground testing (p. 59), and that one authority has concluded that the post-PTB system is "what many believe is a nuclear-weapons development effort that is indistinguishable in scope—if it is not even larger—from the one that had been under way before the treaty" (p. 59). Among the nations that did not sign the treaty, France and China alone conducted 53 nuclear explosions during the 1963-1973 period, 45 of them in the atmosphere. As of 1974, about half of the 1,012 nuclear explosions since 1945 were carried out after the PTB (p. 59).

In the case of the INPFC, too, we find conflicting evidence of compliance. Within the strict confines of the convention itself, there are many instances of compliance among the three signatories (the United States, Canada, and Japan), who were until recently the only nations with sufficient mutual fishing interests to warrant a convention and compliance with it. However, the recent growth of high seas fishing operations in the North Pacific by noncontracting parties (the USSR, Korea, and Poland) "has produced serious difficulty for the INPFC regime. . ." (p. 69). In short, we do not know whether to call the glass half empty or half full. Young ultimately opts for the more optimistic view, but the evidence seems equally consistent with the opposite conclusion.

Indeed, on the last page of his case study, Young reveals that the INPFC regime is actually "collapsing under the weight of several factors" (p. 93), including the expanding operations of noncontracting states, and the extension of coastal state jurisdiction over marine fisheries. Although the convention itself does not preclude the inclusion of these other states, according to Young, "it has not proven politically feasible to move in this direction" (p. 69). He does not speculate on why this is so, but one guess is that the noncontracting parties have not joined because it is not in their interest to join. In other words, compliance may be related simply to the individual interests of the concerned parties—an unfortunate defect in the convention, but one that nevertheless illustrates the special character of the international system.

Thus, member nations have strong incentives to avoid violating the provisions of the PTB, because compliance does not prevent them fro pursuing most of their key objectives. Violations that do occur, moreover, are discernible even without inspection. Although compliance is costly to INPFC signatories, nonetheless, Young notes, with respect to the clauses on conservation, that "these parties would have compelling reasons to formulate and carry out similar programs of conservation even if the INPFC did not exist" (p. 75).

This book paints a correctly complex picture of compliance, and although emphasis here has been on the two case studies, the book is not limited to these cases. The two other sections consist of lengthy elaborations of definitions, concepts, assumptions about behavior, and scenarios describing potential compliance behavior under various conditions. However, this conceptual apparatus seems to be virtually divorced from the case studies: no reference to either case can be found outside of their respective chapters. Moreover, while thorough and detailed, it is, contrary to the subtitle, not so much a theory in anything approaching the formal sense, but rather the conceptual apparatus necessary to begin working on a theory of compliance. But any work on complliance, especially related to the international system, must not view any evidence of compliant behavior as a major breakthrough, and would ideally try to establish some criteria of the importance of the compliance behavior that does occur. Young's work is rich and complex, and goes much beyond the mostly normative and simplistic analyses of compliance in international relations that have been produced in the past.

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Statement of Ownership, Management and Circulation

Required by Act of October 23, 1962:
Section 4369, Title 39, United States Code

1. **Date of Filing:** October 23, 1980.
2. **Title of Publication:** The American Political Science Review
3. **Frequency of Issue:** Quarterly in March, June, September, December
4. **Location of Known Office of Publication:**
   1527 New Hampshire Avenue, N.W., Washington, D.C. 20036
5. **Location of the Headquarters or General Business Offices of the Publishers:**
   1527 New Hampshire Avenue, N.W., Washington, D.C. 20036
6. **Names and Addresses of Publisher and Managing Editor:**
   Publisher: American Political Science Association,
   1527 New Hampshire Avenue, N.W., Washington, D.C. 20036

   Managing Editor: Charles O. Jones,
   Department of Political Science, University of Pittsburgh, Pittsburgh, Pennsylvania 15260
7. **Owner:**
   American Political Science Association, 1527 New Hampshire Avenue, N.W.,
   Washington, D.C. 20036
8. **Known Bondholders, Mortgagees, etc.:** None
9. The purpose, function and nonprofit status of this organization and the exempt status for federal income tax purposes have not changed during the preceding 12 months.
10. **Extent and Nature of Circulation:**

	Average No. Copies Each Issue During Preceding 12 Months	Actual No. Copies Single Issue Nearest to Filing Date
A. Total Number Copies Printed	14,000	14,000
B. Paid Circulation		
1. Sales	65	65
2. Mail Subscriptions	12,250	12,250
C. Total Paid Circulation	12,315	12,315
D. Free Distribution by Mail, Carrier or Other Means	59	59
E. Total Distribution (C + D)	12,374	12,374
F. Office Use, Left-Over, Unaccounted, Spoiled after Printing	1,626	1,626
G. Total	14,000	14,000

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1979, 262 pp., illus. Paperbound 03103 $14.50

Prices quoted in U.S. dollars. Outside U.S.A. prices may vary somewhat from those listed, reflecting distribution costs and currency fluctuations. Prices are subject to change without notice.

SCIENCE and ETHICAL RESPONSIBILITY
Based on the First U.S. Student Pugwash Conference, held at the University of California, San Diego, June 19–26, 1979

Edited by Sanford A. Lakoff, [illegible]
[illegible]
With [illegible] Jeffrey R. Leifer, Ronald J. [illegible]
Eric Markusen, [illegible]
Foreword by Bernard T. Feld, [illegible]
[illegible]

A collection of addresses and papers by eminent scientists and scholars as well as undergraduates and graduate students addressing the general issue of the ethical responsibility of scientists in a variety of practical contexts, including arms control, bio-medical research, development assistance, and the role of scientists and engineers in political controversies. A select bibliography by the editors is included.

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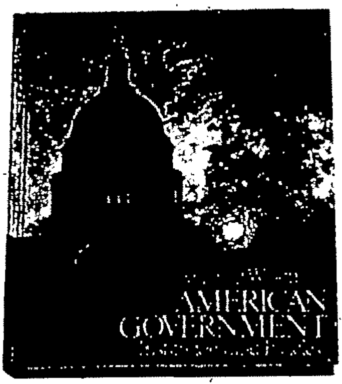

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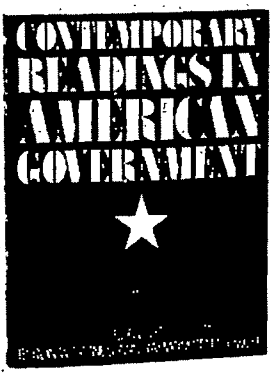

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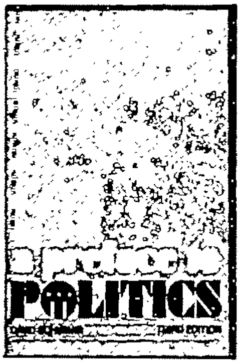

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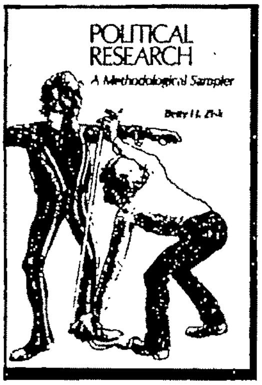

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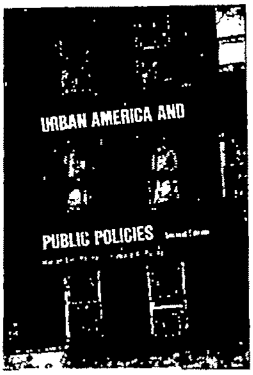

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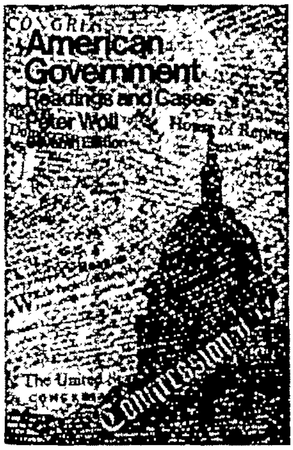

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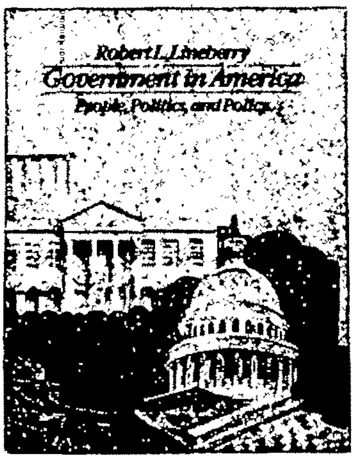
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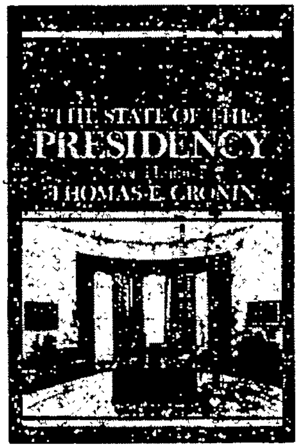
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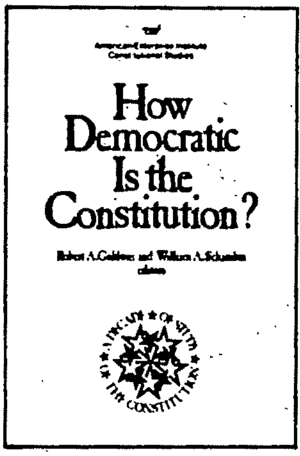
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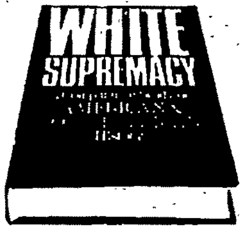

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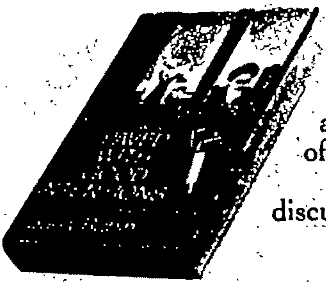

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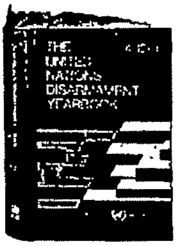

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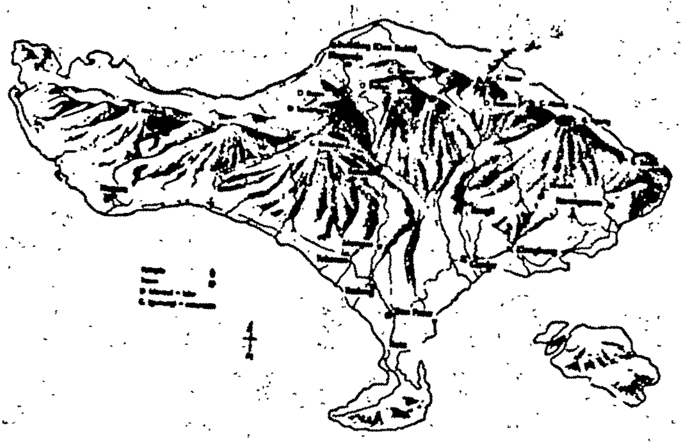

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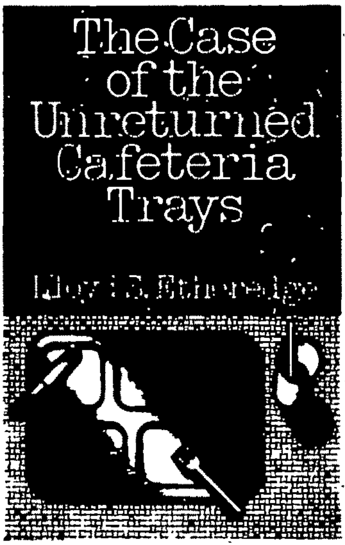
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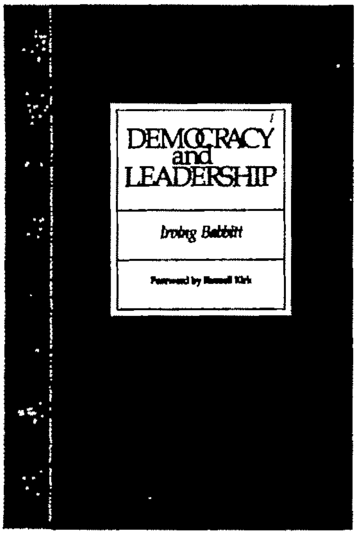

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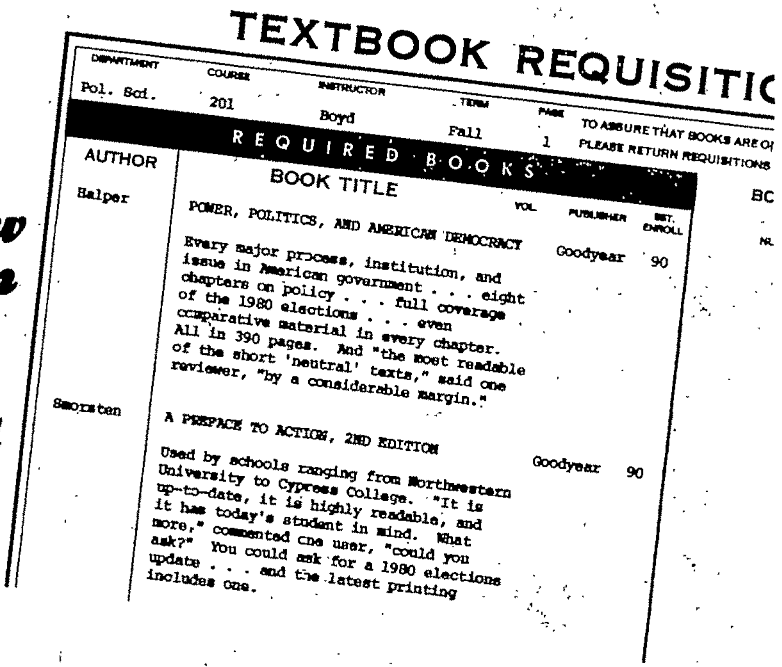

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Second class postage paid at Washington, D.C., and at additional mailing offices.
Printed in the United States of America by George Banta Company, Inc., Menasha, Wisconsin.
Composition by TypoGraphics, Columbia, Maryland.
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Current issues are priced at $20.00 per copy; for back issues prior to 1970 address Walter J. Johnson, Inc., 355 Chestnut St., Norwood, New Jersey 07648; for issues 1970 and later send request directly to the American Political Science Association.

Applications for membership, orders for the *Review,* and remittances should be addressed to the Executive Director, The American Political Science Association, 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036. Notices of change of address should be received in the Washington office by the 25th day of the months before publication.

INDEXING: Articles and notes appearing in the *Review* before the June 1953 issues were indexed in *The Reader's Guide to Periodical Literature.* Current issues are indexed by The International Political Science Abstracts, the *United States Political Science Documents,* and the *Social Sciences and Humanities Index.* Microfilm of the *Review,* beginning with Volume 1, may be obtained from University Microfilms, 313 North First Street, Ann Arbor, Michigan 48106. A *Cumulative Index of the Review,* Volumes 1-62; 1906-1968, may also be obtained from University Microfilms. Articles appearing in the *Review* are listed regularly in *ABC Pol Sci* and *Current Contents: Behavioral, Social & Management Sciences.* Book reviews are indexed in *Book Review Index.*

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Machiavelli's Political Science

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Machiavelli is presented as the founder of modern political science, with due regard to the fact that he never spoke of "political science." His usage of "prudence" and "art" in The Prince *is examined to see whether, as founder, he was a teacher or a ruler of future generations. His comprehensive attack on classical political science is outlined and developed through two essential points, the cycle and the soul.*

Modern political science presents itself today as both narrowing and progressive. Despite certain misgivings arising from the encounter with the New Left, most political scientists still put their trust in the fact-value distinction[1] as the method designed to narrow their range of concern and thereby to bring social as well as scientific progress. To an observer, the narrowing might seem more evident than the progress. At least it must be admitted that scientific narrowing has lost its evident connection to progress, since the very meaning of the fact-value distinction is that any good that might come of it is strictly accidental. To understand modern political science, therefore, one should look back to a time when this connection was argued in comprehensive fashion. Before investigating, one cannot exclude the possibility that this comprehensive argument was conceived, not in gradual stages, but *ad uno tratto* ("with one stroke") in the thought of *uno solo* ("one alone"). To set forth this claim on behalf of Machiavelli, with a view to our own self-understanding, is the purpose of this article.

Machiavelli's Modernity

We will not find the fact-value distinction in Machiavelli, for he passes value judgments right and left with unmethodical abandon. But we do find a realism that was ancestor or parent of the fact-value distinction in Machiavelli's famous call, in chapter 15 of *The Prince,* not to depart from what is done for what ought to be done. As opposed to making a profession of good in all regards, one should align one's values with facts in the sense of deeds. So the distinction is between deeds and professions rather than between facts and values, and the lesson is for the good of men, not for a methodological purity that cannot be proved to be for the good of anybody. For the sake of one's preservation (which is good), even for the common benefit of each human being, one must learn how to be not good. With this promise of preservation, Machiavelli connects his political science to progress toward the human good.

What one's preservation requires, according to Machiavelli, will become clear only gradually, but it may be glimpsed behind the meaning today of "modern," as for example in the phrase "modern political science." "Modern" as we use it today is defined against "traditional" so that what is modern constitutes self-conscious progress beyond tradition. But when modernity is established and what is "modern" becomes traditional, modernity must define itself against what had once bravely claimed to be modern. Thus, "modern" is always in danger of being surpassed by "more modern"; defined as against the traditional—that is, in relation to the traditional—"modern" seems to have no definition. Nor does "tradition": "traditional" is or supports the status quo, and "modern" advances—or merely moves—beyond it. "Modern," then, has perhaps a moving definition; it is always ahead of itself, not to say self-destructive. Or one might say that modernity has a certain direction.

In politics, more modern means further left and more democratic, as we can see in the alleged progression of the great modern revolutions—American, French, Russian, Chinese—each more democratic, supposedly, than the preceding one. What is more democratic, it is assumed, disposes of barriers between men, both humanly created and otherwise, as far as possible. Such barriers are especially the forms or formalities that define distances between men, give society a structure, and keep it from dissolving into an undifferentiated mass. Thus, modern art and literature have shown an inherent tendency away from classicism in perfection of form toward romanticism in concern for feeling that overrides form. Similarly, we have learned from Tocqueville that modern politics as a whole may be seen as the drive of extra-constitutional democratic forces to overcome the restraints of constitutional forms that were once

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My thanks to Astrid Deeth, Susan Esser and Russell Price for help with the manuscript of this article.

[1] That science can establish facts but not values.

modern themselves.[2] The liberalism that was to set men free has been attacked and pushed aside by movements let loose by those dissatisfied with liberal formalism, who desired a more radical freedom: for the decline in respect for forms and formalities is accompanied by a demand for more subjectivity and creativity.

Such, in brief, is the course of the modern world as it races toward perfect democracy and freedom. But this sort of perfection is indefinable; as soon as one defines the modern, it becomes the status quo and hence traditional, an easy target for the next progressive. To understand modernity, therefore, one cannot look to its *end,* as it seems to have none; one must look to its *beginning,* when progress was first set in motion. Especially since modernity now seems out of our control, and "progress" no longer seems progressive, we need to know what was intended and hoped for originally.

When one looks to the periods at which modern history is said to begin, however, they do not appear to suggest, much less to launch, the characteristic dynamism of modernity. Humanism puts man, rather than God, at the center of attention—which does not necessarily imply progress toward a new earthly future for men; and the Renaissance is a rebirth, perhaps a return to the ancients. Only Machiavelli, a single man soaked in the Renaissance and steeped in Humanism, seems, of those in his times, to have declared himself for progress in terms we recognize.[3]

More precisely, Machiavelli is for novelty. In *The Prince,* he praises the new prince over the hereditary prince because the new prince depends only on himself and thus gains more glory; the highest case is the new prince in a new principality who is also the prophet of a new religion (*The Prince* 6, 24). And at the beginning of the *Discourses on Livy,* Machiavelli, comparing himself to those in his times who sought unknown seas and lands, says he is bringing "new modes and orders" to mankind. He appears to be the first political philosopher not merely to admit his own novelty (as for example Marsilius) but even to flaunt it; and he did this not merely to establish his new modes and orders but, in accordance with them, to give new reputation to those who seek out new acquisitions.[4] It is not that Machiavelli invented new political tricks. He admits to having borrowed techniques of government from the ancients and perhaps also from Christianity, and he was aware that before him there had always been half-hearted or untaught Machiavellians in need of instruction.[5] Rather, he believed that political men should be encouraged to make their own innovations so as to increase opportunities for glory and gain for themselves and their peoples, for "preservation" requires both glory and gain. The founder of a state does not legislate once in the hope that his forms will endure; instead, the healthy state must be made and remade, formed and reformed, or it will become corrupt.[6] Machiavelli is far from espousing a formless or stateless society, but he favors frequent reform and he is definitely no respecter of formalities.

Machiavelli's realism unites with his desire for innovation when one sees that learning how to be not good means especially learning how to introduce an innovation.[7] In the chapter of *The Prince* where he calls for learning how to be not good, he announces publicly that he departs in this "from the orders of others." Machiavelli thought he lived in times when men were weak and vile (at least in Italy); he spoke scornfully of modern politics and religion and by contrast appealed to "ancient virtue."[8] Such expressions might appear to put Machiavelli with the Renaissance and to demand a return to the ancients. But Machiavelli, we see, was very far from being reactionary. He rejects the authority of the ancients in this chapter, for they, together with the Christians, were the "others" who based their political science on what should be done rather than on what is done, who elaborated the "profession of good" in all regards, and who therefore constructed imaginary republics and principalities such as Plato's *Republic* and St. Augustine's *City of God.*

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[2] Tocqueville (1969, p. 698). Consider the criticism of liberal formalism in Karl Marx, "On the Jewish Question," in Marx (1975, pp. 219, 234).

[3] See n. 55 below and *The Prince* 12 (*progressi grandissimi*). Two recent works emphasizing Machiavelli's republicanism have lost sight of his progressivism. In Pocock (1975, pp. 158-77, 218), Machiavelli's concern with innovation is given due emphasis with regard to *The Prince,* but denied with regard to the *Discourses;* and see Skinner (1978, Vol. 1, pp. xiv, 45, 179; cf. Vol. 1, p. 181). See also Gerschmann (1973, p. 175).

[4] Thus, although new acquisitions are useful to princes and peoples, they must mainly appear glorious in order to attract glory-seekers. Machiavelli would have found utilitarianism too staid to be useful. Compare what he says about *cose nuove* in *The Prince* 6 with his remark on *cose presenti* in *The Prince* 24; and note *innovare con nuovi modi li ordini antiqui* in *The Prince* 7. Marsilius of Padua, *Defensor Pacis* I.I.3; I.XIX.3.

[5] See especially *Discourses on Livy* I.27 on *knowing* how to be altogether bad or good. On Machiavelli's borrowing from the ancients, see *The Prince* 3 (praise of Romans), 6 (beg.), 19 (end); *Discourses* I, proemium, 2, 4, 5, 21; II.16, 24, 33; III.2. On his possible borrowing from Christianity, consider *The Prince* 7 (on "Remirro de Orco"); *Discourses* I.52; II.16-18.

[6] *Discourses* I.6, 16-20; III.1, 3, 7. Note the progression from *The Prince* 4 (end) to *The Prince* 6 (on form and matter) to *The Prince* 25 (end). But refounding "every day" would be offensive, *The Prince* 8.

[7] *The Prince* 8 (end), 17, 18, 19 (end); *Discourses* III.35.

[8] *Discourses* I. proemium, 12, 55; II.2; III.1; *The Prince* 3, 12, 26; *Art of War,* proemium.

In regard to political teaching, the strong ancients are at one with the weak moderns. So in the preface to the first book of the *Discourses on Livy,* Machiavelli indicates that he will honor ancient politics through ancient histories, not by imitating ancient political science. Moreover, the history he chooses is Roman, not Greek; and near the beginning of the *Discourses on Livy* he reveals a definite preference for Rome over Sparta, the Greek city most favored by political philosophers. While making use of Polybius and Livy, he ignores Polybius' statement of indebtedness to Greek political science; and his use of Livy, of which much has recently been said, does not indicate acceptance of Livy's interpretations of events, to put it mildly.[9] Machiavelli puts his own interpretation on ancient virtue so that it becomes Machiavellian *virtù.* He values the large and imperialistic Roman republic above the Greek cities, and judges its virtue by its fortune in war, although that virtue was exercised in conquering the Greek cities among others.[10] While bowing ironically to the authority of the ancients—so that he can use it against the authority of the moderns—Machiavelli in fact uses ancient examples to reproach ancient teachings. He returns to the ancients in order to improve on them.

Unless one dissolves Machiavelli's arguments into phrases and reduces his design to vulgar office-seeking, one cannot find another thinker or statesman in his times or before who reminds us so vividly and profoundly of the realism and dynamism of modernity. Today, many would perhaps agree with this judgment, if not this formulation. But there remains a great reluctance to admit, or even consider, that Machiavelli might be chiefly responsible for the spirit of modernity and thus is himself the origin of the modern world. Modernity now seems so powerful and all-encompassing that it appears to be unstoppable, and if unstoppable, apparently inevitable. Few are ready to believe that the modern world, which wishes to move in the direction of perfect freedom for men and to give men ever greater control over themselves and the world, could have been founded by the free act of a human being. Strangely, we find it more comfortable to believe in confused and contradictory "forces of history," relishing our fate since we cannot maintain our hopes.

Before estimating what Machiavelli's influence might have been, one must see what he intended and what influence he intended for himself. One may begin from a typical view of Machiavelli's realism, which is that he believed that morality can be one's guide in private affairs but not in politics. Such a view seems implied in a remark of Thomas Hobbes', apparently directed at Machiavelli: "Successful wickedness hath obtained the name virtue . . . when it is for the getting of a kingdom" (*Leviathan,* Ch. 15). It is supported in Machiavelli's *Discourses* I.9, where Romulus' killing of his brother is excused because it was necessary for him to rule alone to order a kingdom or a republic for the common good.[11]

Yet, returning to chapter 15 of *The Prince,* we find that the title mentions "men and especially princes," while the principal advice is addressed to "whoever understands." Actual princes may be imprudent; indeed, according to Machiavelli, their sins are responsible for Italy's plight.[12] They must be replaced by prudent princes, who are now private men. But a private man must behave like a prince because a private man, if he is prudent, must become a prince—as Hobbes' remark suggests. Machiavelli's own suggestion, in his punning use of the word *privato,* is that a private man should regard himself as *deprived* of office.[13] Perhaps the rules of politics are not those of private morality, but when private men are compelled to become princes, they no more than princes can live by professions of good. Moreover, the prudent, private man who wants to become prince must not only be ready to make his way by such crimes as fratricide; he must also pave the way by making it easier for such crimes to be accepted as necessary. In *Discourses* I.10, just after providing an excuse for Romulus, Machiavelli shows how a founder is affected by his reputation, in particular how he can be hampered by an evil reputation as a tyrant. Machiavelli may have excused Romulus, but others—for example, St. Augustine—have not.[14]

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[9] *Discourses* I.5-6. Polybius VI.5.1; Livy IX.36.3; XXXIX.8.3. Strauss (1958, Ch. 3); cf. Gilbert (1977, pp. 115-33).

[10] *Discourses* I.9, 53, 59; II.1, 10; III.16; *The Prince* 3.

[11] Cf. *Discourses* I.18 (end), where Romulus' killing is excused without reference to the common good. See *Florentine Histories* I.3, regarding Attila, who also killed his brother in order to "be alone in the kingdom."

[12] *The Prince* 12; *Discourses* I.21; II.18; III.29.

[13] *The Prince* 2, 6, 7, 8, 14; *Discourses* III.2, 5. Note also the phrase *privata fortuna* in *The Prince* 6, 7, 12, 14. The "private citizen" who must seek office must also have the princely "humor" (*The Prince* 9) to be sure; but this fact does not justify the private lives of those who share the popular humor, who as such are deluded. See Price (1977, p. 620).

Thus, the prudent prince needs a whole new climate of political and moral opinion to facilitate his arrival and maintenance in power. This is what Machiavelli intends to supply. The prince, or founder-prince, "is alone" only if he alone sets the standards by which he is judged and his reputation is made. Only he who sets these standards is alone above ordinary moral persons who live by them and above princes who may share them and who must conform to them, or appear to conform, for the sake of reputation. The highest prince, who is in the fullest sense prince, is the moral or political philospher who establishes the opinions in which lesser princes operate.[15] If, according to Machiavelli, public and private immorality must be controlled by the political necessity of acquiring and maintaining a state, then the highest prince is the political philosopher or scientist, Machiavelli himself—the one who brings "new modes and orders" for the common benefit not merely of Florentines or Italians but also of everyone.

Prudence and the Art of War

In this prospectus for "new modes and orders" establishing a new climate for prudent princes and private men, however, there is an evident difficulty. Is the end of the political scientist to pave the way for princes (thus also improving the lot of peoples), or, since the political scientist too is in a sense a prince, is his end to be prince himself? In the former case, Machiavelli's political science would be teachable to all and his status as teacher of all for the common benefit would be superior to his ambition of ruling as "one alone." He would indeed be teacher rather than prince, for his discoveries, like those of any scientist or philosopher, would not carry along to present and future beneficiaries the personal rule of their discoverer. The scientist, as teacher, is not strictly "one alone," and if he discovers "new modes and orders," his glory is merely to have been the first to see, and not now to be the first in rule, as prince.

Yet this formal truth regarding teaching in general seems overborne by the content of Machiavelli's particular teaching. When the teacher's lesson requires rising to sole rule, it seems unreasonable that the teacher should except himself, and unlikely that he will. In this case, his status as teacher would be subordinate to his ambition as prince, and the prudence he exercises in his own interest would control the science or art that he claims to be in the common interest. How can one sincerely advise "successful wickedness"?[16] If wickedness succeeds for those advised, then why not also for the adviser? And if the advice is wicked, why should it help those advised? Although Machiavelli's difficulty can be sensed by anyone of ordinary moral experience, it is obscured by scholars who make excuses for Machiavelli, conceal the wickedness of his advice and thus blindly rob him of the glory he claimed for having begun the scholarly practice of making such excuses. To excuse Machiavelli is to dismiss not only every popular, but also every interesting, sense of the word "Machiavellian."

In the dedicatory letter to *The Prince,* which was written to be understood "in a very brief time" by an actual prince, this difficulty of Machiavelli's political science appears in the relation between art and prudence, and the word "science" does not occur. Indeed, for some reason Machiavelli does not speak at all of "political science." "Science" is discussed in the *Discourses* (III.39), which were addressed to potential princes with more time on their hands. In this summary treatment, we shall mainly consider *The Prince.*

Machiavelli's dedicatory letter offers to Lorenzo his "knowledge of the actions of great men," which is the gift of everything he knows. From this he dares to "discuss and give rules for the government of princes" and says he possesses knowledge of the nature of princes. What he knows of the government of princes seems to be practical, and what he knows of the nature of princes, theoretical. These two kinds of knowledge appear to combine in the third chapter, where "the Romans" are cited as the example for wise princes of overcoming the particular difficulties of acquiring and maintaining a new principality. The Romans knew the remedies for these difficulties because they knew them "at a distance" (*discosto*) or saw ahead.[17] Such knowledge is likened to that of physicians taking timely measures to cure consumption, but is then limited to "a prudent one," as opposed to "the wise men of our times," and said to be "given" only to that one. The same foresight is recommended in the sixth chapter to "a prudent man"; he should imitate the great men who have not imitated anybody and have made altogether new principalities. He should behave like "prudent archers" who shoot at a mark above the target in order to hit the target. Where one would expect "skillful archers" one finds "prudent archers," as if prudence, understood as discounting ahead of time, were assimilated to art and art thereby given command over the future.[18] He who follows this procedure is said first to have "some odor" of the great men he imitates, and then, because archers know the virtue of their bows, and use the aid of the high mark, they are allowed to "succeed in their design." In a turn typical of Machiavelli's writing, the (singular) prudent man whose prudence is "given" to him becomes the (plural) prudent archers whose learned prudence supplies the lack of highest virtue. We are made aware of, but not directed to, the difference between the prudence of imitating great men and prudence which is similar to possessing an art. When imitating great men, one follows their beaten track and thus does not truly imitate their innovation; however, each archer, as such, is as "prudent" as any.

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[14] St. Augustine, *De civitate Dei* XV.5.

[15] One cannot create opinions altogether, since ordinary morality remains and will remain unaffected even by Machiavelli's instructions; see Mansfield (1979), on *Discourses* I.10. In *The Prince* 15, "the orders of others" from which Machiavelli departs refer to qualities for which men are praised and blamed. See also *Discourses* I, proemium; *Florentine Histories,* proemium.

[16] Or, how can one be a "teacher of evil"? Strauss (1958, p. 9). Lefort (1972, pp. 260-62) has read past the first sentence in Strauss' book and has taken note of the "if" in that sentence.

[17] See also *Florentine Histories* VII.5.

Machiavelli alludes to this difference at the end of the ninth chapter when he remarks that "a wise prince" must think of a "mode" through which his citizens will always have need of him and thus will always be faithful to him. Can a wise prince do this with political science? At the end of the tenth chapter, Machiavelli says he has solved the problem of keeping citizens loyal, at least in part, for "a prudent prince." In the first part of *The Prince* (Chs. 1-11), on the kinds of principalities and how to acquire them, he joins theory and practice or wisdom and prudence, while leaving unclear whether they must be given as prudence or may be learned as an art.

The second part of *The Prince* (Chs. 12-14), on the necessity of using "one's own arms," heightens the difficulty. Machiavelli condemns the use of mercenary arms but praises a certain mercenary captain in the service of the Florentines, Paulo Vitelli, as *uomo prudentissimo* for having risen from private fortune to very great reputation. If he had taken Pisa for the Florentines, Machiavelli remarks, the Florentines either would have had no recourse had he left them or would have had to obey him. Machiavelli does not remark that in fact this "very prudent man" did not take Pisa and was thereupon killed by the Florentines. They spied the danger to them in Vitelli's prudence, since "mercenary arms" to them were "his own arms" to him. But they also did not know how to take advantage of his prudence, such as it was. In chapter 13 Machiavelli deplores "the lack of prudence in men" by contrast to the wisdom which is given to the few, and he criticizes the Romans whose prudence he had praised in chapter 3. Among the few wise who order their own arms, he names Philip of Macedon, whom he had cited in the preceding chapter as a mercenary captain who took away the liberty of his employers. Machiavelli says he gives himself over entirely to Philip's (and to others') orders. Does Machiavelli then admit that the few wise, perhaps including himself, hire out their wisdom and rob their employers in the manner of mercenary captains?

Machiavelli answers this question provisionally in the fourteenth chapter of *The Prince*. Although the chapter heading does not mention the art of war, Machiavelli suddenly announces that the art of war should "therefore" be a prince's only object and only thought.[19] Whence the "therefore"? If the dubious mercenary captain named at the end of chapter 13, to whose orders Machiavelli gives himself over entirely, can be presented as teaching an *art*, then his wisdom can be made to appear as benefaction. Such is the virtue of this art that it not only maintains those born princes but also often causes men to rise from private fortune to the rank of prince. When princes have thought more of delicacies (*delicatezze*) than of arms, they have lost their state. Using a weighty phrase, Machiavelli says that the "first cause" of losing one's state is to neglect the art of war, and that the cause of gaining it is to be "professed" in the art.[20]

This is illustrated by Francesco Sforza, who through being armed became Duke of Milan, and by his sons, who through avoiding arms became private men.[21] The example of the Sforzas does not in truth illustrate the principle because it fails to mention the art of war; besides, Francesco, however proficient in that art, did not "profess" it in the sense of teaching it. Nonetheless, as we have seen, a mercenary captain who fights on all sides for hire and gains his own personal "state" illustrates our problem of art and prudence. Is his art so powerful as to be the first cause of his rise or does he also need his prudence to attune his art to circumstances? In this statement, the art of war has swallowed up prudence. Machiavelli goes on to say that no proportion exists between one who is armed and one unarmed, and that it is not reasonable for the armed to obey willingly whoever is unarmed. Thus, Machiavelli concludes, a prince who understands nothing of militia, among all his other misfortunes, is not trusted by his soldiers. He seems clearly to deny here the fundamental principle of classical natural right, the rule of the wise: it is not reasonable for someone who is unwise but armed to obey one who is wise but unarmed. Yet if the art of war is understood in its full extent as the only object and thought of a prince, Machiavelli in truth affirms the classical principle, while improving on it. For to understand "militia" or to possess the art of war is sufficient to make one armed, and to be armed is sufficient to make oneself obeyed, at least by one's soldiers and perhaps also by one's subjects.[22] Contrary to the classical writers, Machiavelli argues that knowing leads to commanding, and so the art of war in the extended sense includes politics. Nowhere else does Machiavelli give the art of war such amplitude.[23]

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[18] Aristotle, *Nicomachean Ethics* 1139b7-8, 1140a18.

[19] *Quella è sola arte che si espetta a chi comanda:* the article before *sola* is missing perhaps because Machiavelli is at a loss between the definite and the indefinite. Russo's suggestion, assigning *sola* to *chi comanda,* does not make sense; Machiavelli (1963, p. 125n.).

[20] Machiavelli claims to be "professed" in the art of war in *Art of War,* proemium. See also *Discourses* III.13. He does not claim to be a philosopher (*Discourses* I.56) and characteristically uses weighty philosophical phrases only in political contexts.

[21] Elsewhere Sforza is scolded by Machiavelli for having built a fortress which caused his heirs to lose their state; *The Prince* 20; *Discourses* II.24.

Moreover, he says next that a prince (or one) must never lift his thought from the exercise of war, and in peace more than in war. Never?[24] Does this mean that thought (or at least a prince's thinking) should never be detached from advantage in war? Does "effectual truth" mean effectual in war, whether foreign or domestic, and is this the only truth one should seek? Machiavelli's elaboration of this startling "never" seems to take away from its naked force, but only at first. He says that the exercise of war in peace is done in two modes, with works (*opere*) and mind (*mente*). But apart from keeping one's own arms in order, he says that a prince should always be on the chase so that he can accustom his body to hardships and learn the nature of sites. Machiavelli then discusses this topographical knowledge, of which one should take "the greatest care." The "works" of exercising war in peace seem hardly distinct from "mind."

Knowledge of sites is useful because it enables one to understand what is required for self-defense and because the topographical features in one region have a "certain similitude" to those in other regions. Knowledge of one site thus enables one to comprehend any other site. Such knowledge, the "first part" of a captain, is accompanied by reasoning, as Machiavelli makes clear by the example of Philopoemen, a prince who used to ride in the country with his friends and pose hypothetical questions to them concerning all the chances (*casi*) an army might encounter. With these "continuous cogitations," no accident could ever arise for which Philopoemen did not have the remedy: again, a sweeping claim of efficacy for the art of war.[25]

This "first part" of a captain seems equivalent to the "firm science" Machiavelli discusses in *Discourses* III.39, also on the knowledge of sites, where he also praises hunting.[26] He says that Xenophon (in his work that Machiavelli mistitles "Life of Cyrus") makes it clear that hunting is "an image of a war"; and Machiavelli adds that for great men this exercise is honorable and necessary. It appears that particular knowledge of one country can be generalized and the familiar made applicable to the new by the use of images representing the similitudes mentioned in chapter 14 of *The Prince*. By contrast to that chapter, *Discourses* III.39 speaks of both images and sciences so as to suggest that images (in addition to similitudes) are necessary to science (as beyond mere art). It was "firm science" that on one occasion enabled a Roman tribune to save the Roman army, despite the bafflement of the consul in charge, by spying a summit above the enemy on which both he and that army could take refuge. An image, then, might be a similitude visible only to one person or to a few, for sometimes it takes a rare brain to see an invisible similitude in a visible one; and the use of images would make possible the "perfect possession" of a science and therewith the rapid comprehension of new things. Such a science might be teachable only in part to the soldiers of an army, that part being the art of war; but the art of war would imply a complete understanding and might therefore be said to encompass that understanding in a work addressed to actual princes in which Machiavelli does not advertise himself as the inventor of new modes and orders.[27] Even in *Discourses* III.39 Machiavelli speaks modestly of "general and particular knowledge" as knowledge of sites, and is far from claiming the glory of founding a new science in the comprehensive sense, as did Bacon and Descartes.

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[22] *The Prince* 6 (on armed prophets); 19 (on Severus); *Discourses*, Dedicatory Letter (on the writers' praise of Hiero); I.21; III.13, 38.

[23] Not in the *Art of War*, proemium, I (beg.), where Machiavelli says that the art of war is necessary to defend the other arts and is useful for civil life; not in his exchange with the French cardinal in *The Prince* 3, in which understanding war is distinct from understanding the state. See also the "arts of peace" in *Discourses* I.11.

[24] Cf. milder statements in *Discourses* I.21 and III.31.

[25] Livy merely says (XXXV.28.7) that Philopoemen did this so that "no consideration would be new to him in such a matter." Machiavelli has also suppressed Plutarch's remark that Philopoemen loved military affairs more than necessary; Plutarch, *Philopoemen* 4.6.

[26] For a more complete interpretation, see Mansfield (1979, pp. 421-24).

Machiavelli's "firm science," although making use of images, does not incur his own condemnation of the many (in *The Prince* 15) who have imagined republics and principalities "that have never been seen or known to exist in truth." Those imaginary states are based on a "profession of good," whereas Machiavelli's imagination begins from "what is done." In *Discourses* III.39 he reports Xenophon's use of hunting as an image of war, and by "hunting" Machiavelli indicates he means catching one's prey. One may suppose he was aware of the difference (and the similarity) between hunting as catching and hunting as dialectic in Plato's *Laws*.[28] If hunting is for catching, and hunting is an image of war, we can infer—because politics centers on acquisition—that in *The Prince* and the *Discourses* Machiavelli uses war as an image of politics. More attention might be given to the use of imagination in Machiavelli's behavioral political science.

Returning to chapter 14 of *The Prince*, we see that Machiavelli recommends for the second part of the art of war, the exercise of the mind, that the prince read histories in which the actions of excellent men are considered.[29] He must above all do as some excellent man has done in the past who has taken someone before him to imitate who had been praised and glorified, as it is said Alexander the Great imitated Achilles; Caesar, Alexander; Scipio, Cyrus. Thus, contrary to the impression given at the beginning of the sixth chapter, even excellent men may imitate others. Those they imitate are found in "histories," and as in *Discourses* III.39 Machiavelli singles out the "life of Cyrus written by Xenophon" and asserts that in his chastity, affability, humanity and liberality, Scipio conformed to what had been written about Cyrus by Xenophon. Those whom excellent men imitate, then, are men of whom authors such as Homer and especially Xenophon, the Socratic philosopher, have written. From whatever source princes learn the art of war, they imitate the virtues or qualities taught by authors, and, it is implied, their art is incomplete without these qualities. As with founders as Machiavelli describes them in *Discourses* I.9-10, princes possessing the art of war are not self-sufficient but dependent on the moral opinion of society. Accordingly, Machiavelli closes the chapter with a less promising remark about fortune; with this imitation a wise prince can *resist* fortune's adversities, not have a remedy for any accident.

With this modulation we are led to the fifteenth chapter of *The Prince*, entitled "On those things for which men and especially princes are praised or blamed." Here begins the second half of *The Prince*, in which the difficulties caused by the moral qualities are considered.[30] For apparently the art of war cannot teach one to surmount the moral expectations of one's subjects and friends which stand in the way of one's necessary acquisitions. Machiavelli's exaggeration of the art of war does put one in the right frame of mind; for in representing prudence as art, in contradiction to Aristotle, he makes prudence morally neutral (*Nicomachean Ethics* 1140b 25; 1144a 7-9, 23-37). We are thus prepared to be told that one must be prudent to know how to avoid the infamy of those vices that take away one's state (Ch. 15), for example, "the name of stingy" (Ch. 16), that a new prince must temper his cruelties with prudence and humanity (Ch. 17), and that a prudent lord should not keep faith when keeping it works against him (Ch. 18). Machiavelli offers a definition of prudence in chapter 21 that sums up its morally neutral use of the moral qualities: "Prudence consists in knowing how to recognize the qualities of inconveniences and in picking the less bad as good."

After chapter 14, meanwhile, the status of art is diminished as the arts are reduced to partial human activities such as the art of gaining reputation (Ch. 19) or subordinated to the virtues (Ch. 21) or contrasted with violence (Ch. 25). Remarkably, the art of war, which was said to be the sole object and thought of a prince in chapter 14 because it seemed to comprehend all human activities, is not even mentioned after that chapter. Instead, soldiers are said to love a prince of "military spirit" (Ch. 19), Caracalla and Philip of Macedon are praised as "military men" (Chs. 19, 24) and Lorenzo is exhorted to revive "military virtue" in Italy with a saying of Petrarch's predicting that virtue, not art, will take up arms there (Ch. 26).

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[27] In *Discourses* III.39, Decius, the Roman tribune, puts on a cloak so that the enemy would not notice the leader.

[28] Plato, *Laws* 823b; Xenophon makes a characteristically less obtrusive reference to "love of hunting" (*philotheria*), which Cyrus deprecates, *Cyropaideia* II.4.26.

[29] Histories, not mirrors of princes; cf. *Discourses* III.5.

[30] *Resta ora*, *The Prince* 15 begins; on the four parts of *The Prince*, see Strauss (1958, pp. 56-60).

As art is reduced to activities that can be ordered but are still subject to chance (Aristotle, *Nicomachean Ethics* 1140a 12-20), prudence is enlarged to include the governing of fortune. At the end of chapter 23, Machiavelli says that a prince who is not wise himself cannot be well advised unless by chance he gives himself over[31] to "one alone" (*uno solo*) to govern him in everything, who would be *uomo prudentissimo.* This second occurrence of *prudentissimo* in *The Prince,* used to describe the adviser of a prince who indeed governs him in everything, reminds us of the "very prudent" Paulo Vitelli (Ch. 12) who would have had the Florentines in a dilemma if he had taken Pisa. Machiavelli will show how to solve the problem of the adviser who governs the advised, thus benefiting the advised and, not incidentally, saving himself from the fate of Vitelli (cf. *Discourses* I.21, II.33). As adviser of princes (or republics), Machiavelli both is and is not a prince himself. He cannot acquire a new principality for himself, but in teaching princes he gains some of their glory, leaving for them the obvious glory enjoyed by successful politicians and taking for himself the glory of having facilitated their glory, a glory evident only to the discerning. Thus, in the next chapter[32] Machiavelli speaks of the double glory of "having given a beginning to a new principality" (*avere dato principio a uno principato nuovo*), a phrase that both falls short of and exceeds "acquiring a new principality." It exceeds acquiring a new principality because this prince does not merely imitate the great men whose beaten tracks a prudent man was previously required to follow (Ch. 6). His advice gives a beginning to every principality by supplying the moral prerequisites and their theoretical foundation.[33]

The last part of *The Prince* (Chs. 24-26) concerns the problem of sustaining the prince's glory despite "the brevity of life" (Ch. 25). Ordinary princes may not receive the glory they deserve or may receive more glory than they deserve, according to how ill or how well their habits and nature are suited to their times. But the prince who gave ordinary princes their beginning can afford to wait patiently while virtue and fortune are sorted out since the opportunities for his virtue, and hence his glory, are not bound by the brevity of his life.[34] He alone has sure defenses against stormy times, as only for him do such defenses depend on himself and his virtue (Ch. 24). Machiavelli's art has disappeared into his virtue, which is his alone. With his virtue he teaches the art of war or something like it to ordinary princes, but their opportunities for using his teaching depend on their fortune after all. Machiavelli will not allow "these princes of ours" to accuse fortune rather than admit their indolence; he demands, in the notorious sentence at the end of chapter 25, that they proceed impetuously to manhandle lady fortune. Imitating the furious Pope Julius II, they should go beyond where "all human prudence" would have led them. This now limited and subdued prudence serves to distinguish virtue from caution for the generality of Machiavelli's readers as they rush from the necessities he describes toward the prizes he holds out for them. But only he is truly impetuous. Only he is not bound by the moral qualities because only he has learned how to be good and not good. His impetuosity is both impetuous and respectful: impetuous in theory by comparison to other writers, and respectful in practice to Lorenzo and other princes, both actual and potential, to whom he allows their subordinate glories. His position as adviser enables him to share in the natures of all whom he advises as he uses the diverse qualities of those who follow his advice. So by the use of the moral qualities of others, he is not bound by his times as are others, hence not bound by his nature.[35] Since he is not bound by his nature and knows how to be flexible (both *facile* and *duro*) in all times,[36] his fortune is assured. Others may absorb his teaching and a few may perhaps equal or surpass him, but if his teaching is true they follow his fortune. They follow the fortune of the man who first showed men how to become responsible for their fortune. His virtue depends on himself alone, his glory is unrepeatable and he is *uno solo,* not in contemplative isolation but governing for the common benefit of each. His government is established *ad uno tratto* in his books, but also over time as his influence or fortune advances.

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[31] Machiavelli "gave himself over" to the orders of others (*The Prince* 13, end), took that back (15), and now looks for a prince who will give himself over to his adviser (23).

[32] *The Prince* 24 begins: "The things written above, observed prudently. . . ." On the glory of advising, see *Discourses* III.35.

[33] Cf. the double glory in *Discourses* III.13 and *Art of War* VII (in Machiavelli, 1961, p. 515).

[34] On brevity of life, see also *The Prince* 7, 11. And see *Discourses* III.8, 34, 35.

[35] "Nature" occurs 26 times in *The Prince* proper. The first occurrence is "the nature of government" (4); the ninth is "all other things in nature" (6); the eleventh is a "natural defect of spirit" (9); the thirteenth is "the nature of sites" (14); the fourteenth is "the nature of rivers and marshes" (14; cf. 25, beg.); the seventeenth is that a prince needs to know how to use "the one and the other nature" (18); and the nineteenth is "by nature or by art" (19). "Prudence" occurs 28 times and "art" 9 times. The sixth occurrence of "art" is with the nineteenth of "nature," the only association of "nature" and "art."

[36] See especially *The Prince* 24; *facile* and *duro* are the ninth pair in the list of eleven moral qualities in *The Prince* 15, distinct from the others as the qualities of qualities. See also *Discourses* I.41.

Machiavelli could be said to have "ornamented and confirmed" his new principality "with good laws, good arms, good friends and good examples" (Ch. 24) to be found in his books, but he did not write the handbook of an art of politics with rules for all occasions. Nor did he, like Thomas Hobbes, author a new political science or "civil philosophy" on the basis of a new method, and claim credit for it.[37] Even if Machiavelli had thought it possible for a political art to do the work of prudence, and even if the new scientific method had been available to him as it was to Hobbes, he would have had reason to decline the honor of founding a new political art or science. Such a founding would have given him too much glory because it would have subtracted from the glory of later princes—and not enough glory, because later princes applying an impersonal science would not have been subjects of his (see *Discourses* III.13, end).

The Cycle and the Soul

Machiavelli presents his new political science in opposition to classical political science. But despite his statement in chapter 15 of *The Prince* against the imaginary republics and principalities essential to classical political science, his opposition becomes clear only gradually. As can be seen from the preface to the first book of the *Discourses,* he found it convenient to condemn the weakness of his own age by contrast to the ancients, for in doing so he could appeal to opinion in his age favorable to the ancients. My account of his critique of classical political science, passing over the subtlety of his rhetoric, will consider two points of Machiavelli's attack, the cycle and the soul.

In the preface just mentioned, Machiavelli says that the ancients have been imitated in many things, though not in politics. Yet soon after, in *Discourses* I.2, he makes a significant departure from classical political science in regard to the cycle. Classical political science (here assembled from diverse sources) had supposed that regimes tend to change in a regular way—from good to less good to bad to worst; or alternating from good to bad and bad to good. These regular changes make a circle or cycle, coming back to their beginning and beginning again, with the consequence that the progress in human affairs that we moderns expect does not occur and should not be expected. Good times are followed by bad, and bad by good; so, as Isocrates once remarked, a reasonable man might wonder whether it is better to be born in good times that will worsen or bad times that will improve.[38] Civilization may progress as morality, arts and sciences advance; but civilizations are subject to natural catastrophes which return them to their barbarous beginnings, from which they must recommence. The cycle of regimes assumes that the city is self-sufficient; that it becomes better or worse through the actions of its own ruling class, which deserves praise or blame for them; that domestic policy is therefore primary. The cycle of civilizations, however, reminds men of the power of extra-human forces and of the fragility of human constructions. But this reminder of the limits to human choice actually promotes human choice because it teaches men that in the end all merely human force will be overborne, hence that they need not judge their actions merely by their consequences nor their governments by their durability. Moderation—not fearful, but responsible moderation—is the moral lesson of the classical teaching on the cycle.

Machiavelli did not approve of this lesson. In *Discourses* I.2 he almost copies an account of the cycle from Polybius, without naming him as source. The closeness of his copying enables one to see the significant differences, which center on his replacement of "nature" in Polybius by "chance" or "necessity" to explain the origin of governments and of morality.[39] After concluding this account, Machiavelli abruptly adds that almost no republic could survive these ups and downs without becoming subject to a neighboring state better ordered than itself. Speaking for himself ("I say"), he says that all these regimes are pestiferous, the good ones because of their "brevity of life" and the bad ones because of their malignity. Then, having introduced foreign policy as the decisive consideration and thus having denied the assumptions of the classical account of the cycle, he loses interest in the cycle of regimes and never discusses or even refers to it again.

Machiavelli offers, still in *Discourses* I.2, two kinds of legislated beginnings for cities: *uno solo* may make all the laws *ad uno tratto,* as Lycurgus in Sparta; or, as in Rome, the city may lack perfect order but may have made a good beginning apt to become better, and through the occurrence of accidents, may be capable of becoming perfect. At first the beginning which is legislated all at once—the mixed regime of classical political science—seems superior, but Machiavelli shows in *Discourses* I.3-8 that the way of perfection through accidents is far better. Although subject to party discord and "tumults" (in which men shout at each other instead of adjudicating their claims),[40] Rome's way was more flexible in meeting "accidents," especially the accident that if someone wishes to conquer you, you will have to conquer him first. Since every state must meet this accident, it should have a regime that enables it to keep what it has got, like Rome's. Rome could expand successfully, converting foreigners into Romans, while Sparta could not. Rome's regime of accidental perfection, with its emergency solutions and individual initiatives, was superior to Sparta's regime of planned perfection. One should not infer from this that human planning is incompetent in politics, however. We may suspect that Machiavelli, who had to overcome the prejudice against tumultuous republics (*Discourses* I.4), arranged the accidents of the Roman republic as supplied by Livy to suit his plan of perfection. His plan, we have suggested, is legislated all at once in his books but allows for accidents, especially of foreign policy, as they arise over time. It thus combines the two kinds of legislated beginnings set forth in *Discourses* I.2.

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[37] Hobbes (1839-45, I.viii-ix; VII.170-71). Cf. Sternberger (1974, pp. 40-41).

[38] Plato, *Laws* 676b-c; *Republic* 540e-541a; *Statesman* 271e-274e; Aristotle, *Physics* 223b 24-31; *Politics* 1316a 15-22; *On Philosophy* 8; Polybius, VI.9.10; Isocrates, *Areopagiticus* 3-5.

[39] Gennaro Sasso (1967, Chs. 4, 5; 1958, pp. 306-15). Strauss (1958, p. 222); Guillemain (1977, pp. 266-67).

One particular difficulty in the accidental perfection of Rome calls for Machiavelli's intervention. It appears in *Discourses* I.7, where he is considering why party government worked successfully in Rome and brought disaster in Florence. In Florence certain party politicians were able, when pressed, to appeal to "foreign forces," "outside forces," or "private forces." Machiavelli refers to Francesco Valori, a prince of the city as it were, a man who was judged to have wished "with his audacity and spiritedness [*animosità*] to rise above [*trascendere*] civil life," and who could be resisted only with a "sect" contrary to his. Valori's "sect" was the party of Savonarola, who is not mentioned. Soon after, in *Discourses* I. 11, he refers to Savonarola himself, and says that Savonarola was able to persuade the people of Florence, which did not think itself ignorant or rude, that he spoke with God. Clearly one who can persuade others that he speaks with God has a private advantage over other party politicians. He appeals to both a foreign (or outside) and a private source of power.

In the next chapter Machiavelli informs his readers that the Roman Church keeps Italy disunited, chiefly because the Church is not strong enough to seize Italy but strong enough to prevent anyone else from doing so. It prevents others from conquering—or should one say unifying?—Italy by appealing to outside powers such as the king of France to intervene on its behalf. But why can it do this? Because the pope can persuade not necessarily the king of France but the French people that he speaks with God. Thus Machiavelli speaks in this chapter of "Christian states and republics" and of the "Christian republic." Christian republics are divided from each other because of what unites them—a religion that gives opportunity, or makes it necessary, for priests to interfere in politics.

Therefore, a wider view is necessary. One must look not merely to the regime but also to the religion that controls the part of the world where the regime is, to the *sect*. "Sect" is an important Machiavellian term apparently borrowed from Marsilius; and Machiavelli follows Marsilius' impudent application of it to Christianity, for example in the strange phrase "Catholic sect."[41] Machiavelli takes up the cycle of sects in *Discourses* II.5, where he considers what cause is responsible for their rise and fall—men, heaven or nature. He raises the possibility that men, or even a single man, could control not the cycle of regimes but the cycle of sects. For sect is a more comprehensive phenomenon than regime; it includes language and customs, the moral climate of government, as well as politics. Even names and dates are determined by sects. In the *Florentine Histories* (I.5), Machiavelli refers to the fact that after Christianity was established, people stopped naming their sons Caesar and Pompey and began calling them Peter, John and Matthew. And he takes note of the manner in which we date events by ensuring that all the dates given in the *Discourses*—26 of them—occur in his own lifetime.

From the viewpoint of sects, one must consider foreign or outside forces in the widest sense of things foreign to or outside of man. One becomes aware that Machiavelli is not an unreflective humanist who puts human concerns first but a philosopher reflecting on God and nature who puts human necessities first. He does not behold God and nature with wonder, and discourse on what he understands of them. Rather, he regards them with fear, and sees this fear exemplified in fearful, wholesale changes of sect;[42] and so he discourses on the remedies he finds for the fears men must have of their beginnings. Then, from the viewpoint of sects, one could say that in modern times, that is, Machiavelli's times, the cycle is stalled. The proximate cause of the stall is Christianity, which does not esteem the honor of the world and keeps it weak (*Discourses* II.2). But the ultimate cause is in the principles of classical political science, perhaps especially in the classical notion of soul. In reading through *The Prince* and the *Discourses,* the only works of Machiavelli's in which he says he puts everything he knows, one does not find the word "soul" (*anima*).[43] What does this fact mean?

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[40] Aristotle, *Politics* 1280a 9-17.

[41] *Florentine Histories* I.5; V.1; Marsilius of Padua, *Defensor Pacis* I.X.3, 7; II.VIII.4; XVI.7; XX.1; Garin (1970, p. 61); Mansfield (1972, pp. 209-66).

[42] *Discourses* III.1; *Florentine Histories* I.5, VII.1.

As conceived by Plato and Aristotle, the soul was intended to give protection against "outside forces" in the extended sense. Having soul enables men to be different from their environment, to have dignity above the rest of nature and to be free of (or within) the forces of nature and gods outside them. In the classical definition of soul there were two essentials: the soul as beginning of motion and the soul as intellect. Having soul, men can begin an action on their own, not determined from outside; and because men have soul, this action can be intelligent. The problem of this definition was in connecting the two essentials. Whatever action one begins seems to be his own, and yet if the action is to be intelligent, one must be capable of detachment from one's own to achieve an impartial outlook that does not merely endorse one's prejudices. Yet while the two essentials are difficult to connect, it is also necessary to connect them. One's actions are not his own if they are determined by fancy or chance, that is, stupidly or by blindly following the authority of another.[44]

According to Machiavelli, and for a reason to be explained, there is terrible danger to mankind in the attempt to detach oneself from what is one's own. He therefore denied the possibility of detachment in the human soul. What was left in the classical definition was the soul as beginning of motion; but the soul cannot begin motion unless it can act intelligently. Other animals have instincts instead of intelligence and are incapable of truly voluntary or deliberate action. So, in denying the possibility of detached intelligence, Machiavelli had to pay the price of denying that men are capable of voluntary action.[45] But instead of having instincts, men are determined by necessity, or by the necessity that they have the prudence to recognize and foresee. Necessity well understood, not any fool's opinion, replaces deliberate choice in the soul as that which begins voluntary actions. Men may choose, but the only prudent choice is anticipation of necessity.[46] The human desire for glory, which seems to be opposed to necessity because it seems to seek what is in excess of necessary, is in truth comprised in necessity.[47] As we have seen, glory is redeemed from incaution by Machiavelli's plan for the common benefit of mankind, and at the same time it is required for the virtuous promotion of one's own.

It is notorious that Machiavelli once said in a letter that he loved his native country more than his own soul.[48] But which was his native country, Florence or Italy? Should he promote Florentine independence or Italian unity; and if one should get in the way of the other, which should he prefer? Machiavelli does not speak only to Florentines or to Italians but to all men. We may not be Florentines or Italians but we can nonetheless be Machiavellians. Machiavelli shows his solicitude for foreigners by giving them advice in both *The Prince* (3) and the *Discourses* (I.23) on how to invade Italy. Much as he loved Florence and Italy, he is not fundamentally a city or national patriot. He is a patriot on behalf of humanity, seeking to protect men against outside forces, consequently a patriot of the home of human beings, the earth.

In *Discourses* III.2 Machiavelli praises a trick played by Junius Brutus, the father of Roman liberty. According to the oracle of Apollo (as reported by Livy), the first among a group of young men, including Brutus, who kissed his mother, would come to the "highest power in Rome." Brutus decided that his mother was the earth, and pretended to fall in order to kiss the earth. Machiavelli urges us to believe not only that Brutus was ambitious for himself but also that he wished to crush the kings and liberate his native country. Summing up the point in its highest case, Machiavelli says of a "man who is notable for his quality" that he cannot live quietly and untroubled out of politics. However insistently he forswears honor and profit, his excuses will be heard but not accepted. The attitude of philosophic detachment is impossible even for one who does not feel the attraction of honor and profit. You cannot stand so far from princes as not to arouse their suspicion, and if you attempt it, you will nonetheless be involved in their fall. This "middle way" of classical political science between partisanship and unconcern cannot be sustained because the extreme of unconcern is impossible. Even Plato and Aristotle can be described (though to a pope) as no less ambitious for glory than are princes.[49]

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[43] Strauss (1958, pp. 200, 333n.59). *Anima* occurs occasionally in Machiavelli's other works.

[44] Plato, *Laws* 896a-e; *Phaedrus* 245c-d, 247d; *Timaeus* 89a; *Republic* 353d; *Phaedo* 105c; Aristotle, *De Anima* 404b 28-30, 432 a 15-18; *Nicomachean Ethics* 1139a 5-12.

[45] Men must make use of the beast, or beasts, in man; *The Prince* 18.

[46] *The Prince* 3, 6, 9, 25; *Discourses* I.1, 30, 32, 33, 52; III.5, 6.

[47] Thus *uno prudente e virtuoso* has the task of introducing form into the matter which will bring honor to him and good to the universality of men (*The Prince* 26); such a one is perhaps not *prudentissimo* to show so much of his virtue. This is the second of three instances of "form" and "matter" together in *The Prince,* where "form" appears 3 times and "matter" 13 times. See also *Discourses* I.1, 36, 37, 43, 60; III.12.

[48] Letter of April 16, 1527 to Vettori. See *Discursus florentinarum rerum post mortem iunioris Laurentii Medices,* in Machiavelli (1961, p. 275), and cf. Plato, *Laws* 731c. In his comments on Machiavelli's famous statement, Berlin (1980) makes it uncertain whether Machiavelli "revealed his basic moral beliefs" (p. 54) or left open the possibility of choosing to save one's soul (pp. 50, 64).

Necessity is always one's own necessity; that is why necessity overpowers any human capacity of detachment. What is necessary for me is especially what is necessary for me against you. Machiavelli says: "It is enough to ask someone for his weapon without saying, I wish to kill you with it; then after you have the weapon in your hand, you can satisfy your appetite" (*Discourses* I. 44). Since necessity is one's own as opposed to the necessity of others, one's designs prompted by necessity must be disguised. Like Brutus, the prudent man must use deceit. The most general mode of deception practiced by humans is by use of authority.[50] You say that some authority, for example the oracle of Apollo, supports or commands you, when in truth necessity requires it. When men use an authority, they put their own opinions in someone else's mouth—in the mouths of God or of their ancestors, or, like poets, in the mouths of their characters. Under necessity reason is translated into authority. Authority is reason in disguise, or better to say, authority is the effectual truth of detachment. Those who live a life of the mind, detached from politics, in effect elevate some authority in politics. This authority is not truly detached; it only pretends to be. It is their own necessity in disguise.

The greatest example of detachment become authority is the Christian God, which for Machiavelli, I will suggest, was the effectual truth of the classical notion of soul. The soul was intended to preserve human freedom and dignity, but the detachment of soul could be preserved only if one supposed that the soul is divine—that intelligence or soul is God, as Aristotle said. To prevent the divinity of soul from endangering human freedom, Aristotle may have conceived God as impersonal, but this reservation was unavailing. Any idea elevated above human beings is bound to be personified by them and made responsible for their good,[51] and the God of intelligence will be humanized and made providential. Aristotle's God was transformed into the personal Christian God that was used by priests to interfere with princes and tyrannize over peoples. The effectual truth of Aristotelianism was Christianity or that combination of Aristotle and Christianity which Thomas Hobbes was to call Aristotelity. Aristotle should have known that his detached intelligence in the soul would become a God that would be an outside force threatening the liberty of men.

Acting under necessity, then, Machiavelli substitutes what he calls *animo* ("spirit" or "spiritedness") for *anima* ("soul").[52] Whereas *anima* never occurs in *The Prince* and the *Discourses*, *animo* occurs frequently. *Animo* means a spirited defense of one's own, especially of one's own body; for *animo* defends a body and is satisfied with that body, but *anima* always attempts to transcend the body. *Animo* also means intent, as when Machiavelli urges the man notable for his quality to conceal his *animo* (*Discourses* III.2); but the intent, even in this case, is never contemplative or detached from one's own concerns. *Animo* is responsible for *ostinazione*, the obstinacy characteristic of spirited infantry who have planted their feet on the ground.[53]

Machiavelli's principle of spirited selfishness offers something for everybody, not in a common good to which all may contribute different virtues, but (as he says so precisely) for the common benefit of each one (*ciascuno*). Each one in his separate body is encouraged by Machiavelli in the spirited defense of his own, because one's own body is the only common thing that can be benefited. Thus everybody can fight for his own glory and yet be "good and faithful soldiers" to Machiavelli.[54] Machiavellianism can advance without paying allegiance to Machiavelli. A new sect will emerge with an invisible leader that will offer princes more glory and people more security. Not depending on the virtue of rulers but only on virtue in the Machiavellian sense of imaginative aggrandizement, this sect will not be subject to the ups and downs of the cycle. States will rise and fall, but the whole will remain strong and mankind will progress in a condition Machiavelli calls "the perpetual republic."[55]

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[49] "And this glory [of reforming republics and kingdoms] has been so esteemed by men who have not attended to anything other than glory that, when they have been unable to make a republic in deed, they have made it in writing, like Aristotle, Plato and many others. They wished to show the world that if they were unable to found a civil association like Solon and Lycurgus, they did not fail because of their ignorance but because of their impotence. . ." (Machiavelli, *Discursus*, 1961, pp. 275-76). See also Manent (1977, p. 21).

[50] *The Prince* 7; *Discourses* I.7, 9, 34, 35, 53; II.12, 18; III.3, 5, 10, 30, 35, 38, 48.

[51] As Machiavelli personifies *fortuna*, *The Prince* 25; *Discourses* II.29. See *Discourses* I.53 for an instance of the people being deceived by a "false image of good"; also I.56, III.43. On the cause of making the Church powerful, see *The Prince* 3 (end), 11.

[52] As in the striking phrase, *virtù di corpo e di animo*, *The Prince* 8; *Discourses* III.8; *Florentine Histories* VI.6; and in *grande virtù di animo*, *The Prince* 19; *grandezza dello anima*, 8, 26. See also the greater reliability of touching than seeing, *The Prince* 18 (end), and the use of *umori* (humors of the body) in *The Prince* 9, 19; *Discourses* I.4. Scholarly discussion of *virtù* in Machiavelli as a rule forgets to ask what it is that virtue is the virtue of.

[53] There is republican obstinacy (*Discourses* II.2), religious obstinacy (I.14, 15), obstinacy in the infantry (II.16, 17; III.12), and obstinate conspirators (III.6; *The Prince* 19).

If Machiavelli's ambition seems grandiose, we should look carefully at the modern world we live in to see whether our ambition, which resembles his, is reasonable. Machiavelli, indeed, left a restraint on human aggrandizement which has not proved durable. Since for him there is no soul or principle *above* one's own country, there could be no universal principle *beyond* one's country. The crucial political implication is that, for Machiavelli, no *patria* could ever regard itself as essential to the destiny of mankind. There could be no chosen people, race or class. Machiavelli would have abhorred our twentieth-century tyrannies, but with our century in view he might have to admit that he had left no effectual alternative to dubious patriotism but a modern version of the "pious cruelty" (*The Prince* 21) he had meant to destroy. Twentieth-century totalitarianism promises fantastic betterment in the human condition from the realization of universal principles, but it does not claim to improve the soul. It has, without reference to soul, proved to exceed the pious cruelty of any sect with which Machiavelli was acquainted.

Perhaps, then, not the asserted existence of soul but loss of moderation in the soul has been the cause of our troubles. But we shall never learn whether this is so if we hold to a political science that routinely excludes soul from what it calls "behavior" and flutters at the mention of virtue.

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[54] *Discourses* I.43. See *The Prince* 19 (end) on maintaining a state without having the respect of peoples.

[55] A perpetual republic is denied in *Discourses* III.17, but affirmed in III.22 after the remedy is found; and it is assumed possible in 1.20; II.5; III.1, 3.

The Emergence of Cooperation among Egoists

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This article investigates the conditions under which cooperation will emerge in a world of egoists without central authority. This problem plays an important role in such diverse fields as political philosophy, international politics, and economic and social exchange. The problem is formalized as an iterated Prisoner's Dilemma with pairwise interaction among a population of individuals.

Results from three approaches are reported: the tournament approach, the ecological approach, and the evolutionary approach. The evolutionary approach is the most general since all possible strategies can be taken into account. A series of theorems is presented which show: (1) the conditions under which no strategy can do any better than the population average if the others are using the reciprocal cooperation strategy of TIT FOR TAT, *(2) the necessary and sufficient conditions for a strategy to be collectively stable, and (3) how cooperation can emerge from a small cluster of discriminating individuals even when everyone else is using a strategy of unconditional defection.*

Under what conditions will cooperation emerge in a world of egoists without central authority? This question has played an important role in a variety of domains including political philosophy, international politics, and economic and social exchange. This article provides new results which show more completely than was previously possible the conditions under which cooperation will emerge. The results are more complete in two ways. First, *all* possible strategies are taken into account, not simply some arbitrarily selected subset. Second, not only are equilibrium conditions established, but also a mechanism is specified which can move a population from noncooperative to cooperative equilibrium.

The situation to be analyzed is the one in which narrow self-maximization behavior by each person leads to a poor outcome for all. This is the famous Prisoner's Dilemma game. Two individuals can each either cooperate or defect. No matter what the other does, defection yields a higher payoff than cooperation. But if both defect, both do worse than if both cooperated. Figure 1 shows the payoff matrix with sample utility numbers attached to the payoffs. If the other player cooperates, there is a choice between cooperation which yields R (the reward for mutual cooperation) or defection which yields T (the temptation to defect). By assumption, $T > R$, so it pays to defect if the other player cooperates. On the other hand, if the other player defects, there is a choice between cooperation which yields S (the sucker's payoff), or defection which yields P (the punishment for mutual defection). By assumption, $P > S$, so it pays to defect if the other player defects. Thus no matter what the other player does, it pays to defect. But if both defect, both get P rather than the R they could both have got if both had cooperated. But R is assumed to be greater than P. Hence the dilemma. Individual rationality leads to a worse outcome for both than is possible.

To insure that an even chance of exploitation or being exploited is not as good an outcome as mutual cooperation, a final inequality is added in the standard definition of the Prisoner's Dilemma. This is just $R > (T+S)/2$.

Thus two egoists playing the game once will both choose their dominant choice, defection, and get a payoff, P, which is worse for both than the R they could have got if they had both cooperated. If the game is played a known finite number of times, the players still have no incentive to

	Cooperate	Defect
Cooperate	R=3, R=3	S=0, T=5
Defect	T=5, S=0	P=1, P=1

$$T > R > P > S$$
$$R > (S+T)/2$$

Source: Robert Axelrod, "Effective Choice in the Prisoner's Dilemma," *Journal of Conflict Resolution* 24 (1980): 3-25; "More Effective Choice in the Prisoner's Dilemma," *Journal of Conflict Resolution* 24 (1980): 379-403.

Note: The payoffs to the row chooser are listed first.

Figure 1. A Prisoner's Dilemma

I would like to thank John Chamberlin, Michael Cohen, Bernard Grofman, William Hamilton, John Kingdon, Larry Mohr, John Padgett and Reinhard Selten for their help, and the Institute of Public Policy Studies for its financial support.

cooperate. This is certainly true on the last move since there is no future to influence. On the next-to-last move they will also have no incentive to cooperate since they can anticipate mutual defection on the last move. This line of reasoning implies that the game will unravel all the way back to mutual defection on the first move of any sequence of plays which is of known finite length (Luce and Raiffa, 1957, pp. 94-102). This reasoning does not apply if the players will interact an indefinite number of times. With an indefinite number of interactions, cooperation can emerge. This article will explore the precise conditions necessary for this to happen.

The importance of this problem is indicated by a brief explanation of the role it has played in a variety of fields.

1. *Political Philosophy.* Hobbes regarded the state of nature as equivalent to what we now call a two-person Prisoner's Dilemma, and he built his justification for the state upon the purported impossibility of sustained cooperation in such a situation (Taylor, 1976, pp. 98-116). A demonstration that mutual cooperation could emerge among rational egoists playing the iterated Prisoner's Dilemma would provide a powerful argument that the role of the state should not be as universal as some have argued.

2. *International Politics.* Today nations interact without central control, and therefore the conclusions about the requirements for the emergence of cooperation have empirical relevance to many central issues of international politics. Examples include many varieties of the security dilemma (Jervis, 1978) such as arms competition and its obverse, disarmament (Rapoport, 1960); alliance competition (Snyder, 1971); and communal conflict in Cyprus (Lumsden, 1973). The selection of the American response to the Soviet invasion of Afghanistan in 1979 illustrates the problem of choosing an effective strategy in the context of a continuing relationship. Had the United States been perceived as continuing business as usual, the Soviet Union might have been encouraged to try other forms of noncooperative behavior later. On the other hand, any substantial lessening of U.S. cooperation risked some form of retaliation which could then set off counter-retaliation, setting up a pattern of mutual defection that could be difficult to get out of. Much of the domestic debate over foreign policy is over problems of just this type.

3. *Economic and social exchange.* Our everyday lives contain many exchanges whose terms are not enforced by any central authority. Even in economic exchanges, business ethics are maintained by the knowledge that future interactions are likely to be affected by the outcome of the current exchange.

4. *International political economy.* Multinational corporations can play off host governments to lessen their tax burdens in the absence of coordinated fiscal policies between the affected governments. Thus the commodity exporting country and the commodity importing country are in an iterated Prisoner's Dilemma with each other, whether they fully appreciate it or not (Laver, 1977).

In the literatures of these areas, there has been a convergence on the nature of the problem to be analyzed. All agree that the two-person Prisoner's Dilemma captures an important part of the strategic interaction. All agree that what makes the emergence of cooperation possible is the possibility that interaction will continue. The tools of the analysis have been surprisingly similar, with game theory serving to structure the enterprise.

As a paradigm case of the emergence of cooperation, consider the development of the norms of a legislative body, such as the United States Senate. Each senator has an incentive to appear effective for his or her constituents even at the expense of conflicting with other senators who are trying to appear effective for *their* constituents. But this is hardly a zero-sum game since there are many opportunities for mutually rewarding activities between two senators. One of the consequences is that an elaborate set of norms, or folkways, have emerged in the Senate. Among the most important of these is the norm of reciprocity, a folkway which involves helping out a colleague and getting repaid in kind. It includes vote trading, but it extends to so many types of mutually rewarding behavior that "it is not an exaggeration to say that reciprocity is a way of life in the Senate" (Matthews, 1960, p. 100; see also Mayhew, 1974).

Washington was not always like this. Early observers saw the members of the Washington community as quite unscrupulous, unreliable, and characterized by "falsehood, deceit, treachery" (Smith, 1906, p. 190). But by now the practice of reciprocity is well established. Even the significant changes in the Senate over the last two decades toward more decentralization, more openness, and more equal distribution of power have come without abating the folkway of reciprocity (Ornstein, Peabody and Rhode, 1977). I will show that we do *not* need to assume that senators are more honest, more generous, or more public-spirited than in earlier years to explain how cooperation based on reciprocity has emerged and proven stable. The emergence of cooperation can be explained as a consequence of senators pursuing their own interests.

The approach taken here is to investigate how individuals pursuing their own interests will act, and then see what effects this will have for the system as a whole. Put another way, the approach is to make some assumptions about micro-motives, and then deduce consequences for macrobehavior (Schelling, 1978). Thinking about the paradigm case of a legislature is a convenience, but the same style of reasoning can apply to the emergence of cooperation between individuals in many other political settings, or even to relations between nations. While investigating the conditions which foster the emergence of cooperation, one should bear in mind that cooperation is not always socially desirable. There are times when public policy is best served by the prevention of cooperation—as in the need for regulatory action to prevent collusion between oligopolistic business enterprises.

The basic situation I will analyze involves pairwise interactions.[1] I assume that the player can recognize another player and remember how the two of them have interacted so far. This allows the history of the particular interaction to be taken into account by a player's strategy.

A variety of ways to resolve the dilemma of the Prisoner's Dilemma have been developed. Each involves allowing some additional activity which alters the strategic interaction in such a way as to fundamentally change the nature of the problem. The original problem remains, however, because there are many situations in which these remedies are not available. I wish to consider the problem in its fundamental form.

1. There is no mechanism available to the players to make enforceable threats or commitments (Schelling, 1960). Since the players cannot make commitments, they must take into account all possible strategies which might be used by the other player, and they have all possible strategies available to themselves.

2. There is no way to be sure what the other player will do on a given move. This eliminates the possibility of metagame analysis (Howard, 1971) which allows such options as "make the same choice as the other player is about to make." It also eliminates the possibility of reliable reputations such as might be based on watching the other player interact with third parties.

3. There is no way to change the other player's utilities. The utilities already include whatever consideration each player has for the interests of the other (Taylor, 1976, pp. 69-83).

Under these conditions, words not backed by actions are so cheap as to be meaningless. The players can communicate with each other only through the sequence of their own behavior. This is the problem of the iterated Prisoner's Dilemma in its fundamental form.

Two things remain to be specified: how the payoff of a particular move relates to the payoff in a whole sequence, and the precise meaning of a strategy. A natural way to aggregate payoffs over time is to assume that later payoffs are worth less than earlier ones, and that this relationship is expressed as a constant discount per move (Shubik, 1959, 1970). Thus the next payoff is worth only a fraction, w, of the same payoff this move. A whole string of mutual defection would then have a "present value" of $P + wP + w^2P + w^3P \ldots = P/(1-w)$. The discount parameter, w, can be given either of two interpretations. The standard economic interpretation is that later consumption is not valued as much as earlier consumption. An alternative interpretation is that future moves may not actually occur, since the interaction between a pair of players has only a certain probability of continuing for another move. In either interpretation, or a combination of the two, w is strictly between zero and one. The smaller w is, the less important later moves are relative to earlier ones.

For a concrete example, suppose one player is following the policy of always defecting, and the other player is following the policy of *TIT FOR TAT*. *TIT FOR TAT* is the policy of cooperating on the first move and then doing whatever the other player did on the previous move. This means that *TIT FOR TAT* will defect once for each defection by the other player. When the other player is using *TIT FOR TAT*, a player who always defects will get T on the first move, and P on all the subsequent moves. The payoff to someone using *ALL D* when playing with someone using *TIT FOR TAT* is thus:

$$\begin{aligned} V(ALL\,D \mid TFT) &= T + wP + w^2P + w^3P \ldots \\ &= T + wP(1 + w + w^2 \ldots) \\ &= T + wP/(1-w). \end{aligned}$$

Both *ALL D* and *TIT FOR TAT* are strategies. In general, a *strategy* (or decision rule) is a function from the history of the game so far into a probability of cooperation on the next move. Strategies can be stochastic, as in the example of a rule which is entirely random with equal probabilities of cooperation and defection on each move. A strategy can also be quite sophisticated in its use of the pattern of outcomes in the game so far to determine what to do next. It may, for example, use Bayesian techniques to estimate the parameters of some model it might have of the other player's rule. Or it may be some complex combination of other strategies. But a strategy must rely solely on the information available through this one sequence of interactions with the particular player.

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[1] A single player may be interacting with many others, but the player is interacting with them one at a time. The situations which involve more than pairwise interaction can be modeled with the more complex n-person Prisoner's Dilemma (Olson, 1965; G. Hardin, 1968; R. Hardin, 1971; Schelling, 1973). The principal application is to the provision of collective goods. It is possible that the results from pairwise interactions will help suggest how to undertake a deeper analysis of the n-person case as well, but that must wait. For a parallel treatment of the two-person and n-person cases, see Taylor (1976, pp. 29-62).

The first question one is tempted to ask is, "What is the best strategy?" This is a good question, but unfortunately there is no best rule independent of the environment which it might have to face. The reason is that what works best when the other player is unconditionally cooperative will not in general work well when the other player is conditionally cooperative, and vice versa. To prove this, I will introduce the concept of a *nice* strategy, namely, one which will never be the first to defect.

THEOREM 1. *If the discount parameter,* w, *is sufficiently high, there is no best strategy independent of the strategy used by the other player.*

PROOF. Suppose A is a strategy which is best regardless of the strategy used by the other player. This means that for any strategies A' and B, $V(A|B) \geq V(A'|B)$. Consider separately the cases where A is nice and A is not nice. If A is nice, let $A' = ALL\ D$, and let $B = ALL\ C$. Then $V(A|B) = R/(1-w)$ which is less than $V(A'|B) = T/(1-w)$. On the other hand, if A is not nice, let $A' = ALL\ C$, and let B be the strategy of cooperating until the other player defects and then always defects. Eventually A will be the first to defect, say, on move n. The value of n is irrelevant for the comparison to follow, so assume that $n = 1$. To give A the maximum advantage, assume that A always defects after its first defection. Then $V(A|B) = T + wP/(1-w)$. But $V(A'|B) = R/(1-w) = R+wR/(1-w)$. Thus $V(A|B) < V(A'|B)$ whenever $w > (T-R)/(T-P)$. Thus the immediate advantage gained by the defection of A will eventually be more than compensated for by the long-term disadvantage of B's unending defection, assuming that w is sufficiently large. Thus, if w is sufficiently large, there is no one best strategy.

In the paradigm case of a legislature, this theorem says that if there is a large enough chance that a member of Congress will interact *again* with another member of Congress, then there is no one best strategy to use independently of the strategy being used by the other person. It would be best to be cooperative with someone who will reciprocate that cooperation in the future, but not with someone whose future behavior will not be very much affected by this interaction (see, for example, Hinckley, 1972). The very possibility of achieving stable mutual cooperation depends upon there being a good chance of a continuing interaction, as measured by the magnitude of w. Empirically, the chance of two members of Congress having a continuing interaction has increased dramatically as the biennial turnover rates in Congress have fallen from about 40 percent in the first 40 years of the Republic to about 20 percent or less in recent years (Young, 1966, pp. 87-90; Jones, 1977, p. 254; Patterson, 1978, pp. 143-44). That the increasing institutionalization of Congress has had its effects on the development of congressional norms has been widely accepted (Polsby, 1968, esp. n. 68). We now see how the diminished turnover rate (which is one aspect of institutionalization) can allow the development of reciprocity (which is one important part of congressional folkways).

But saying that a continuing chance of interaction is necessary for the development of cooperation is not the same as saying that it is sufficient. The demonstration that there is not a single best strategy still leaves open the question of what patterns of behavior can be expected to emerge when there actually is a sufficiently high probability of continuing interaction between two people.

The Tournament Approach

Just because there is no single best decision rule does not mean analysis is hopeless. For example, progress can be made on the question of which strategy does best in an environment of players who are also using strategies designed to do well. To explore this question, I conducted a tournament of strategies submitted by game theorists in economics, psychology, sociology, political science and mathematics (Axelrod, 1980a). Announcing the payoff matrix shown in Figure 1, and a game length of 200 moves, I ran the 14 entries and *RANDOM* against each other in a round robin tournament. The result was that the highest average score was attained by the simplest of all the strategies submitted, *TIT FOR TAT.*

I then circulated the report of the results and solicited entries for a second round. This time I received 62 entries from six countries.[3] Most of the contestants were computer hobbyists, but there were also professors of evolutionary biology, physics, and computer science, as well as the five disciplines represented in the first round. *TIT FOR TAT* was again submitted by the winner of the first round, Anatol Rapoport of the Institute for Advanced Study (Vienna). And it won again. An analysis of the 3,000,000 choices which were made in the second round shows that *TIT FOR TAT* was a very robust rule because it was nice, provocable into a retaliation by a defection of the other, and yet forgiving after it took its one retaliation (Axelrod, 1980b).

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[3] In the second round, the length of the games was uncertain, with an expected median length of 200 moves. This was achieved by setting the probability that a given move would not be the last one at $w = .99654$. As in the first round, each pair was matched in five games. See Axelrod (1980b) for a complete description.

The Ecological Approach

To see if *TIT FOR TAT* would do well in a whole series of simulated tournaments, I calculated what would happen if each of the strategies in the second round were submitted to a hypothetical next round in proportion to its success in the previous round. This process was then repeated to generate the time path of the distribution of strategies. The results showed that as the less-successful rules were displaced, *TIT FOR TAT* continued to do well with the rules which initially scored near the top. In the long run, *TIT FOR TAT* displaced all the other rules and went to what biologists call fixation (Axelrod, 1980b).

This is an ecological approach because it takes as given the varieties which are present and investigates how they do over time when interacting with each other. It provides further evidence of the robust nature of the success of *TIT FOR TAT.*

The Evolutionary Approach

A much more general approach would be to allow all possible decision rules to be considered, and to ask what are the characteristics of the decision rules which are stable in the long run. An evolutionary approach recently introduced by biologists offers a key concept which makes such an analysis tractable (Maynard Smith, 1974, 1978). This approach imagines the existence of a whole population of individuals employing a certain strategy, B, and a single mutant individual employing another strategy, A. Strategy A is said to *invade* strategy B if $V(A|B) > V(B|B)$ where $V(A|B)$ is the expected payoff an A gets when playing a B, and $V(B|B)$ is the expected payoff a B gets when playing another B. Since the B's are interacting virtually entirely with other B's, the concept of invasion is equivalent to the single mutant individual being able to do better than the population average. This leads directly to the key concept of the evolutionary approach. A strategy is *collectively stable* if no strategy can invade it.[3]

The biological motivation for this approach is based on the interpretation of the payoffs in terms of fitness (survival and fecundity). All mutations are possible, and if any could invade a given population it would have had the chance to do so. Thus only a collectively stable strategy is expected to be able to maintain itself in the long-run equilibrium as the strategy used by all.[4] Collectively stable strategies are important because they are the only ones which an entire population can maintain in the long run if mutations are introduced one at a time.

The political motivation for this approach is based on the assumption that all strategies are possible, and that if there were a strategy which would benefit an individual, someone is sure to try it. Thus only a collectively stable strategy can maintain itself as the strategy used by all—provided that the individuals who are trying out novel strategies do not interact too much with one another.[5] As we shall see later, if they do interact in clusters, then new and very important developments become possible.

A difficulty in the use of this concept of collective stability is that it can be very hard actually to determine which strategies have it and which do not. In most biological applications, including both the Prisoner's Dilemma and other types of interactions, this difficulty has been dealt with in one of two ways. One method has been to restrict the analysis to situations where the strategies take some particularly simple form such as in one-parameter models of sex-ratios (Hamilton, 1967). The other method has been to restrict the strategies themselves to a relatively narrow set so that some illustrative results could be attained (Maynard Smith and Price, 1973; Maynard Smith, 1978).

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[3] Those familiar with the concepts of game theory will recognize this as a strategy being in Nash equilibrium with itself. My definitions of invasion and collective stability are slightly different from Maynard Smith's (1974) definitions of invasion and evolutionary stability. His definition of invasion allows $V(A|B) = V(B|B)$ provided that $V(B|A) > V(A|A)$. I have used the new definitions to simplify the proofs and to highlight the difference between the effect of a single mutant and the effect of a small number of mutants. Any rule which is evolutionarily stable is also collectively stable. For a nice rule, the definitions are equivalent. All theorems in the text remain true if "evolutionary stability" is substituted for "collective stability" with the exception of Theorem 3, where the characterization is necessary but no longer sufficient.

[4] For the application of the results of this article to biological contexts, see Axelrod and Hamilton (1981). For the development in biology of the concepts of reciprocity and stable strategy, see Hamilton (1964), Trivers (1971), Maynard Smith (1974), and Dawkins (1976).

[5] Collective stability can also be interpreted in terms of a commitment by one player, rather than the stability of a whole population. Suppose player Y is committed to using strategy B. Then player X can do no better than use this same strategy B if and only if strategy B is collectively stable.

The difficulty of dealing with all possible strategies was also faced by Michael Taylor (1976), a political scientist who sought a deeper understanding of the issues raised by Hobbes and Hume concerning whether people in a state of nature would be expected to cooperate with each other. He too employed the method of using a narrow set of strategies to attain some illustrative results. Taylor restricted himself to the investigation of four particular strategies (including *ALL D, ALL C, TIT FOR TAT,* and the rule "cooperate until the other defects, and then always defect"), and one set of strategies which retaliates in progressively increasing numbers of defections for each defection by the other. He successfully developed the equilibrium conditions when these are the only rules which are possible.[6]

Before running the computer tournament I believed that it was impossible to make very much progress if all possible strategies were permitted to enter the analysis. However, once I had attained sufficient experience with a wide variety of specific decision rules, and with the analysis of what happened when these rules interacted, I was able to formulate and answer some important questions about what would happen if all possible strategies were taken into account.

The remainder of this article will be devoted to answering the following specific questions about the emergence of cooperation in the iterated Prisoner's Dilemma:

1. Under what conditions is *TIT FOR TAT* collectively stable?
2. What are the necessary and sufficient conditions for any strategy to be collectively stable?
3. If virtually everyone is following a strategy of unconditional defection, when can cooperation emerge from a small cluster of newcomers who introduce cooperation based on reciprocity?

TIT FOR TAT as a Collectively Stable Strategy

TIT FOR TAT cooperates on the first move, and then does whatever the other player did on the previous move. This means that any rule which starts off with a defection will get T, the highest possible payoff, on the first move when playing *TIT FOR TAT.* Consequently, *TIT FOR TAT* can only avoid being invadable by such a rule if the game is likely to last long enough for the retaliation to counteract the temptation to defect. In fact, no rule can invade *TIT FOR TAT* if the discount parameter, w, is sufficiently large. This is the heart of the formal result contained in the following theorem. Readers who wish to skip the proofs can do so without loss of continuity.

THEOREM 2. TIT FOR TAT *is a collectively stable strategy if and only if* $w > \max(\frac{T-R}{T-P}, \frac{T-R}{R-S})$. *An alternative formulation of the same result is that* TIT FOR TAT *is a collectively stable strategy if and only if it is invadable neither by* ALL D *nor the strategy which alternates defection and cooperation.*

PROOF. First we prove that the two formulations of the theorem are equivalent, and then we prove both implications of the second formulation. To say that *ALL D* cannot invade *TIT FOR TAT* means that $V(ALL\ D|TFT) < V(TFT|TFT)$. As shown earlier, $V(ALL\ D|TFT) = T + wP/(1-w)$. Since *TFT* always cooperates with its twin, $V(TFT|TFT) = R + wR + w^2R\ldots = R/(1-w)$. Thus *ALL D* cannot invade *TIT FOR TAT* when $T + wP/(1-w) < R/(1-w)$, or $T(1-w) + wP < R$, or $T-R < w(T-P)$ or $w > \frac{T-R}{T-P}$. Similarly, to say that alternation of D and C cannot invade *TIT FOR TAT* means that $(T+wS)/(1-w^2) < R/(1-w)$, or $\frac{T-R}{R-S} < w$. Thus $w > \frac{T-R}{T-P}$ and $w > \frac{T-R}{R-S}$ is equivalent to saying that *TIT FOR TAT* is invadable by neither *ALL D* nor the strategy which alternates defection and cooperation. This shows that the two formulations are equivalent.

Now we prove both of the implications of the second formulation. One implication is established by the simple observation that if *TIT FOR TAT* is a collectively stable strategy, then no rule can invade, and hence neither can the two specified rules. The other implication to be proved is that if neither *ALL D* nor Alternation of D and C can invade *TIT FOR TAT,* then no strategy can.

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[6] For the comparable equilibrium conditions when all possible strategies are allowed, see below, Theorems 2 through 6. For related results on the potential stability of cooperative behavior, see Luce and Raiffa (1957, p. 102), Kurz (1977) and Hirshleifer (1978).

TIT FOR TAT has only two states, depending on what the other player did the previous move (on the first move it assumes, in effect, that the other player has just cooperated). Thus if *A* is interacting with *TIT FOR TAT*, the best which any strategy, *A*, can do after choosing *D* is to choose *C* or *D*. Similarly, the best *A* can do after choosing *D* is to choose *C* or *D*. This leaves four possibilities for the best *A* can do with *TIT FOR TAT*: repeated sequences of *CC*, *CD*, *DC*, or *DD*. The first does the same as *TIT FOR TAT* does with another *TIT FOR TAT*. The second cannot do better than both the first and the third. This implies if the third and fourth possibilities cannot invade *TIT FOR TAT*, then no strategy can. These two are equivalent, respectively, to Alternation of *D* and *C*, and *ALL D*. Thus if neither of these two can invade *TIT FOR TAT*, no rule can, and *TIT FOR TAT* is a collectively stable strategy.

The significance of this theorem is that it demonstrates that if everyone in a population is cooperating with everyone else because each is using the *TIT FOR TAT* strategy, no one can do better using any other strategy *provided* the discount parameter is high enough. For example, using the numerical values of the payoff parameters given in Figure 1, *TIT FOR TAT* is uninvadable when the discount parameter, *w*, is greater than 2/3. If *w* falls below this critical value, and everyone else is using *TIT FOR TAT*, it will pay to defect on alternative moves. For *w* less than 1/2, *ALL D* can also invade.

One specific implication is that if the other player is unlikely to be around much longer because of apparent weakness, then the perceived value of *w* falls and the reciprocity of *TIT FOR TAT* is no longer stable. We have Caesar's explanation of why Pompey's allies stopped cooperating with him. "They regarded his [Pompey's] prospects as hopeless and acted according to the common rule by which a man's friends become his enemies in adversity" (trans. by Warner, 1960, p. 328). Another example is the business institution of the factor who buys a client's accounts receivable. This is done at a very substantial discount when the firm is in trouble because

> once a manufacturer begins to go under, even his best customers begin refusing payment for merchandise, claiming defects in quality, failure to meet specifications, tardy delivery, or what-have-you. The great enforcer of morality in commerce is the continuing relationship, the belief that one will have to do business again with this customer, or this supplier, and when a failing company loses this automatic enforcer, not even a strong-arm factor is likely to find a substitute (Mayer, 1974, p. 280).

Similarly, any member of Congres who is perceived as likely to be defeated in the next election may have some difficulty doing legislative business with colleagues on the usual basis of trust and good credit.[7]

There are many other examples of the importance of long-term interaction for the stability of cooperation in the iterated Prisoner's Dilemma. It is easy to maintain the norms of reciprocity in a stable small town or ethnic neighborhood. Conversely, a visiting professor is likely to receive poor treatment by other faculty members compared to the way these same prople treat their regular colleagues.

Another consequence of the previous theorem is that if one wants to prevent rather than promote cooperation, one should keep the same individuals from interacting too regularly with each other. Consider the practice of the government selecting two aerospace companies for competitive development contracts. Since firms specialize to some degree in either air force or in navy planes, there is a tendency for firms with the same specialties to be frequently paired in the final competition (Art, 1968). To make tacit collusion between companies more difficult, the government should seek methods of compensating for the specialization. Pairs of companies which shared a specialization would then expect to interact less often in the final competitions. This would cause the later interactions between them to be worth relatively less than before, reducing the value of *w*. If *w* is sufficiently low, reciprocal cooperation in the form of tacit collusion ceases to be a stable policy.

Knowing when *TIT FOR TAT* cannot be invaded is valuable, but it is only a part of the story. Other strategies may be, and in fact are, also collectively stable. This suggests the question of what a strategy has to do to be collectively stable. In other words, what policies, if adopted by everyone, will prevent any one individual from benefiting by a departure from the common strategy?

The Characterization of Collectively Stable Strategies

The characterization of all collectively stable strategies is based on the idea that invasion can be prevented if the rule can make the potential invader worse off than if it had just followed the common strategy. Rule B can prevent invasion by rule A if B can be sure that no matter what A does later, B will hold A's total score low enough. This leads to the following useful definition: B has a *secure position* over A on move n if no matter what A does from move n onwards, $V(A|B) < V(B|B)$, assuming that B defects from move n onwards. Let $V_n(A|B)$ represent A's discounted cumulative score in the moves before move n. Then another way of saying that B has a secure position over A on move n is that

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[7] A countervailing consideration is that a legislator in electoral trouble may receive help from friendly colleagues who wish to increase the chances of reelection of someone who has proven in the past to be cooperative, trustworthy, and effective. Two current examples are Morris Udall and Thomas Foley. (I wish to thank an anonymous reviewer for this point.)

$$V_n(A|B) + w^{n-1}P/(1-w) < V(B|B),$$

since the best A can do from move n onwards if B defects is get P each time. Moving the second term to the right side of the inequality gives the helpful result that B has a secure position over A on move n if $V_n(A|B)$ is small enough, namely if

$$V_n(A|B) < V(B|B) - w^{n-1}P/(1-w). \qquad (1)$$

The theorem which follows embodies the advice that if you want to employ a collectively stable strategy, you should only cooperate when you can afford an exploitation by the other side and still retain your secure position.

THEOREM 3. THE CHARACTERIZATION THEOREM. B *is a collectively stable strategy if and only if* B *defects on move* n *whenever the other player's cumulative score so far is too great, specifically when* $V_n(A|B) > V(B|B) - w^{n-1}(T+wP/(1-w))$.

PROOF. First it will be shown that a strategy B which defects as required will always have a secure position over any A, and therefore will have $V(A|B) < V(B|B)$ which in turn makes B a collectively stable strategy. The proof works by induction. For B to have a secure position on move 1 means that $V(A|ALL\ D) < V(ALL\ D|ALL\ D)$ according to the definition of secure position applied to $n=1$. Since this is true for all A, B has a secure position on move 1. If B has a secure position over A on move n, it has a secure position on move $n+1$. This is shown in two parts.

First, if B defects on move n, A gets at most P, so

$$V_{n+1}(A|B) < V_n(A|B) + w^{n-1}P.$$

Using Equation (1) gives:

$$V_{n+1}(A|B) < V(B|B) - w^{n-1}P/(1-w) + w^{n-1}P$$

$$V_{n+1}(A|B) < V(B|B) - w^{n}P/(1-w).$$

Second, B will only cooperate on move n when

$$V_n(A|B) < V(B|B) - w^{n-1}(T+wP/(1-w)).$$

Since A can get at most T on move n, we have

$$V_{n+1}(A|B) < V(B|B) - w^{n-1}(T+wP/(1-w)) + w^{n-1}T$$

$$V_{n+1}(A|B) < V(B|B) - w^{n}P/(1-w).$$

Therefore, B always has a secure position over A, and consequently B is a collectively stable strategy.

The second part of the proof operates by contradiction. Suppose that B is a collectively stable strategy and there is an A and an n such that B does not defect on move n when

$$V_n(A|B) > V(B|B) - w^{n-1}(T+wP/(1-w)),$$

i.e., when

$$V_n(A|B) + w^{n-1}(T+wP/(1-w)) > V(B|B). \qquad (2)$$

Define A' as the same as A on the first $n-1$ moves, and D thereafter. A' gets T on move n (since B cooperated then), and at least P thereafter. So,

$$V(A'|B) > V_n(A|B) + w^{n-1}(T+wP/(1-w)).$$

Combined with (2) this gives $V(A'|B) > V(B|B)$. Hence A' invades B, contrary to the assumption that B is a collectively stable strategy. Therefore, if B is a collectively stable strategy, it must defect when required.

Figure 2 illustrates the use of this theorem. The dotted line shows the value which must not be exceeded by any A if B is to be a collectively stable strategy. This value is just $V(B|B)$, the expected payoff attained by a player using B when virtually all the other players are using B as well. The solid curve represents the critical value of A's cumulative payoff so far. The theorem simply says that B is a collectively stable strategy if and only if it defects whenever the other player's cumulative value so far in the game is above this line. By doing so, *B* is able to prevent the other player from eventually getting a total expected value of more than rule *B* gets when playing another rule *B*.

The characterization theorem is "policy-relevant" in the abstract sense that it specifies what a strategy, *B*, has to do at any point in time as a function of the previous history of the interaction in order for *B* to be a collectively stable strategy.[8] It is a complete characterization because this requirement is both a necessary and a sufficient condition for strategy *B* to be collectively stable.

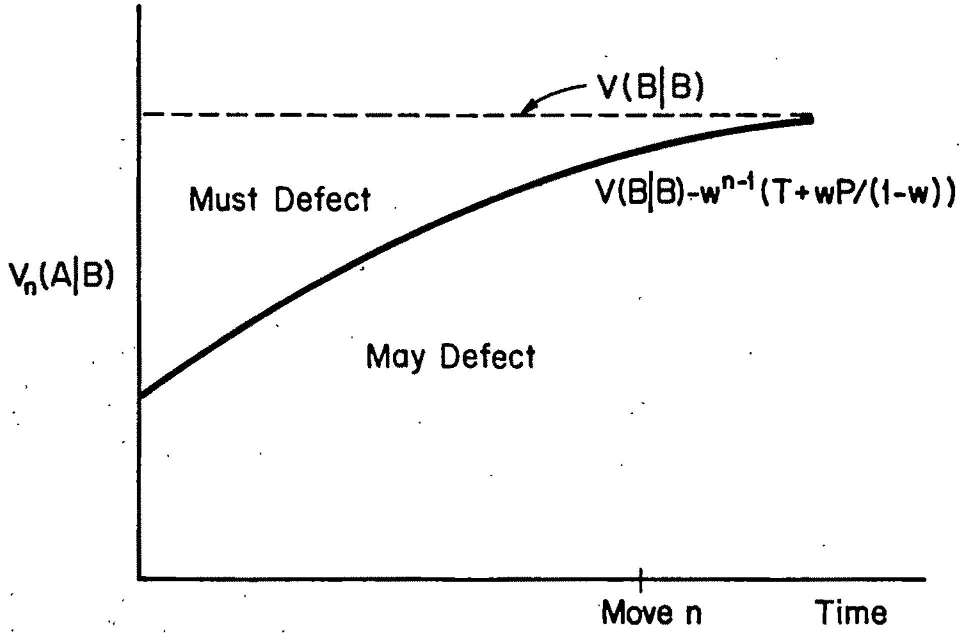
Two additional consequences about collectively stable strategies can be seen from Figure 2. First, as long as the other player has not accumulated too great a score, a strategy has the flexibility to either cooperate or defect and still be collectively stable. This flexibility explains why there are typically many strategies which are collectively stable. The second consequence is that a nice rule (one which will never defect first) has the most flexibility since it has the highest possible score when playing an identical rule. Put another way, nice rules can afford to be more generous than other rules with potential invaders because nice rules do so well with each other.

The flexibility of a nice rule is not unlimited, however, as shown by the following theorem. In fact, a nice rule must be *provoked* by the very first defection of the other player, i.e., on some later move the rule must have a finite chance of retaliating with a defection of its own.

THEOREM 4. *For a nice strategy to be collectively stable, it must be provoked by the first defection of the other player.*

PROOF. If a nice strategy were not provoked by a defection on move *n*, then it would not be collectively stable because it could be invaded by a rule which defected only on move *n*.

Besides provocability, there is another requirement for a nice rule to be collectively stable. This requirement is that the discount parameter *w*, be sufficiently large. This is a generalization of the second theorem, which showed that for *TIT FOR TAT* to be collectively stable, *w* has to be large enough. The idea extends beyond just nice rules to any rule which might be the first to cooperate.



Source: Drawn by the author.

Figure 2. Characterization of a Collectively Stable Strategy

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[8] To be precise, $V(B|B)$ must also be specified in advance. For example, if *B* is never the first to defect, $V(B|B) = R/(1-w)$.

THEOREM 5. *Any rule,* B, *which may be the first to cooperate is collectively stable only when* w *is sufficiently large.*

PROOF. If B cooperates on the first move, $V(ALL\ D|B) > T + wP/(1-w)$. But for any B, $R/(1-w) > V(B|B)$ since R is the best B can do with another B by the assumptions that $R>P$ and $R>(S+T)/2$. Therefore $V(ALL\ D|B) > V(B|B)$ is so whenever $T + wP/(1-w) > R/(1-w)$. This implies that $ALL\ D$ invades a B which cooperates on the first move whenever $w<\frac{T-R}{T-P}$. If B has a positive chance of cooperating on the first move, then the gain of $V(ALL\ D|B)$ over $V_1(B|B)$ can only be nullified if w is sufficiently large. Likewise, if B will not be the first to cooperate until move n, $V_n(ALL\ D|B) = V_n(B|B)$ and the gain of $V_{n+1}(ALL\ D|B)$ over $V_{n+1}(B|B)$ can only be nullified if w is sufficiently large.

There is one strategy which is *always* collectively stable, that is regardless of the value of w or the payoff parameters T, R, P, and S. This is $ALL\ D$, the rule which defects no matter what.

THEOREM 6. ALL D *is always collectively stable.*

PROOF. $ALL\ D$ is always collectively stable because it always defects and hence it defects whenever required by the condition of the characterization theorem.

This is an important theorem because of its implications for the evolution of cooperation. If we imagine a system starting with individuals who cannot be enticed to cooperate, the collective stability of $ALL\ D$ implies that no single individual can hope to do any better than just to go along and be uncooperative as well. A world of "meanies" can resist invasion by anyone using any other strategy—provided that the newcomers arrive one at a time.

The problem, of course, is that a single newcomer in such a mean world has no one who will reciprocate any cooperation. If the newcomers arrive in small clusters, however, they will have a chance to thrive. The next section shows how this can happen.

The Implications of Clustering

To consider arrival in clusters rather than singly, we need to broaden the idea of "invasion" to include invasion by a cluster.[9] As before, we will suppose that strategy B is being used by virtually everyone. But now suppose that a small group of individuals using strategy A arrives and interacts with both the other A's and the native B's. To be specific, suppose that the proportion of the interactions by someone using strategy A with another individual using strategy A is p. Assuming that the A's are rare relative to the B's, virtually all the interactions of B's are with other B's. Then the average score of someone using A is $pV(A|A) + (1-p)V(A|B)$ and the average score of someone using B is $V(B|B)$. Therefore, a p-*cluster of* A *invades* B if $pV(A|A) + (1-p)V(A|B) > V(B|B)$, where p is the proportion of the interactions by a player using strategy A with another such player. Solving for p, this means that invasion is possible if the newcomers interact enough with each other, namely when

$$p > \frac{V(B|B)-V(A|B)}{V(A|A)-V(A|B)}. \qquad (3)$$

Notice that this assumes that pairing in the interactions is not random. With random pairing, an A would rarely meet another A. Instead, the clustering concept treats the case in which the A's are a trivial part of the environment of the B's, but a nontrivial part of the environment of the other A's.

The striking thing is just how easy invasion of $ALL\ D$ by clusters can be. Specifically, the value of p which is required for invasion by *TIT FOR TAT* of a world of $ALL\ D$'s is surprisingly low. For example, suppose the payoff values are those of Figure 1, and that $w = .9$, which corresponds to a 10-percent chance two interacting players will never meet again. Let A be *TIT FOR TAT* and B be $ALL\ D$. Then $V(B|B) = P/(1-w) = 10$; $V(A|B) = S + wP/(1-w) = 9$; and $V(A|A) = R/(1-w) = 30$. Plugging these numbers into Equation (3) shows that a p-cluster of *TIT FOR TAT* invades $ALL\ D$ when $p>1/21$. Thus if the newcomers using *TIT FOR TAT* have any more than about 5 percent of their interactions with others using *TIT FOR TAT*, they can thrive in a world in which everyone else refuses ever to cooperate.

In this way, a world of meanies can be invaded by a cluster of *TIT FOR TAT*—and rather easily at that. To illustrate this point, suppose a business school teacher taught a class to give cooperative behavior a chance, and to reciprocate cooperation from other firms. If the students did, and if they did not disperse too widely (so that a sufficient proportion of their interactions were with others from the same class), then the students would find that their lessons paid off.

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[9] For related concepts from biology, see Wilson (1979) and Axelrod and Hamilton (1981).

When the interactions are expected to be of longer duration (or the time discount factor is not as great), then even less clustering is necessary. For example, if the median game length is 200 moves (corresponding to $w = .99654$) and the payoff parameters are as given in Figure 1, even one interaction out of a thousand with a like-minded follower of *TIT FOR TAT* is enough for the strategy to invade a world of *ALL D*'s. Even when the median game length is only two moves ($w = .5$), anything over a fifth of the interactions by the *TIT FOR TAT* players with like-minded types is sufficient for invasion to succeed and cooperation to emerge.

The next result shows which strategies are the most efficient at invading *ALL D* with the least amount of clustering. These are the strategies which are best able to discriminate between themselves and *ALL D.* A strategy is *maximally discriminating* if it will eventually cooperate even if the other has never cooperated yet, and once it cooperates it will never cooperate again with *ALL D* but will always cooperate with another player using the same strategy.

THEOREM 7. *The strategies which can invade* ALL D *in a cluster with the smallest value of* p *are those which are maximally discriminating, such as* TIT FOR TAT.

PROOF. To be able to invade *ALL D,* a rule must have a positive chance of cooperating first. Stochastic cooperation is not as good as deterministic cooperation with another player using the same rule since stochastic cooperation yields equal probability of S and T, and $(S+T)/2<R$ in the Prisoner's Dilemma. Therefore, a strategy which can invade with the smallest p must cooperate first on some move, n, even if the other player has never cooperated yet. Employing Equation (3) shows that the rules which invade B=*ALL D* with the lowest value of p are those which have the lowest value of p^*, where $p^* = [V(B|B) - V(A|B)]/[V(A|A) - V(A|B)]$. The value of p^* is minimized when $V(A|A)$ and $V(A|B)$ are maximized (subject to the constraint that A cooperates for the first time on move n) since $V(A|A) > V(B|B) > V(A|B)$. $V(A|A)$ and $V(A|B)$ are maximized subject to this constraint if and only if A is a maximally discriminating rule. (Incidentally, it does not matter for the minimal value of p just when A starts to cooperate.) *TIT FOR TAT* is such a strategy because it always cooperates for n=1, it cooperates only once with *ALL D,* and it always cooperates with another *TIT FOR TAT.*

The final theorem demonstrates that nice rules (those which never defect first) are actually better able than other rules to protect themselves from invasion by a cluster.

THEOREM 8. *If a nice strategy cannot be invaded by a single individual, it cannot be invaded by any cluster of individuals either.*

PROOF. For a cluster of rule A to invade a population of rule B, there must be a $p<1$ such that $pV(A|A) + (1-p)V(A|B) > V(B|B)$. But if B is nice, then $V(A|A) < V(B|B)$. This is so because $V(B|B) = R/(1-w)$, which is the largest value attainable when the other player is using the same strategy. It is the largest value since $R>(S+T)/2$. Since $V(A|A) < V(B|B)$, A can invade as a cluster only if $V(A|B) > V(B|B)$. But that is equivalent to A invading as an individual.

This shows that nice rules do not have the structural weakness displayed in *ALL D. ALL D* can withstand invasion by any strategy, as long as the players using other strategies come one at a time. But if they come in clusters (even in rather small clusters), *ALL D* can be invaded. With nice rules, the situation is different. If a nice rule can resist invation by other rules coming one at a time, then it can resist invasion by clusters, no matter how large. So nice rules can protect themselves in a way that *ALL D* cannot.[10]

In the illustrative case of the Senate, Theorem 8 demonstrates that once cooperation based on reciprocity has become established, it can remain stable even if a cluster of newcomers does not respect this senatorial folkway. And Theorem 6 has shown that without clustering (or some comparable mechanism) the original pattern of mutual "treachery" could not have been overcome. Perhaps these critical early clusters were based on the boardinghouse arrangements in the capital during the Jeffersonian era (Young, 1966). Or perhaps the state delegations and state party delegations were more critical (Bogue and Marlaire, 1975). But now that the pattern of reciprocity is established, Theorems 2 and 5 show that it is collectively stable, as long as the biennial turnover rate is not too great.

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[10] This property is possessed by population mixes of nice rules as well. If no single individual can invade a population of nice rules, no cluster can either.

Thus cooperation can emerge even in a world of unconditional defection. The development cannot take place if it is tried only by scattered individuals who have no chance to interact with each other. But cooperation can emerge from small clusters of discriminating individuals, as long as these individuals have even a small proportion of their interactions with each other. Moreover, if nice strategies (those which are never the first to defect) eventually come to be adopted by virtually everyone, then those individuals can afford to be generous in dealing with any others. The population of nice rules can also protect themselves against clusters of individuals using any other strategy just as well as they can protect themselves against single individuals. But for a nice strategy to be stable in the collective sense, it must be provocable. So mutual cooperation can emerge in a world of egoists without central control, by starting with a cluster of individuals who rely on reciprocity.

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Political and Mathematical Perspectives on Educational Equity

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A selective review of the literature on equal educational opportunity supports an argument for explicit use of normative models in research. The argument has two parts. First, we show that confusion over the Coleman Report (1966) was due to a mismatch between the implied reform objective and the statistical approach. The objective was to reduce black/white differences in educational achievement, but the statistics explained individual differences in achievement. Second, we partly develop and apply a normative dynamic model of the objectives of school integration over time. Normative dynamic models could be used in conjunction with behavioral data to operationalize theoretical concepts such as accountability, symbolic politics and non-decisions.

Critics say that social scientists fail to make values explicit in their research (Meehan, 1973; Storing, 1962; Walker, 1966). The first section of this article illustrates the problem with a discussion of the Coleman Report (1966). We emphasize that the contrasting findings result from differences in normative/mathematical frameworks, not from differences in data or raw statistics, such as means, standard deviations, or regression coefficients. Indeed, the contrasting findings that we present in Tables 1 and 2 below are generated by the same regression equations.[1] One of the most disturbing features of the Coleman Report episode is the failure of peer review. Despite extensive reanalysis and discussion, critics generally did not attribute the report's counter-intuitive findings to a mismatch between reforms under consideration (to eliminate the black/white achievement gap), and the statistical approach used in the analysis (to explain individual differences in achievement).

The second section of the article partially develops and applies a normative, dynamic model of reform expectations for integration, as of 1966. The model is intended to operationalize political constraints and the effects of desegregation on social relationships over time. At the close of the section, we touch on possible applications of this sort of model in policy analysis, program evaluation, implementation and basic research. From a theoretical perspective, the normative model might be used with behavioral data to operationalize Edelman's concept of symbolic politics (1971, 1977) and Bachrach's and Baraetz's concept of non-decisions (1962, 1963). Also, because the reform model predicts changes in the rules of the game governing "who gets what," as these are reflected in regression coefficients, it does not have the conservative implications that are often attributed to strictly empirical analyses of equity issues (Aaron, 1978, p. 98).

The following symbols will be used throughout:

A = Achievement as measured by conventional tests;
S = School quality indicators;
C = Class or social background;
M = Merit or genuine talent;
p = Raw regression coefficients for the black population;
q = Raw regression coefficients for the white population;
b = Black;
w = White;
i = Individual;
t = Time.

The author is a Program Officer at the National Science Foundation. However, all opinions and conclusions are those of the author and do not reflect the views of NSF.

[1] Speaking of the data presented in Table 1, McPartland and Sprehe say (1973, pp. 323): "The variables for these regression analyses are the same variables which were used in the major regression analyses of the 1966 report." They list these variables by grade. Coleman (1972, p. 160), who produced Table 2 after the report, says that it employs measures of the sort used in the report scaled up, and he lists the number of variables that apply in each of four categories. The variable numbers in Coleman's categories are the same as those reported by McPartland and Sprehe. Of course, both tables use the same data. Thus, while there may be modest differences in the equations that undergird the two tables, they are substantially the same. The contrast in findings in the tables stems from the different weights that are applied to the raw regression coefficients. This is explained at length in the text.

The Coleman Episode, or What Was the Question?

Three normative perspectives on educational equity are especially useful in understanding the controversy generated by the study of equal educational opportunity conducted by Coleman and his colleagues (1966). These are the objectives of (1) equality of schools for blacks and whites, (2) equality of average achievement for blacks and whites, and (3) equality of individual achievement. Each of these objectives is defined statistically in this section. (See Miller, 1977, 1981, for graphic representations.)

We show that the confusion generated by the Coleman study stems from the fact that it was widely thought to address the first two policy objectives, but actually it addressed the first and third. Misapplied as an explanation of achievement differences between the races, explanations of individual differences within each race contradict the conventional wisdom about the importance and effects of schools. In fact, schools explain a substantial portion of the black/white achievement gap, and only a trivial portion of individual achievement differences.

It must be stressed again, that contrasts in the explanations of between-race and within-race differences in achievement do not reflect contradictory findings of fact. The data, variance, variables and form of the equation can all be held constant, while explanations diverge, owing to the mathematical frameworks that are used to interpret the raw statistics. These mathematical frameworks, in turn, embody contrasting assumptions, values, and policy perspectives.

Equal Schools for Blacks and Whites. The normative goal is to eliminate past and present manifestations of racial discrimination so that schools for the minority will be equal in quality to schools for the majority. Some variance in school quality is expected within each race, but the reform goal requires that on the average schools be equal. The end result should be $\overline{S}_w = \overline{S}_b$ for the various S_1, $S_2, \ldots S_n$ that are significant indicators of school quality. Graphically, the problem is that the distribution (and mean) of school resources for blacks is at a lower level than that for whites. Equalization requires merging the two distributions, which could be achieved by raising resources for blacks, lowering resources for whites, or some combination of the two.

There are at least two views on what constitutes school equality. In breaking with the tradition of "separate but equal" schools, the 1954 *Brown* decision established the principle that segregated schools are "inherently" unequal. From this perspective, Coleman's study found extensive school inequality—that is, racial segregation. Most American school children were attending schools where children of their own color constituted a large proportion of the student body (National Institute of Education, 1976, pp. 23-24).

On the other hand, to the surprise of Coleman and others, the data showed that school spending and the material qualities of the schools attended by blacks and whites were quite similar. Mosteller and Moynihan say (1972, p. 11):

> The nation had, within geographic regions, come close to supplying equal, though largely separate, facilities to Negroes and whites according to the classical notion of traditional physical inputs. Insofar as that is true, the country, having chosen a goal in a social problem, had come close to reaching it.

The report also found that the remaining differences between the schools, including segregation, had modest effects on achievement. It is this latter finding that needs to be put in the context of the normative framework that produced it.

Equal Achievement for Blacks and Whites. The Coleman Report (1966) is credited with changing the operational definition of "equal educational opportunity." Before the report, the objective "was measured in terms of school inputs, including racial mixture"; after the report "it became increasingly the practice, even the demand, that equality be measured by school outputs; that is to say, by results of tests of academic achievement" (Mosteller and Moynihan, 1972, p. 6). In the words of analysts for the National Institute of Education (1976, pp. 5-6), "Better educational opportunities for blacks became associated with closing the educational achievement gap between blacks and whites." The graphic statement of the equal average achievement objective is the same as for the equal school objective, but the variable is achievement, and the equity goal is $\overline{A}_w = \overline{A}_b$, or more realistically, given constraints, to minimize the difference in average attainment—Min $(\overline{A}_w - \overline{A}_b)$. Again, equalization can be achieved by raising black achievement levels, lowering white achievement levels, or a combination of both.

Coleman's original analysis plan (1972, pp. 153-55) was to explain lower achievement levels for blacks using equations that related achievement to race-linked differences in school characteristics, controlling for other factors. Regression coefficients in an equation which related A (achievement) to indicators of S (school quality) for the black population could then be used as weights to estimate the effects on black achievement of replacing the "levels of input resources for the average black in the region with those of the white in the same region," while holding C (home background) at black averages. This analysis produces two types of findings. First, it provides a measure of "effective inequality of opportunity": the average gain in black achievement resulting from exposure to all school resources enjoyed by whites. Second, by selectively changing some school variables while keeping others at average levels, the researcher can identify school equalization policies that are expected to raise black achievement most rapidly.

Operationally, then, the measure of unequal opportunity resulting from black/white school differences would be $\bar{A}_{b,t+n} - \hat{A}_{b,t}$, where $\hat{A}_{b,t+n}$ is the estimated achievement of blacks after a completed reform (t+n) that exposes them to the school advantages enjoyed by the average white. The proposed measure of effective inequality of opportunity is equal to the sum of the products of the school variable raw regression coefficients in the black equation ($p_{as.c}$) and the average black/white school variable differences:[2]

$$\hat{\bar{A}}_{b,t+n} - \bar{A}_{b,t} = \sum_{s=1}^{n} p_{as.c}(\bar{S}_{w,t} - \bar{S}_{b,t}).$$

Also, the $p_{as.c}$ $(\bar{S}_w - \bar{S}_b)$ that makes the largest contribution in explaining effective inequality of opportunity would be the key target for equalization.

Coleman (1972, p. 155) did not carry out the analysis as planned. McPartland and Sprehe (1973, p. 323) did execute Coleman's original concept using the same data and variables that were employed in the major regressions of the 1966 report. Consistent with the unconstrained objective stated above to minimize the difference between black and white achievement by any means—gains for blacks, or losses for whites—McPartland and Sprehe calcuated impacts for both races. Thus in addition to the equations estimating potential gains for the average black illustrated above, equations estimate potential losses to the average white from exposure to conditions faced by the average black:

$$\hat{\bar{A}}_{w,t+n} - \bar{A}_{w,t} = \sum_{s=1}^{n} q_{as.c}\ (\bar{S}_{w,t} - \bar{S}_{b,t}).$$

Results of the McPartland and Sprehe reanalysis are presented below in terms of standardized units of achievement. Figures in the body of the table are changes in achievement that are predicted to result from black exposure to school characteristics experienced by the average white in the same region, and from white exposure to school characteristics experienced by the average black.[3]

The predicted changes in achievement indicate that inequalities between schools are important in explaining the achievement gap between blacks and whites. Changes in achievement resulting from school equalization are greatest for whites in the sixth and ninth grades. For example, the .61 loss of a standard unit of achievement predicted for sixth-grade southern whites who attend typical schools for southern blacks accounted for 57 percent of the gap in racial achievement in that region and grade.[4] If southern whites went to the "separate but equal" schools of blacks, the estimates say that their achievement advantage over southern blacks would have been half as great as it was in the mid-1960s. The table also tells us that the white advantage in achievement is largely attributable to differences in teacher characteristics.

Another striking feature of the estimates is the contrast between the sensitivity of whites and blacks to school characteristics in the sixth and ninth grades. Whites are generally more sensitive to changes. In contrast to the largest loss for whites (.79 of a standard unit of achievement), the largest gain in predicted achievement for blacks receiving white school inputs is .33 of a standard unit, or only 24 percent of the twelfth-grade racial achievement gap in the South. While the generalization from the sixth and ninth grades is contradicted by a reversal in the twelfth grade in this table, results for the sixth and ninth grades are believable because they are consistent with the literature, which shows that raw regression coefficients calculated on white populations are generally higher than those calculated for blacks, when the dependent variable is achievement or attainment (Miller, 1981, with supporting evidence from Jensen, 1968, pp. 59-66; 1972, p. 161; Jencks, 1979, pp. 192-212; Brown and Saks, 1975, p. 592; Featherman and Hauser, 1978, pp. 313-84; Heyns, 1976). This is true regardless of the input variables. Thus, generally, $p_{as.c} < q_{as.c}$.

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[2] Operationalization of this approach to produce results found in Tables 1 and 3 would require an additional step. The products would be standardized by the standard deviation of the achievement variable.

[3] For example, the figure .61 in the top left of the table means that the expected change in average white student achievement owing to a simulated "reassignment" of whites to typical black schools is a drop of 61 percent of one standard deviation of achievement. Note that the response of blacks in the same grade and region appears to be of smaller magnitude. Since the absolute difference in inputs, $|\bar{S}_w - \bar{S}_b|$, that is multiplied by each regression coefficient is the same in both estimates, the primary explanation for the larger change predicted for whites is larger, (on average) raw regression coefficients in the white equation. In fact, in all but the twelfth grade northern sample, the standard deviations used to standardize the sum of the products are larger in the white samples. So these figures in Table 1 make the difference between raw regression coefficients that generally favors whites ($p < q$) appear to be smaller than it actually is.

[4] The percentage of the achievement gap that is explained is calculated by dividing standardized achievement changes in the table by the difference between black/white means in the appropriate region and grade, after standardizing that difference by dividing it by the standard deviation of achievement within the group for which the equation was estimated. The means and variance figures for achievement are from Coleman's statistical appendix (1966).

In contrast to McPartland and Sprehe (1973), who explained average differences in achievement between the races in terms of average differences in school quality, Coleman's report (1966) explained individual differences in achievement in terms of individual exposure to school and home advantages. The normative political perspective that corresponds most closely to the Coleman analytic approach is, as we shall see in the next sub-section, the equalization of individual differences in achievement. The large discrepancies in the findings from the between-race and individual modes of analysis is evident when one compares a summary table prepared by Coleman (1972, p. 160) with the table above. Figures in the cells of Table 2 are measures of the unique contribution of school factors and family factors to variance in individual achievement.

Although variables and equations for the two tables are the same (see n. 1), the explanation of achievement provided in Table 2 contrasts with that in Table 1. Table 2 indicates that school characteristics explain very little of the variance in

Table 1. Inequalities Due to Race

Subgroups Equation Used	School Resources Assigned	Change in Expected Achievement Due to Reassignment of:			
		Facilities	Teachers	Student Body	Total School Resources
White South	BS				
6		–.01	–.59	–.01	–.61
9		.00	–.70	–.08	–.79
12		.01	–.13	–.12	–.25
Average of three grades		.00	–.47	–.07	–.55
White North	BN				
6		–.04	–.29	–.10	–.43
9		.00	–.30	–.11	–.40
12		.03	–.13	–.08	–.18
Average of three grades		.00	–.24	–.10	–.34
Black South	WS				
6		–.02	.18	.09	.25
9		–.03	.14	.18	.29
12		.00	.16	.17	.33
Average of three grades		–.02	.16	.15	.29
Black North	WN				
6		.02	.04	.13	.19
9		.06	.02	.11	.19
12		.00	.10	.15	.25
Average of three grades		.03	.05	.13	.21

Source: The table is taken directly from James McPartland and J. Timothy Sprehe, "Racial and Regional Inequalities in School Resources Relative to Their Educational Outcomes," *Social Science Research* 2 (1973), 321-32. It is their reanalysis of data from James S. Coleman, *Equality of Educational Opportunity* (Washington, D.C.: U.S. Government Printing Office, 1966).

Key: BS = Black South
BN = Black North
WS = White South
WN = White North

Table 2. Inequalities Due to Individual Opportunities within Categories of Race and Region

	Black North	Black South	White North	White South
Grade 12				
Family	.067	.119	.133	.144
Facilities and curriculum	.018	.009	.014	.007
Teacher	.016	.026	.009	.002
Student body	.046	.078	.008	.013
R^2	.146	.232	.165	.166
Grade 9				
Family	.065	.098	.160	.183
Facilities and curriculum	.017	.046	.010	.004
Teacher	.012	.014	.010	.010
Student body	.027	.053	.006	.005
R^2	.121	.211	.186	.203
Grade 6				
Family	.086	.137	.135	.179
Facilities and curriculum	.002	.033	.004	.003
Teacher	.024	.023	.007	.011
Student body	.021	.021	.017	.006
R^2	.132	.213	.163	.199

Source: This table is taken directly from James S. Coleman, "The Evaluation of Equality of Educational Opportunity," in Frederick Mosteller and Daniel P. Moynihan (eds.), *On Equality of Educational Opportunity* (New York: Vintage Books, 1972). Only the title is changed from the original. The original title was "Unique contributions to variance in verbal achievement (scaled up to sum to R^2 in each regression) as measures of the importance of each of four clusters of variables: family background (6 variables), school facilities and curriculum (11 variables), teacher characteristics (7 variables), and student-body characteristics (5 variables), blacks and whites in North and South, Grades 12, 9, 6."

achievement, and teachers do not stand out as having the greatest influence on white student achievement. Also in contrast with Table 1, school variables explain more of the variation in achievement for blacks than for whites. This finding in Table 2 makes it appear that blacks are more sensitive to school variables, as Coleman reported (1966, p. 317). However, raw regression coefficients are better indicators of sensitivity than are measures of variance explained (Hanushek and Kain, 1972, pp. 135-36), and we can infer from Table 1 that the regression coefficients are generally larger for whites than they are for blacks (see n. 3).

If the raw statistics—regresssion coefficients, means, standard deviations—behind the two tables are the same, what accounts for the different explanations of achievement? As noted above, the tables provide answers to two different questions. The McPartland and Sprehe analysis asks and answers questions about the causes of achievement differences between the races. The Coleman analysis asks and answers questions about individual achievement differences within the races. Both studies address a common total distribution of individual differences, but the first focuses on the relatively small between-race portion of that distribution and the second focuses on the relatively large within-race portion.

Although the studies are logically and statistically compatible, the reader who thought that both analyses explained achievement in an absolute sense would conclude that social scientists had come up with conflicting findings. One study found that schools had substantial effects, while the other found they had inconsequential effects; one study found that schools were more important for whites, while the other found schools more important for blacks; and one study found that teacher quality was the most important school factor for whites, while the other found other school factors equally important. But when these findings are placed in the context of their normative/statistical frameworks, they do not conflict. Rather, the first study explains the achievement gap between the faces, while the other explains achievement differences among individuals within each race.

Equal Achievement for Individuals. A little over five years after Coleman redefined equal educational opportunity, Christopher Jencks and his colleagues (1972) tried to convince readers to accept yet another substitute for equalizing schools.

Jencks, who must be credited for supplying explicit linkages between values and statistical methods, warns readers that he is not addressing the widely accepted objective of reducing racial disparities in achievement (1972, p. 14):

> The reader should also be warned that we are primarily concerned with inequality between individuals, not inequality between groups. This accounts for much of the discrepancy between our conclusions and those of others who have examined the same data. There is always far more inequality between individuals than between groups. It follows that when we compare the degree of inequality between groups to the degree of inequality between individuals, inequality between groups often seems relatively unimportant. It seems quite shocking, for example, that white workers earn 50 percent more than black workers. But we are even more disturbed by the fact that the best-paid fifth of all white workers earns 600 percent more than the worst-paid fifth. From this viewpoint, racial inequality looks almost insignificant.

Stated statistically, the objective of equalizing individual differences in achievement is to minimize the variance or average sum of the squares of individual achievement around the mean: Min $(A_i - \bar{A}_i)^2/N$ or $Min\ \delta_i^2$. This has the property of placing priority on eliminating the "extremes of wealth and poverty," which is Jencks' political objective (1972, p. 14). In other words, the reform objective is $\delta^2_{i,t+n} < \delta^2_{i,t}$. Graphically, this implies squeezing the distribution of individual differences in achievement so that individuals are more like one another.

Although Coleman (1966) did not embrace the reduction of individual differences in achievement as his policy objective, his statistical approach does so—it is the same as that of Jencks, and explains individual differences in achievement. It is this mismatch between Coleman's apparent articulation of a between-race policy objective and his statistical analysis of within-race variation that is responsible for the confusion over the report. To be fair, however, Coleman and his colleagues never inappropriately claim that they are explaining black/white differences. Readers, not the authors, leap from within-race findings to between-race conclusions.

Why Findings Differ: Value-Laden Assumptions. We have presented two tables that are compatible if readers understand that Table 1 explains achievement differences between the races (in terms of average school quality differences between the races), while Table 2 explains individual achievement differences (in terms of individual exposure to school and background advantages within the races). Different value-relevant assumptions characterize the two statistical approaches and we explore some of these. The nature of the implied normative assumptions illustrates further that it is inappropriate to generalize from studies of individuals to groups.

In defining what is to be explained, the framework for studying individuals makes no distinction between variation in achievement that is independent of social background (and likely to result from merit) and variation in achievement that is associated with race (and likely to result from racial discrimination). Indeed, in order to find the best predictors of individual achievement, rigorous analyses of individual differences (such as Table 2) focus on achievement variation among students of the same social background, and ask schools to explain all differences that are left after statistical adjustments that eliminate (control for) differences owing to race, region and home background. Average school quality indicators account for very little of the variation in achievement that is independent of demographic characteristics, which is to be expected, since the bulk of it lies among family members and individuals who attend the same schools (Mosteller and Moynihan, 1972, pp. 17-18).

In the framework for studying racial differences, by contrast, explanations focus on the roughly one standard deviation of achievement that separates the races, because this is suspected to be the result of discrimination and other environmental disadvantages that are targets of reform. Since variation in measurable school inputs other than facilities is substantial between the races and is statistically related to achievement, schools explain a considerable portion of the $A_w - A_b$ gap. The racial achievement gap is, in turn, only a portion of σ_i^2, which is probably mostly explained by variables that are hard to measure, such as innate intelligence.

A related value-relevant feature of the typical analysis of individual achievement is that all associations between achievement and school characteristics that are correlated with race and class are assumed to be spurious. Coleman (1966, p. 311) says:

> When achievement differs as much as it does between these groups, then to analyze the groups together, without controlling for race or ethnicity of the student, would cause any school characteristics highly associated with race or ethnicity to show a spurious relation to achievement.

In accordance with the assumption that school differences that are correlated with race are not the real causes of achievement differences that are correlated with race, Coleman analyzes variance within each group. In effect, he attributes the disputed variance in achievement to racial background, rather than to schools. A similar assumption justifies the use of "unique contributions to variance" as measures of the "importance" of variables in Table 2 (1972, pp. 160-61), which nets out school effects that are confounded with home background. Thus Coleman's analysis of individual variance fails to ask whether discrimination in the distribution of school resources is responsible for the low achievement of disadvantaged students (Smith, 1972, pp. 239-40). The causal path that represents discrimination in the distribution of school resources ($C \rightarrow S \rightarrow A$) is downgraded by asssumption, which is surprising, given that the study was commissioned to assess "the lack of availability of equal educational opportunities for individuals by reason or race, color, religion or national origin" (*Harvard Educational Review,* 1969, p. 3).

In contrast to studies of individual differences, studies of group differences use the relationships among indicators of race, class and school quality to explain status-linked achievement gaps. Various versions appear in the literature (Jencks, 1979; Duncan, 1968; Bowles, 1973; McPartland and Sprehe, 1973; Mayeske, 1970; Blau and Duncan, 1967). Basically, these studies attribute the relationship between race (or status) and achievement to other variables that are associated with both. Thus the joint variation among achievement, school and race variables that is netted out of explanations of individual achievement becomes the basis for explaining group differences.

Moreover, the version of the between-race method that is responsible for Table 1 (and Table 3 below) follows Coleman's original design (1972, pp. 153-55) and uses regression coefficients for school variables that are estimated with controls for race, class and region. Thus the two tables both employ raw regression coefficients that are calculated with all other variables (controls) in the equation.

Given the common raw regression coefficients, the contrasting findings of Tables 1 and 2 can also be explained in terms of the different weights that are applied to the coefficients. Raw regression coefficients are weighted to provide estimates of the importance of variables in explaining the portion of the distribution of achievement that is addressed by the study. The study of racial differences weights each school variable regression coefficient with the difference between black and white averages for the variable ($\overline{S}_w - \overline{S}_b$). Both the sensitivity of achievement to the school factor and the contrast between majority and minority schools are included in the measure of importance. The study of individual differences weights each school variable regression coefficient with some measure of the variation in exposure to the school characteristic within each race (Werts and Linn, 1969; Coleman, 1972, pp. 159-60). Both the sensitivity of achievement to the school factor and variation in exposure within each race are included in the measure of importance. To paraphrase Coleman, if all whites go to equally good schools, and all blacks go to equally bad schools, then schools will be found to have no importance as explanations of individual achievement differences (1966, p. 310).

As a result of the different weights and the treatment of variance confounded with race, the rank order of variables by policy importance in a study of racial differences tends to follow the rank order of correlations between the school characteristics and race, while just the opposite tends to occur in a study of individual differences. In other words, if the correlation between race and school quality measures is taken to be an indicator of discrimination, then the between-race study is designed to measure the importance of indicators of discrimination, while the within-race study is designed to assess the importance of independent variables, assuming that discrimination does not explain achievement.

Detailed Findings about Black/White Achievement Differences. Table 3, constructed from equations produced by Smith (1972, pp. 331-38) from the Coleman data, allows a more complete exploration of the explanations of racial differences in achievement as of 1966.

Table 3 (bottom) tells us first and foremost that environmental variables can explain black/white differences in scholastic achievement. In the sixth grade, where schools are still relatively homogenous, we find that the average white would lose over half the white advantage over the average black as a result of attending the average black school. If the average white also lost home environment advantages ($S + C$), 81 percent of the remaining achievement advantage would disappear. Clearly, the Coleman data show that environmental factors are able to explain black/white differences in achievement.

Findings also indicate that in 1966 schools alone were important in explaining achievement differences between blacks and whites. In all but the white equation for the twelfth grade, school differences account for substantially more of the achievement gap between the races than does home background. It follows that eliminating school differences remaining between blacks and whites would substantially reduce the black/white achievement gap, even if home environment differences remained. While residual school differences appeared small to Mosteller and Moynihan (1972, p. 11) and others, statistical analysis shows that these differences explained a large portion of the black-white achievement gap.

Alarmingly from a policy perspective, the table also indicates that the achievement of whites would drop roughly twice as much as the achievement of blacks would rise as a result of a complete exchange of school resources in the early school years. While home background variables were far more important for whites than for blacks in 1966, the analysis indicates that they were not strong enough to insulate whites from the effects of significant changes in school quality. These findings, paralleling those of McPartland and Sprehe, suggest that a policy of redistributing existing school resources by moving whites to predominantly black schools and blacks to predominantly white schools (population sizes permitting) might hurt whites more than it would help blacks. This would, however, be an efficient way to achieve the stated goal $\bar{A}_b = \bar{A}_w$.

Despite problems of multicollinearity, which make coefficients unstable when variables in the equation change, it seems useful to use the greater detail of Table 3 to interpret the finding that whites are generally more sensitive to changes in school quality than are blacks. The greater influence of changes in school quality on whites seems to be attributable to variables that refer to race of teacher (18) and race of student (48), which have substantial and contrasting results for blacks and whites. Exposure to majority-black schools has a largely consistent and negative effect on white achievement. But the effects of majority-white schools on black achievement are mixed. If the majority-white student body variable has a positive effect, then the majority-white staff variable

Table 3. Predicted Changes in Standard Units of Achievement for Northern Blacks and Whites That Would Result from Exposure to the Average School and Home Conditions of Members of the Other Race[a]

		Sixth Grade		Ninth Grade		Twelfth Grade	
No.	Variable Name	Black	White	Black	White	Black	White
	FAMILY BACKGROUND FACTOR						
3	Reading Material	+.052	–.077	+.031	–.054	+.016	–.085
4	Items in Home	+.075	–.072	+.025	–.035	+.010	+.000
5	Siblings	+.033	–.058	+.080	–.110	+.058	–.078
6	Structure for Integrity	+.026	–.036	+.037	–.063	+.020	–.021
39	Parents' Education	+.032	–.063	+.028	–.071	+.049	–.066
40	Urbanism	+.000	+.002	–.011	+.019	–.008	+.002
	TOTAL C[b]	+.217	–.304	+.191	–.313	+.144	–.248
	FACILITIES & CURRICULUM FACTOR						
38	Per-Pupil Expenditure	+.001	–.000	–.001	+.007	–.000	+.002
23	Volume/Student	+.004	+.002	–.001	–.003	–.007	+.006
22	Science Lab			+.001	–.002	–.002	–.009
24	Extracurricular Activity			–.002	+.000	+.001	+.001
36	Accelerated Curriculum	+.004	+.000	+.011	+.000	+.007	–.018
25	Comprehensive Curriculum			–.008	+.010	–.001	+.001
35	Tracking			+.005	+.001	+.003	+.000
27	Movement between Tracks			+.002	+.001	–.002	–.001
9	School Size	+.002	+.002	–.001	+.003	+.016	–.011
30	Guidance Counselor			+.004	–.005	–.009	+.031
33	School Location	+.021	–.034	+.006	+.000	–.025	+.033
37	Promotion of Slow Students	+.001	–.001				
	TOTAL S_1	+.033	–.030	+.017	+.013	–.018	+.036
	STUDENT BODY FACTOR						
10	Encyclopedia	+.068	+.015	+.043	–.012	+.045	–.000
26	Student Transfer	–.004	+.016	+.029	–.018	+.027	–.015
31	Attendance	–.039	–.009	+.022	–.059	+.033	–.013
52	College Track			–.032	+.006	+.067	–.033
53	Homework			–.008	+.001	+.027	+.000
55	Teacher Evaluation of Pupils	+.100	–.097				
48	Proportion White	–.021	–.266	+.381	–.250	+.122	–.120
	TOTAL S_2	+.105	–.341	+.435	–.331	+.322	–.181

Continued on next page

Table 3. (continued)

		Sixth Grade		Ninth Grade		Twelfth Grade	
No.	Variable Name	Black	White	Black	White	Black	White
	TEACHER CHARACTERISTICS FACTOR						
11	SES Level	–.000	–.000	+.003	+.000	+.008	+.000
12	Experience	+.003	–.004	+.016	–.004	+.003	+.013
13	Localism	+.079	+.000	+.002	+.003	+.015	+.012
15	Degree	+.009	–.004	–.025	–.007	+.006	–.010
16	Preference	–.007	–.006	+.003	+.027	+.087	–.038
	Student						
17	Verbal	+.006	–.016	+.014	+.007	+.016	–.030
18	Race	+.044	–.165	–.096	–.170	–.078	–.000
	TOTAL S_3	+.134	–.196	–.083	–.145	+.057	–.054
	TOTAL CHANGE	+.489	–.871	+.560	–.776	+.505	–.447
	DUE TO SCHOOL	+.272	–.567	+.369	–.463	+.361	–.198
	DUE TO HOME	+.217	–.303	+.191	–.313	+.144	–.248
	$\bar{A}_w - \bar{A}_b$ in Standardized Units[c]	1.319	1.076	1.159	1.048	1.028	1.060
	% $\bar{A}_w - \bar{A}_b$ explained by schools, S[d]	21	53	32	44	35	19
	% $\bar{A} - \bar{A}$ explained by S + C[d]	37	81	48	74	49	42

Source: The table is calculated from equations provided by Educational Opportunity. Marshall S. Smith, "Equality of Educational Opportunity: The Basic Findings Reconsidered." In Frederick Mosteller and Daniel P. Moynihan (eds.), *On Equality of Educational Opportunity* (New York: Vintage Books, 1972). The data are from James S. Coleman, *Equality of Educational Opportunity* (Washington, D.C.: U.S. Government Printing Office, 1966).

[a] The method used in constructing this table is the same as used by James McPartland and J. Timothy Sprehe, "Racial and Regional Inequalities in School Resources Relative to Their Educational Outcomes," *Social Science Research* 2 (1973): 321-32.

[b] Figures do not add precisely to the totals because of rounding. Totals are correct.

[c] $\bar{A}_w - \bar{A}_b$ in standardized units is the difference of average achievement for the two races standardized by the variation in achievement for that race.

[d] The percent of $\bar{A}_w - \bar{A}_b$ explained is the sum of changes in standardized units over the achievement gap in standardized units.

has a negative effect, and vice versa.

These observations from Table 3 are consistent with the results of case studies of integration. In the short run, racial hostility, stereotyping and other holdovers from segregation work against swift improvement in achievement for blacks (National Institute of Education, 1976, pp. 31-47, 90-92; Crain and Mahard, 1979, p. 707). The prediction that white achievement will drop from integration into majority-black schools is also confirmed. This rarely occurs, because of white parent resistance (Crain and Mahard, 1979, pp. 726-27), but when it does, white students suffer (National Institute of Education, 1976, p. 49).

Also, these data support the *Brown* argument that segregated schools are inherently unequal. Both blacks and whites are predicted to do less well in majority-black schools. For blacks, the effects of otherwise effective schools are dampened by continued discrimination, but whites get full benefit of effective majority-white schools, and the predicted negative effects of shifting to majority-black schools are dramatic. This suggests that the performance of the average majority-black school is substantially inferior to the performance of the average majority-white school.

While suggesting that segregated education is "inherently unequal," the findings raise questions about commonly accepted interpretations of the nature of the disadvantage for blacks. The damage from segregated education does not appear to lodge in the attitudes and self-images of black students. If it did, we would not find a dramatic lag in the achievement of white students who are exposed to majority-black schools. The evidence suggests that the average majority-black school is simply less productive than the average majority-white school. This is an institutional problem, not a psychological problem of black individuals.

Impacts. The Coleman Report had enormous influence (Moynihan, 1969, p. 195). First, it convinced many well-informed educators that families were more important than schools in producing achievement. Seymour Lipset is quoted as saying to Patrick Moynihan: "Guess what Coleman's found? . . . Schools make no difference, families make the difference " (Hodgson, p. 35). As a result of the report, policy instruments and explanations for black-white differences in achievement were sought outside the schools, for example in theories of cultural deprivation (National Institute of Education, 1976, pp. 26-27).

Genetic explanations for black/white differences were also advanced. Arthur Jensen (1972, p. 10) notes that he became interested in genetic rather than environmental explanations of racial differences in intelligence because the Coleman Report

> presents massive evidence that discrepancies in educational achievement by different social class and racial groups are correlated to only a slight degree with inequalities in those variables over which schools traditionally have control. The data made it abundantly clear that biological and social environmental factors associated with social class, race and family background accounted for most of the variance in intellectual ability and scholastic performance.

Collins (1978, pp. 172-73) talks about the effects of the report on the movement toward school integration:

> The report stressed two main points. First, the single greatest determinant of a child's academic performance is family background. And, second, if a minority pupil from a home lacking educational strength studies with schoolmates who have strong educational backgrounds, the minority child's achievement will improve—without effecting a negative response in the performance of other children. Thus increasing contact between white and minority students, from a purely educational viewpoint, was not a zero-sum game. . . . The publication of the Coleman Report in 1966 gave fresh impetus to the desegregation drive, which had been stymied by years of southern intransigence. In particular, civil rights groups seized on Coleman's second point in advocating integration through the busing of school children. As a result, the Coleman Report strongly influenced the issuance of the federal busing order of 1970 and was persuasive in subsequent litigation.

But the findings from Coleman's within-race analysis of individual differences in achievement cannot be generalized to explain between-race and between-status differences in achievement. The influential interpretations of the report go beyond its assertions and are largely unsupported. In contrast with the impacts noted above, a between-race analysis of the 1966 Coleman data (Table 3) supports the following general conclusions:

- Environmental factors can and do explain white achievement advantages over blacks;
- School factors explain more of the black/white achievement gap than home background factors;
- In the short run, the achievement of whites who are sent to majority-black schools is expected to drop;
- In the short run, integration will have only a modest, positive effect on black achievement;
- In general, involuntarily segregated black schools are "inherently unequal" because they are less productive than majority-white schools.[5]

Peer Review. The counter-intuitive results of the Coleman Report stirred controversy, and the data were thoroughly reanalyzed (Mosteller and Moynihan, 1972; McPartland and Sprehe, 1973; Averch, 1972; Jencks, 1972; Mayeske, 1970; Harvard Educational Review, 1969). The most influential of these studies were Averch (1972) and Jencks (1972) which, according to Aaron (1978, pp. 72-98), had the effect of reinforcing Coleman's disturbing findings about the ineffectiveness of schools.

Given strong professional norms governing the use of value-relevant assumptions and selections in research, the effect of the reanalyses should have been quite the opposite from that reported by Aaron. Unfortunately, professional standards regarding the use of values in social science are underdeveloped (Strauch, 1976). Thus peer review failed to identify the contradictions between the reformers' policy perspectives and the assumptions required to identify the independent causes of individual differences in achievement. (There were at least two analyses that explained racial differences using the Coleman data, Mayeske, 1970, and McPartland and Sprehe, 1973, but these were not influential.)

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[5] It is worth noting that the "education production function" literature rarely deals with contributions to achievement that result from improved use of fixed resources—for example, from better management of highly qualified teachers so that they, in turn, have more effect on student achievement. In effect, the productivity of the average school is tacitly assumed to be good enough, and policy recommendations to reduce the black/white achievement gap focus on input levels. Some analysts have questioned this approach and have tried alternatives (Klitgaard and Hall, 1973; Levin, 1976). Our normative dynamic model also calls for improved school productivity, because we argue that closing the black/white achievement gap requires that good schools be at least effective in teaching blacks as they are in teaching whites. We should note in this context that the expectation for improved productivity could be applied to all-black schools as well as to integrated schools. In a society devoid of discrimination, attendance at an all-black school would not be a disadvantage.

In one of the influential analyses, Averch (1972) promises to assess the internal and external validity of studies of school effects, including Coleman's, by asking: "Did the researcher use methods appropriate to the problem he addressed?" (p. vii), and "Were results credible in the light of accumulated knowledge?" (p. v). Given the existence of a well-established methodology for studying between-race differences (Blau and Duncan, 1967; Duncan, 1968), the inclusion of between-race studies among the reanalyses (Mayeske, 1970; McPartland and Sprehe, 1973) and Coleman's original plan to execute a between-race study (Coleman, 1972, pp. 153-55), Averch seemed likely to identify problems, but did not do so. Instead, his conclusions about "input/output" studies, which included Coleman's, reiterated findings that schools have little effect, and are less important than social background (Averch, 1972, pp. 44-45). The other influential review (Jencks, 1972, p. 14) advocated the policy goal of equalizing individual differences in achievement and executed an appropriate analysis. Jencks did not, of course, contradict Coleman's major findings, since his statistical approach was roughly the same as Coleman's.[6]

Thus, peer review did not convincingly reveal and correct the failure to match the client's policy question with the research approach. Even today, research-informed individuals generally believe that family and other non-school advantages, independent of schooling, explain the superior educational achievement of whites and upper-class children over the "socially disadvantaged." The entire "Coleman episode" is an embarrassment to social scientists. To avoid similar embarrassments, especially in dealing with questions of equity, social scientists need to develop professional norms for assessing the match between statistical articulations and policy issues.

Toward a Normative Dynamic Model of Integration for 1966

Because studies of racial differences explain the black/white achievement gap, they are useful in identifying potential policy variables to reduce the gap. Analyses of individual variation in achievement are not helpful in identifying the causes of the achievement gap, because they produce a rank-order of variables by importance that is the reverse of what it should be. But, ironically, between-race studies produce poor predictions about the long-term effects of integration, while the apparently non-responsive within-race approach makes reasonable long-term predictions. Actual case studies of integration show that white achievement is largely unaffected by integration, while black achievement is raised modestly over time (Crain and Mahard, 1979, p. 707). This outcome is consistent with the results of studies of individuals (Table 2), but it is out of line with the predictions, from studies of racial differences, that white achievement will drop faster than black achievement will rise (Tables 2 and 3).

Poor predictions suggest that the simple normative model we have used to explain racial differences in achievement must be modified. First, we must add explicit constraints that reflect political pressures and resource limitations. A usual constraint in school equalization is to hold advantaged students "harmless"—that is, to assure no deterioration in their schooling. It is the operation of this constraint that invalidates the predictions from the simple between-race model. White achievement would decline if whites were exposed to school conditions faced by the average black, but political pressures do not allow this to happen (Crain and Mahard, 1979, pp. 726-27). The simple model identifies the probable causes of black/white differences in achievement, but it is a poor policy simulator, because it does not deal with constraints that occur in implementation (see Hargrove, 1975). This point is discussed in more detail below.

To fully represent the reform objective, the model must also contain modified raw regression coefficients that relate achievement to merit, institutional variables, and background characteristics. One goal of integration is to foster equal opportunity—that is, to alter the rules of the game that govern attainment. These processes or rules are reflected in regression coefficients (Duncan, 1968). For example, reflections of discrimination and segregation are seen in the findings of Table 3, which indicate that regression coefficients are lower or mixed for blacks. Specifically, $p_{as.c} < q_{as.c}$ (school multipliers are smaller for blacks), and $p_{ac.s} < q_{ac.s}$ (home background multipliers are smaller for blacks). Also, other research implies that $p_{am} < q_{am}$) multipliers of individual merit are smaller for blacks (National Institute of Education, 1976, pp. 56-57, 86-94, 151-56; Heyns, 1976; Jensen, 1968; Miller, 1981).

If regression coefficients reflect segregation and are targets of reform, then an analysis that treats them as empirical laws makes the conservative assumption that caste and class biases cannot be altered. Thus a normative, dynamic model of integration must include the expectation that regression coefficients as well as school inputs will be equalized (Miller, 1981).

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[6] This statement applies to Jencks, 1972, not Jencks, 1979. *Who gets ahead?* focuses primarily on differences in attainment between groups from different social backgrounds. Results of Jencks' between-race status analyses conform to prior studies using the approach. Jencks does not make sloppy connections between values and research methods.

In the brief sub-sections that follow, we explicitly modify the simple between-race educational reform model to reflect resource constraints and expected changes in the rules governing "who gets what" in the attainment process. We also illustrate possible applications of the normative model.

Constraints on Resource Equalization. The symbolic language of reform implies pure equity objectives—that schools, and ultimately achievement, should be equal for blacks and whites ($\bar{S}_b = \bar{S}_w$ and $\bar{A}_b = \bar{A}_w$). No constraints are stated. But in the specifics of legislation and in implementation, equity objectives are often swamped by constraints—concessions to affluent interests and resource limitations. The result can be "symbolic reform" (Edelman, 1971, 1977). An effective constraint in school reform has been $\bar{S}_{w,t+n} > \bar{S}_{w,t}$—that is, whites must not lose school resources in the reform, even if this slows equalization. Operation of this constraint causes a departure from efficient realization of Min ($\bar{A}_w - \bar{A}_b$). Looking back at Tables 1 and 3, we see that the stated, unconstrained equity goal could be accomplished most swiftly by sending whites to majority-black schools, within the limits of numbers. This would lower white achievement dramatically, raise black achievement slightly, and require minimum commitment of new resources to education.

As a practical matter, reforms to Min ($\bar{A}_w - \bar{A}_b$) are not achieved through redistribution of existing school resources. Rather, in both school finance reform and school integration, attempts are made to maintain high-quality education for advantaged children, and new resources are dedicated to disadvantaged children (see Orfield, 1978). Indeed, studies of the politics of educational reform tell us that it is generally necessary to put together a benefit package so that representatives of advantaged students have some incentive to go along with the "reform" (Garms, Guthrie, and Pierce, 1978, pp. 356-57; Odden, 1978, p. 146). But achieving equalization by committing new resources is inefficient—it takes more time and money than are required to meet the objective. Indeed, if political realities indicate that advantaged students must gain as a result of "reforms" to help the needy, the operational political constraint may be $\bar{S}_{b,t+n}/\bar{S}_{w,t+n} = \bar{S}_{b,t}/\bar{S}_{w,t}$—no equalization of school resources. Moreover, operation of this constraint implies substantial growth of the public sector if the "reform" is pursued.

Limits involving commitments to professional interest groups, as well as other factors, further constrain achievement of equity goals. Because many of these resource constraints can be anticipated at the time a reform is mandated, policy analysts should lay out an expected timetable for progress toward school equalization and reductions in the black/white achievement gap. If constraints reduce expected school equalization to insignificance, it is misleading to justify expenditure increases in terms of "equal opportunity" to help blacks achieve parity with whites.

Rules Governing "Who Gets What." Historically, discrimination kept most blacks in second-class occupations, regardless of individual merit, talent, and contribution to the economy. Thus, whites were differentiated according to their potential for society-wide leadership, while blacks were prepared for leadership within the oppressed black caste. One result was that white educational achievement ran from very low to very high, while most blacks were bunched together at low-to-moderate levels (Jensen, 1972, p. 161).

If we assume an equal distribution of genetic ability between the races, then ending caste discrimination requires equal differentiation among blacks and whites according to their talent (Thurow, 1972). High-ability blacks must achieve leadership at the same rates as equally able whites. These expectations about changes in the rules of the game that accompany desegregation must be incorporated in our statement of the reform objectives of school integration. In addition to Min ($\bar{S}_w - \bar{S}_b$) (redistribute resources) we must Min ($q - p$) (reduce institutional barriers to upward mobility). If raw coefficients continue to be lower or more mixed for blacks as compared to whites, achievement equalization ($\bar{A}_b = \bar{A}_w$) cannot be attained despite input equalization ($\bar{S}_b = \bar{S}_w$ and $\bar{C}_b = \bar{C}_w$).

More specifically, in designing a normative model of integration for the 1960s, we would insist that white expectations for black achievement go up, so that exposure to good majority-white schools has strong favorable effects on blacks (National Institute of Education, 1976, pp. 79-110). This objective implies $p_{as} > q_{as}$ for indicators of school quality—that is, black achievement should be at least as sensitive to good schools as white achievement. Also, successful black parents should have at least the same ability to boost the educational achievement of their children as successful white parents. Thus, regression coefficients with family background would have to go up so that $p_{ac} > q_{ac}$. (Jencks, 1979, pp.

192-212, indicates that this is one area which has shown substantial improvement since the 1960s.) Finally, and most importantly, individual blacks who have the same innate ability as whites must be encouraged to achieve at levels that are at least equal. Thus regression coefficients that relate measures of learning ability to achievement have to go up. The objective is $p_{am} > q_{am}$. (Learning ability is not measured in the Coleman data.)

Applying the Model. A normative, dynamic model for integration should be useful in policy analysis, program evaluation, implementation and basic research on political change. Some examples of uses that might be made of such a model are included below.

The research applications of a normative model of integration include explaining the causes of black/white differences in achievement. Results in Table 3 suggest that the findings as of 1966 point primarily to school and attainment process disadvantages. While results are not definitive in terms of causal effects, they identify the probable explanations of the black/white achievement gap.

Second, policy analysis (taking implementation into account) could be used to specify expectations for reform, year by year, given constraints. Expectations would involve changes in both school factor levels and regression coefficients. Political constraints, such as protecting white students' advantages, should be taken into account explicitly, along with total resource limits.[7] This would provide a realistic schedule for input equalization. Political pressures that constrain expectations for changes "in the rules of the game," or raw regression coefficients, should also be specified. For example, if schools intend to protect affluent whites of modest talent from displacement by more talented blacks in college-bound classes, projected rates of upward mobility for blacks will be depressed, unless total college attendance increases dramatically.

Timetables for input and mobility equalization produced by policy analysis could be used in program evaluation, and in studies of implementation and accountability. For example, progress predicted by a constrained reform model could be compared to actual progress to monitor implementation. As predicted by a realistically constrained model, progress toward school equalization would be very slow.

Alternatively, critics of the status quo could model the reform without constraints to point out the degree to which policies to equalize achievement for blacks are "symbolic." This would be easy to do in the case of integration. For example, we noted that efficient achievement of $\bar{A}_b + \bar{A}_w$ would result from redistribution of existing school resources—say, two-way busing—which implies $\bar{A}_{w,t+1} < \bar{A}_{w,t}$ (a drop in achievement for whites) in the short run. The expected cost and progress toward $\bar{A}_b = \bar{A}_w$ under this efficient reform could be compared to the observed cost and progress of the actual, politically constrained, reform. Public sector costs above the efficient level would be attributed to maintaining the status quo or conservatism, not to equalization. Also, persistence of $\bar{A}_w - \bar{A}_b$ at pre-reform levels could be attributed to the maintenance of white advantages in schooling.

In addition to monitoring and assessing implementation, normative, dynamic models might be used to encourage reform by suggesting performance targets. Some schools can be expected to be "ahead of their time" in achieving integration. Parameters for indicators of merit-recognition could be calculated within these schools. Results should show more efficient use of inputs and black student assets, and would be used to target $p_{t+1} > p_t$ (a rise in regression coefficients in black equations) for less progressive schools. This technique could be used to encourage greater efficiency in the use of resources in majority-black as well as integrated schools, because 1966 and later data show that institutions that serve the disadvantaged are not as productive in their use of resources (parents, teachers, student skills) as are institutions that serve the advantaged.

Finally, normative dynamic models could be employed in basic research on political change. We have hinted at their use in identifying "symbolic reforms" and in explorations of the causes of public sector growth. In addition, the failure of policy makers (and policy analyses) to seriously consider achieving equity through redistribution of existing resources from whites to blacks may be an example of a "non-decision."

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[7] In a sense, the Coleman Report did take constraints into account, but it did so implicitly. The implicit constraint was that there would be no radical change in the distribution of school resources. After reform, schools would continue to be uniform and fairly good for whites, but would vary more for blacks, due to black attendance at majority-white schools. In other words, analysis of variance in individual achievement within each race "simulates" a "reform" under conditions of no significant redistribution of school resources that would hurt whites. While this outcome isn't what reformers say they have in mind, it captures their behavior. The damage from Coleman's use of implicit, rather than explicit, constraints in policy analysis was the apparent (false) finding that the major causes of the black/white achievement gap were genetic and social, not institutional (see Jensen, 1972, p. 10).

Implications for Political Science

Our review of the literature on educational equity lends support to two observations. First, political scientists are not fulfilling their unique responsibility among social scientists to develop empirical theories, concepts and measures that incorporate political objectives and realities into policy analyses. Second, the failure stems from the fact that political ideas, such as equity and accountability, cover more behavioral possibilities than can be observed in a given locality or point in history, yet empirically oriented social scientists try to limit research to the study of observed events (Merelman, 1968; Wolfinger, 1971).

More specifically, review of the interdisciplinary literature on educational equity indicates that social scientists have difficulty interpreting empirical methods and findings within appropriate political contexts. Political scientists fail to do this in their own work (Wahlke, 1979), which leaves a gap for all of social science. Thus, as we saw above, Coleman's otherwise sophisticated study of equal educational opportunity failed to deal explicitly, rigorously and appropriately with political goals and constraints.

While supporting Wahlke's excellent criticisms (1979), we advocate a direction for political science that differs from his recommendation to study pre-programmed, genetically determined individual behavior. Along with building up political theory from biology and basic human drives, we recommend a top-down approach that measures concepts like "symbolic politics," "accountability" and "non-decisions" by comparing expectations for reform with observed events.

We advocate this partially normative approach because we believe that human beings can imagine new social relations that are not yet true and may never be true, and that these visions, executed through political institutions, are the driving force of history. We also believe that advantaged groups turn to political institutions to preserve and enhance their status. Thus, to be understood, current behavioral outcomes must be viewed in the context of reformers' visions, legal entitlements, and conservative forces. Equally important, the long-run future must be predicted from knowledge of what is wanted, as well as of what is.

From this perspective, we recommend that normative, dynamic, models be used to quantify moral visions, foreign examples, and empirically referenced political theories. Such models would have use in basic as well as applied research. In applied research they would forecast desired changes in the distribution of resources with and without constraints expected in implementation. One use would be to clarify goals and encourage goal achievement. In basic research, comparisons of the normative expectations of various interests to actual events could be used to identify political winners, independent of symbolic statements about political outcomes. Where political constraints negate the stated policy, lack of political accountability could be identified. Growth of the public sector resulting from actions that maintain established advantages and undercut progress toward equity could be properly attributed to conservatism, rather than to reform. Finally, empirical research, both basic and applied, could be freed from the conservative bias that results from exclusive reliance on observed relationships.

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The Allocation of Federal Aid Monies:
The Synthesis of Demand-Side and Supply-Side Explanations

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Previous research on the distribution of federal aid monies has been dominated by the donor's perspective. Different distribution formulas, political influence of congressional representatives, bureaucrats, and individual aid recipients have been studied as the sole determinants of aid allocations. Each explanation, however, fails to examine the question of aid allocations from a demand-side perspective. This omission assumes that all governmental units are equally desirous of federal assistance and that any bias in the distribution of federal aid is a function of supply-side conditions. Identifying a linkage between demand-side and supply-side determinants of aid allocations, this article proposes and tests hypotheses derived from an integrated model of federal aid allocations.

The current debate over the allocation of federal aid to state and municipal governments has produced a number of attempts at explaining who gets what, where, when, and how (Havenman et al., 1976). The dominant explanatory approach has been to adopt the perspective of the donor—the federal government. Here, different distribution formulas, political influence of congressional representatives, bureaucrats, and individual recipients have been studied as determinants of aid allocations. Each explanation, however, fails to examine the question of aid allocation from a demand-side perspective. This omission assumes that all governmental units are equally desirous of federal assistance and that any bias in the distribution of federal aid is a function of supply-side conditions.

Identifying a linkage between demand-side and supply-side determinants of aid allocations, this article proposes and tests hypotheses derived from an integrated model of federal aid allocations. The following questions serve to further focus this study:

1. What has been the historical pattern in the allocation of federal aid? Has federal aid been allocated equally to all recipients or has there been a specific bias in the pattern by which federal monies are distributed?
2. How has the grant application process (i.e., demand behavior) affected the distribution of federal aid to municipal governments?
3. To what extent does the degree and nature of prior experience with the federal aid system structure grant seeking and receiving?

Supply-Side Explanations of Aid Allocations

Supply-side explanations of federal aid allocations have emphasized the donor's perspective (i.e., that of the federal government), often at the neglect of the recipient's viewpoint. Supply-side theorists, however, have not been uniform in identifying determinants of federal aid outlays. Congressional scholars (Anagnoson, 1979; Ritt, 1976; Strom, 1975; Murphy, 1974; Ferejohn, 1973, 1972) have long argued that representatives with key committee appointments, seniority, and other positions of relative power, use their influence to obtain a disproportionate amount of the federal largesse for their congressional districts. In spite of the intuitive appeal of this hypothesis, supporting evidence has been lacking. The chronology of research findings suggests a waning of traditional political influences on the allocation of federal aid monies.

A recent trend in supply-side explanations of aid outlays has been the substitution of bureaucratic for congressional authority over the distribution of federal largesse (Anagnoson, 1978; Gilbert and Specht, 1974). A majority of aid programs delegate responsibility for awarding assistance to agency administrators (Anagnoson, 1978, p. 34). Research on bureaucratic influence suggests that administrators use their authority in a timid and non-systematic fashion. Studying

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I am grateful to David Walker, Assistant Director, U.S. Advisory Commission on Intergovernment Relations (ACIR) for his intellectual support of this research. Financial support for the research has been provided under a contract with the ACIR. The opinions presented in this article are solely those of the author and do not reflect the positions or views of the ACIR or its staff. It is also a pleasure to acknowledge Patty Renfrow for her research assistance and her critical comments on an earlier draft of this article.

EDA's public works program, Anagnoson observes (1978, p. 34):

> The criteria the agencies use to select projects reinforces the restraints through their vagueness and ambiguity . . . the standards the regional directors use are so loosely specified that they do not determine the final distribution of projects.

Even in instances where administrators engage in overt and specific targeting of aid, there is little evidence to support the success of their efforts. Gilbert and Specht (1974) found that Model Cities grants allocated on the basis of the "applicants most likely to achieve Model Cities Program objectives," failed to achieve the desired outcome. The results indicated that cities awarded Model Cities monies were no more likely to achieve the desired level of performance than non-recipients.

A Demand-Side Explanation of Aid Allocations

Each of the supply-side explanations assumes that (1) all municipal governments are capable of applying for federal aid monies and (2) that those municipalities outside the federal aid system have no specific need or, for reasons of political preference, choose not to apply for federal aid. A number of researchers have suggested that not all potential recipients are equally capable of seeking and receiving federal assistance. Porter, Warner, and Porter (1973), Saltzstein (1977), Gabris and Giles (1978), Reed and Green (1978), and Stein (1979) have empirically demonstrated that there are significant obstacles to grant seeking which indirectly structure allocation patterns. Certain non-transferable costs (i.e., application, implementation, and opportunity costs) confront the potential applicant for federal assistance. These costs adversely affect grant seeking activities of smaller, fiscally and socially needier communities, effectively eliminating them from the pool of eligible aid recipients and reducing the potential for equalization in the allocation of federal monies.

Many needy and desirous municipalities find the costs of seeking and receiving grant assistance greater than the potential benefits of participation in the federal aid system. The costs associated with grantsmanship, however, do not operate independently of supply-side factors to structure grant allocations. Demand theorists have essentially described a process in which potential applicants respond to specific conditions of the grant system. Matching requirements, administrative oversight, and requisite program activities provide the basis upon which potential applicants decide whether or not to seek and/or receive federal aid.

An Integrated Explanation of Grant Allocations

Proponents of a demand-side explanation of grant allocations have provided an important insight into the operation of municipal grant-seeking activities. However, there remains a significant gap in their research. How does the application process operate to structure grant seeking over time? Specifically, what is the nature of the process operating to encourage or discourage communities from seeking grant assistance at one time, while producing the opposite effect on grant-seeking activities at some later time?

The administrative structure of federal aid programs gives the prospective applicant information with which to assess the merits of seeking and receiving federal assistance. Certain aid packages stimulate grant seeking, while other aid programs have either a neutral or negative effect on grantsmanship. Project grants require formal applications and are competitively awarded by administrative officials. In varying degrees, matching expenditures and compliant policy behavior are required of all aid recipients. Consequently, some communities find project grants difficult to apply for and politically undesirable to accept. Revenue sharing and block grant aid mechanisms have gradually replaced many project grants with a set of functionally integrated formula-based aid programs. Receipt of formula-based assistance is not subject to competitive grant applications, significant matching funds or excessive policy requirements. The result is a more desirable aid mechanism that is both substantively and administratively attractive to municipal governments.

The aid programs of President Johnson's Great Society (1963-1968) were dominated by restrictive and costly project grants (ACIR, 1978, p. 40). Great Society project grants may have discouraged many municipalities from seeking federal assistance, since the costs associated with applying for such assistance and the requirement for fulfilling grant obligations outweighed any probable benefits associated with receipt of federal aid. In contrast, the New Federalism aid packages initiated by the Nixon administration (1969-1977) were dominated by more flexible formula-based block grant and revenue-sharing aid programs. These less restrictive and less expensive New Federalism programs made it possible for many nonrecipients to enter the federal aid system for the first time. Moreover, their positive experience with general revenue sharing and block grant programs led many municipalities to experiment with project grants. Indeed, as Loucks has suggested (1978, p. 7):

> Without the New Federalism, many small communities would still be outside the intergovernmental system. With GRS and block grant funds used as bait, cities . . . have become "hooked" on federal grants and, with each passing year, become unlikely to give them up.

The positive experience of municipal governments with New Federalism programs and their newly acquired dependence on federal assistance led these new aid participants to apply for project grant assistance. The nature of this dependence is both political and economic in character. Unrestricted expenditures of block grant and revenue-sharing monies allowed many communities to substitute federal monies for municipal revenues, effectively reducing taxes (Caputo and Cole, 1977; Urban Institute, 1979). In order to maintain stable tax rates, municipal governments were forced to broaden their involvement with the federal aid system and compete for project grant monies. In addition to the political advantages of reduced taxes, many New Federalism programs stimulated demand for additional government services. Citizens provided new services under the Community Development Block Grant program had their appetites whetted for continued and expanded services, requiring new municipal revenues. Unable to raise taxes, the affected governments began to seek federal aid beyond the scope of revenue sharing and block grants (ACIR, 1980b, p. 174).

Three hypotheses can be derived from the previous explanation of federal aid allocations:

1. The degree of equalization in the allocation of federal aid is positively related to the level of municipal participation in the federal aid system.
2. Entry into the federal aid system after 1967 (i.e., after the implementation of revenue sharing and block grants) is inversely related to the participant's socioeconomic status.
3. Participation in the federal aid system is progressive, initially limited to formula-based aid programs with successive expansion into project grant programs.

The first hypothesis conflicts with a number of studies which have attempted to show that general revenue sharing and block grants have served to decrease equalization in the allocation of federal assistance. Dommel (1975) maintains that broadened eligibility under the block grant and revenue-sharing programs provided aid to municipalities which previously were outside the federal aid system. The impact of this newly enlarged pool of eligible recipients is that "the total central city formula entitlement [for the Community Development Block Grant] of $1.2 billion a year is nearly 20 percent lesss than the $1.48 billion these cities received under the categorical (i.e., project) programs" (Dommel, 1975, p. 170).

Dommel's analysis presumes that the new participants in the Community Development Block Grant program and other New Federalism aid programs need federal assistance less than the previous recipients of federal project grants. Our second hypothesis suggests the opposite. Increased participation in the federal aid system is biased toward socially and fiscally needy municipalities, effectively increasing the degree of equalization in the allocation of federal assistance.

Data and Research Design

The population of municipal governments studied is limited to all general-purpose governments over 25,000 in population as of 1967 (N=810).[1] The latter qualification is necessary to ensure a comparable sample across each year studied (i.e., 1967, 1972, and 1977). Communities under 25,000 must be excluded because many aid programs limit eligibility to communities 25,000 and larger, and the data on socioeconomic characteristics for smaller cities is not collected in a systematic or timely fashion by the Bureau of the Census.

Data on the allocation of federal aid monies are available from the census bureau's *Census of Governments*. The census bureau provides aid allocations by seven functional categories for all general-purpose governments. The census figures, issued every fifth year ending in "2" and "7," include only direct federal aid to municipal governments and exclude monies passed through the state to the municipality. Data on each independent measure of social and fiscal need were available for the three years studied. In the case of several population-based measures (i.e., for the aged and for school-age children), the data are estimates from the 1970 census (U.S. Bureau of the Census, 1979). A fuller description of data sources for federal aid outlays can be found in the appendix; Anton (1979, 1978); Anton, Cawley, and Kramer (1980); and in Hines and Reid (1977).

The appropriate indicators of social and fiscal need are widely debated by both academicians and politicians. Recent legislative battles over the measure of housing stock for inclusion in the distribution formula for the Community Development Block Grant underscore the political importance associated with this issue (Stanfield, 1979). It is not our intent to add to this already growing debate. Selection of need and fiscal capacity measures are based on their relevance to the operation of the federal aid programs studied. The indicators selected are in fact employed in a wide number of federal project and formula-based aid programs.[2] Our measures of a municipality's need and fiscal capacity are as follows:

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[1] Thirty-five cities in California below 30,000 in size were excluded from the analysis because of missing federal aid data.

Factors Measuring a Municipality's Need:

- Aged (percent population over 65 years of age)
- Dependent children (percent population below 18 years of age)
- Education (percent population with high school education, 25 years of age and over)
- Unemployment (percent adult population unemployed)
- Poverty (percent of families below poverty income level)
- Crime (crimes per 1,000 persons)
- Income (median family income)
- Population size

Factors Measuring a Municipality's Fiscal Capacity and Effort:

- Home ownership (percent of dwellings owner occupied)
- Home value (median value of owner-occupied home)
- Property tax rate (full value municipal property tax)
- Tax burden (percent of per capita income paid for taxes[3]

Findings

The first hypothesis implies that changes in the distribution of federal aid are partially a function of a significant increase in the number of municipal governments participating in the federal aid system. Table 1 presents the percentage of communities participating in the federal aid system between 1967 and 1977 by functional area. In each successive year the percentage of participating governments increases. Between 1967 and 1977, 46.7 percent of the communities studied initially entered the federal aid system. Moreover, this dramatic increase is not merely a function of the passive general revenue-sharing program. Of those communities studied, more than 90 percent participated in project grants in 1977, suggesting that at least since 1972 (the first year of general revenue sharing), a large number of municipal governments have broadened their use of aid programs to include project grants.

Participation in each functional category of federal aid increased during the period studied. Interestingly, overall participation in each of these functional areas lags behind participation in all aid programs and does not exceed 50 percent of the communities studied in any year. Limited use of federal aid in at least five areas is due to the absence of municipal functional responsibility for these functional activities. Education, public welfare, employment, and highways are functional responsibilities associated with state and county governments and special districts instead of general-purpose municipal governments. This condition would suggest that participation in the federal aid system may be related to the scope of municipal functional responsibility.

Table 3 presents the relationship between per capita federal aid outlays and indicators of need and fiscal capacity.[4] For comparative purposes,

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[2] See ACIR (1978). There is a tendency when evaluating the targeting capacity of federal aid allocations to measure equalization against more objective and sensitive measures of need and fiscal health than those employed by federal officials. The exploration for more isomorphic measures of social and fiscal need raises a different question than the one addressed in this article. To evaluate targeting in terms of criteria other than those employed by federal authorities is to engage in a biased analysis of federal targeting efforts. In order to evaluate properly the effectiveness and correlates of federal targeting activities, it is necessary to consider the indicators actually employed in distributing the federal largesse, rather than an alternative set of indicators, regardless of their validity.

[3] Tax burden is calculated by dividing the per capita tax revenue from 11 sources by per capita personal income.

[4] There is a growing debate over the appropriateness of per capita aid measures when examining the allocation of federal grant assistance. Uslaner (1976) and Ward (1980) both argue that there must be a theoretically compelling reason for the use of standardized aid measures. Ward notes that aid monies are not allocated by states or the federal government on a per capita basis, and therefore evaluations of these aid programs employing per capita or percentage measures produce distorted results. Though we agree with the logic of this argument, our own analysis fails to produce a significant empirical difference between per capita and total aid measures. Both dependent measures of aid allocations were employed in separate analyses and failed to produce different results. Though the coefficients are obviously different due to differences in measurement, the pattern of equalization observed for both dependent measures is virtually identical. This should not be taken

Table 1. Percent of Municipal Governments Participating in the Federal Aid System by Program and Year: 1967, 1972, and 1977

Program	1967	1972	1977
All Aid Programs	53.3	63.3	100.0 (94.1)*
Education	11.7	11.7	12.3
Employment	.1	.2	.2
Housing/Urban Renewal	25.0	29.9	38.7
Highways	2.5	1.9	6.9
Health	8.2	7.3	10.0
Public Welfare	.4	1.8	2.1
Other	38.8	53.3	90.1
N = 810			

Source: Compiled from U.S. Bureau of the Census, *Census of the Governments: Finances of Municipalities and Township Governments*—1967, 1972, and 1977 (Washington, D.C.: U.S. Government Printing Office).

*Excluding participation in general revenue sharing.

the unstandardized and standardized coefficients and y-intercept are presented. Interpretation, however, is limited to the unstandardized coefficient.[5] Nine of eleven need and fiscal capacity measures produced the expected change in the observed relationship with per capita federal aid outlays. Substantively, this indicates that in each successive year a unit change in need and/or fiscal capacity produced a greater change in federal outlays in the hypothesized direction. Fiscal capacity variables appear to lag behind need indicators in the magnitude of their impact on changes in aid outlays. Unit changes in median income and home value each produced less than a $1.00 change in per capita dollar outlays in any year. The coefficients for median home value in each year were insignificant, indicating that home value makes no contribution to the level of equalization in the allocation of federal assistance.

Population size has a significant but extremely weak influence on the allocation of federal aid in 1967 and 1972. As expected, larger cities received more aid, though this ratio is negligible. In 1977, however, this relationship is reversed. There is a moderate and negative relationship between city size and aid allocations. This change reflects the broadening of eligibility requirements to include cities under 50,000 in size in aid programs (e.g., discretionary funding under the Community Development Block Grant). Beyond the political

Table 2. Mean Per Capita Direct Federal Aid Allocation by Year: 1967, 1972, and 1977

	Year		
	1967	1972	1977
Allocation	$4.47	$9.61	$56.02 ($39.95)*
N =	431	513	810

Source: Compiled from U.S. Bureau of the Census, *Census of the Governments: Finances of Municipalities and Township Governments,* 1967, 1972, and 1977 (Washington, D.C.: U.S. Government Printing Office).

*Excluding general revenue sharing allocation.

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as an endorsement of the use of per capita measures. We have reported per capita measures because of their ease of interpretation, not because they are more theoretically meaningful.

[5] I have deviated from previous analysis procedures (Dye and Hurley, 1978; National Governors' Association, 1980) by relying on the unstandardized regression coefficient rather than the (standardized) correlation coefficient to measure equalization over time. Comparisons between standardized coefficients cannot be made over time, since the means and variances, the hypothesized source of variation in the observed correlations, are *invariant* (i.e., mean = 0, variance = 1). The unstandardized coefficient, which does not alter the mean or variance of either variable, allows us to compare the magnitude of change in per capita aid outlays as a function of a unit change in the same variable measured over time. Substantively, the unstandardized regression coefficient is interpreted as the dollar increase in per capita allocations associated with a unit change in need or fiscal capacity. For a further discussion of this issue, see Blalock (1971).

Table 3. Relationship between Per Capita Direct Federal Aid and Selected Indicators of Socioeconomic Need by Year

	1967			1972			1977		
Variable	**B**	**Beta**	**Intercept**	**B**	**Beta**	**Intercept**	**B**	**Beta**	**Intercept**
Aged	21.80*	.087	2.26	48.80*	.092	4.65	269.05*	.186	28.69
Dependent Children	24.65*	.114	12.00	52.93*	.115	25.75	208.11*	.167	83.04
Education	−9.90*	−.107	10.23	−26.14*	−.134	24.27	−130.01*	−.245	128.96
Unemployment	39.91	.064	2.85	105.87*	.080	5.30	769.41*	.215	24.73
Poverty	21.09*	.091	2.72	63.39*	.129	4.34	339.10*	.255	27.81
Crime	140.03*	.217	.67	362.11*	.264	.21	1,254.73*	.237	21.97
Home Ownership	−21.43*	−.255	17.56	−49.76*	−.278	40.00	−170.90*	−.352	160.40
Home Value	.00	.016	3.96	.00	−.026	11.41	.00	.079	85.59
Median Family Income	.00	−.074	8.18	−.00	−.069	20.14	−.07*	−.256	129.40
Tax Burden	738.11*	.343	−.54	514.55*	.366	−.55	815.10*	.523	.61
Population Size	.01	.015	3.86	.05*	.111	3.85	−1.09*	−.339	.00
N=810									

Source: Compiled from U.S. Bureau of the Census, *Census of the Governments: Finances of Municipalities and Township Governments 1967, 1972, 1977* (Washington, D.C.: U.S. Government Printing Office), and U.S. Bureau of the Census, *County and City Data Book: Consolidated File City Data 1944-1977 (Washington, D.C.: U.S. Government Printing Office), 1978.*

*The coefficient is at least twice the size of the standard error and significant at the .05 level.

causes of this change,[6] there is an implicit assumption that "smaller cities" (25,000-50,000) are capable of suffering from the social and economic ills of urban decay. Moreover, this change represents a departure from defining need solely in terms of size, and a sincere attempt to identify the causes of social and economic decline. The findings in Table 3 are not sufficient to demonstrate that changes in equalization are due to increased participation in the federal aid system by needier and fiscally strained municipalities. The covariation of these two variables and their independent effect on equalization must first be established before we can conclude that increased grant use increases equalization.

Table 4. Difference of Means Test between New and Old Federal Aid Participants for the Periods 1967-1972 and 1972-1977

	1967-1972		1972-1977	
Variable	**New Participants**	**Old Participants**	**New Participants**	**Old Participants**
Aged (%)	10.6	9.6*	10.2	8.9*
Dependent Children (%)	35.3	32.6*	34.0	33.2*
Education (%)	51.2	58.2*	55.8	59.2*
Unemployment (%)	4.4	3.6*	4.1	2.8*
Poverty (%)	9.1	7.3*	11.9	9.1*
Home Ownership (%)	57.5	65.4*	60.7	66.1*
Home Value ($)	17,191	19,033*	17,850	20,347*
Median Income ($)	9,648	10,692*	10,034	11,257*
Tax Burden (%)	6.5	4.2*	7.1	5.2*
Crime (per 1,000)	3.0	2.1*	6.0	5.2*
Population	46,634	160,339*	37,883	113,752*
N =	153	360	297	513

Source: Compiled from U.S. Bureau of the Census, *Census of the Governments: Finances of Municipalities and Township Governments* 1967, 1972, and 1977 (Washington, D.C.: U.S. Government Printing Office) and U.S. Bureau of the Census, *County and City Data Book: Consolidated File City Data 1944-1977* (Washington, D.C.: U.S. Government Printing Office), 1978.

*$P < .05$.

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[6] See Beer (1976) for a further discussion of this issue.

Table 5. Patterns of Participation in the Federal Aid System: 1967-1977

Pattern	1967	1972	1977	Percent	N
Constant Participants	Participant	Participant	Participant	44.4	360
New Participants in 1972	Non-Participant	Participant	Participant	18.8	153
New Participants in 1977	Non-Participant	Non-Participant	Participant	27.7	225
Deviants	Participant	Non-Participant	Participant	8.9	72

Source: Compiled from U.S. Bureau of the Census, *Census of the Governments: Finances of Municipalities and Township Governments*, 1967, 1972, and 1977 (Washington, D.C.: U.S. Government Printing Office).

Table 4 presents the mean values on indicators of social and fiscal need for new and old aid participants. The second hypothesis suggests that those entering the federal aid system for the first time (i.e., in 1972 and 1977) were significantly needier than those communities already in the system. New aid participants in both 1972 and 1977 are significantly needier on all measures. The evidence suggests that greater equalization in allocation of federal aid came about as a result of changes in the composition of recipient governments. New recipients added a significant degree of heterogeneity and need to the population of federal aid recipients, thus enhancing the degree of equalization.

Our third hypothesis predicted that new participants in the federal aid system have a progressively wider involvement in the federal largesse. Initially, utilization of aid programs is limited both in quantity (i.e., total dollars received) and scope (i.e., block grants and general revenue sharing). With successive years of participation, this level of use broadens fiscally and programmatically to include more restrictive project grants. Implicit in this general hypothesis is the notion that entry into the system is permanent. Once a community enters the federal aid system, exit is unlikely. Data from the CSA outlays report were used to calculate the amount of project and formula grant monies allocated to new aid recipients in 1972 and 1977.

Table 5 identifies the different modes of participation in the federal aid system between 1967 and 1977. As expected, most communities which entered the federal aid system retain their involvement after entry. Only 72 communities (8.9 percent) deviate from this pattern. In the case of each of these communities, the pattern was to enter the system in 1967, leave in 1972, and re-enter in 1977. No other deviant pattern was observed among the communities studied. Examination of these deviant cases did not identify a trait common to these communities. Standard deviation scores on selected socioeconomic variables indicate that there is substantial diversity among deviant aid participants. More detailed information on the nature of each community's experience with the aid system is necessary before we can arrive at any conclusive statements as to the cause and nature of deviations from the expected participation pattern.

Among those communities entering the federal aid system in 1972 and 1977, a definite pattern of participation can be identified. The mean per capita aid allocation for new participants increased from $4.65 in 1972 to $35.01 in 1977. Both figures lag significantly behind mean allocations for older participants in both years (see Table 2). As expected, the 1977 mean aid allocation for new participants entering in 1977 was lower than the mean allocation for participants who entered the system in 1972. This finding is

Table 6. Mean Per Capita Aid Allocation and Percent of Total Aid Received from Formula-Based Aid Programs for New Aid Participants in 1972 and 1977

	1972		1977	
	Mean Per Capita Allocation	Percent Formula Grants	Mean Per Capita Allocation	Percent Formula Grants
New Aid Participants 1972 (N = 153)	$4.65	65.7	$35.01	52.3
New Aid Participants 1977 (N = 297)	NA	NA	$25.84	58.1

Source: Community Services Administration, *Geographic Distribution of Federal Funds*, Fiscal Years 1972, 1977 (Washington, D.C.: U.S. Government Printing Office).

contrary to Dommel's thesis that new aid recipients under New Federalism received a disproportionately larger amount of federal assistance than did traditional aid recipients.[7] The findings in Tables 2 and 4 demonstrate that new entrants into the federal aid system were significantly needier than previous recipients and that their allocations continue to lag behind less fiscally stressed and older aid recipients.[8]

The scope of participation among new participants confirms the hypothesized pattern of grant use. New participants' initial involvement in the federal aid system is dominated by less restrictive and less costly formula and block grants. For new 1972 aid participants we observed a significant shift from formula grant (i.e., 65.7 percent) to project grant use (i.e., 52.3 percent) in 1977. These findings clearly establish the evolutionary nature of grant use. Reluctant to invest time and money in seeking competitive aid programs, new entrants to the federal aid system seek more secure and less restrictive block grant and revenue-sharing monies. Having experienced the dependence of federal aid, new entrants seek to broaden their use of the federal largesse by seeking and receiving monies from the larger pool of project grants. In fact, federal aid monies from block grants and revenue sharing are used to finance grant-seeking ventures, thus lessening the costs of competitive grantsmanship.

Discussion

Contrary to prevailing views, the expansion of the federal aid system through general revenue sharing and block grants has not lessened the degree of equalization in the allocation of federal aid monies. The evidence points to the opposite conclusion. Those communities which entered the aid system between 1967 and 1977 were the very communities most in need of federal assistance and the object of many aid programs. Moreover, the relationship between need and fiscal capacity is significantly strengthened by the added participation of these new beneficiaries of the federal largesse.

The findings of this study establish an alternative policy for alleviating inequities in the allocation of federal aid. Rather than relying exclusively on a supply-side approach (i.e., alterations in the allocation mechanisms), policy makers might consider the recipients' perspective. Assisting local governments in costly grant applications and encouraging intergovernmental grant applications would help to defray the costs of seeking and receiving federal assistance and thus increase the use of grants and their equalization among needy and fiscally strained communities. Both the block grant programs and general revenue sharing have provided some initial steps in this direction.

Finally, a note of caution. This study has not commented on the substantive policy impact of federal aid programs. Many critics of revenue sharing and block grants claim that these mechanisms fail to achieve their intended policy goals (e.g., increase municipal expenditures in targeted policy areas). Though not unrelated, this issue is beyond the scope of this study. We have attempted to examine only one aspect of the federal assistance program, the allocation process. Impact and evaluation are clearly the next logical steps to be taken in the study of the federal aid system, particularly among new aid recipients.

Appendix: A Note About Data on Federal Aid Outlays

Studies of the federal aid system are subject to significant obstacles when testing hypotheses. The fundamental problem is the "inability of the nation to keep a usable record of its own fiscal transactions" (Anton, 1978, p. 1). At present there is no single reliable and comprehensive accounting of all federal outlays to state and substate governments.

Information about allocations of federal aid to municipal governments is available from two sources—U.S. Bureau of the Census' *Census of Governments* and the Community Service Administration's (CSA) *Federal Outlays Report*. Anton (1979) identifies three significant shortcomings with the census bureau's data: (1) the functional categories lack specificity, (2) the omission of certain unincorporated areas which are significant participants in the federal aid system, and (3) the exclusion of federal aid monies passed through the state to recipient municipal governments.

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[7] This conclusion requires some qualification. Dommel's analysis was limited to block and formula grants where the grant-seeking costs were not significant for new aid participants. The higher application costs associated with project grants may have limited the mean aid allocation for first-time aid participants.

[8] Some caution should be used in interpreting these findings. The fact that data were available only for five-year periods may distort the characterization of communities as "new entrants" into the federal aid system. An aid recipient new to the 1972 data may in fact have entered the federal aid system at any time between 1968 and 1972. This limitation in the data does not detract from the central hypothesis. The expectation is that cities enter the federal aid system with a heavy reliance on formula aid and block grants, progressively shifting their dependence to project grants. The findings in Table 6 confirm this hypothesis.

CSA outlay data report all federal expenditures to municipal governments over 25,000 in population. *The outlay report includes expenditures well* beyond the scope of federal aid programs (e.g., loans, subsidies, procurements, etc.). Aid allocations are reported by specific programs and administering agencies. Though the CSA data have been collected yearly since 1968, current data holdings are reliable only since 1972 (Anton, 1979).

Some of the problems associated with the census data can be corrected. Aid allocations for unincorporated areas are available from special census reports which can be added to the *Census of Governments* data file. The omission of pass-through monies, though problematic when studying all substate governmental units (i.e., counties, school districts, special districts, etc.), is of minimal importance when studying aid allocations to general purpose municipal governments. The ACIR's analyses (1979, 1980) reveal that most pass-through monies go to special districts (i.e., public school districts) and counties (i.e., general welfare and highways) and not municipal governments.

For studies of aid allocations after 1972, the CSA data are clearly superior to the census bureau's accounting of federal aid transfers. This relative advantage, however, is eliminated for the purposes of this study. The lack of reliable data on aid allocations before 1972 limits the utility of the CSA data in testing the time-series hypotheses posed by this study. Moreover, certain shortcomings identified in the census data set can be corrected, making the census bureau's accounting of federal grant outlays the preferred data base for this study. The work of Thomas Anton's Intergovernmental Fiscal Analysis Project should produce more extensive and reliable data with which to pursue research on federal aid allocations and their impacts on recipients.

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The Election of Blacks to City Councils: Clarifying the Impact of Electoral Arrangements on the Seats/Population Relationship

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The notion that at-large elections for city council seats are discriminatory toward blacks has recently been attacked as empirically invalid. Recent studies have reached conflicting conclusions as to whether electoral arrangements or socioeconomic factors are the major influence on how proportionately blacks are represented. This article addresses this issue, using a regression-based analysis in which proportionality is treated as a relationship across cities with electoral structure as a specifying variable. Socioeconomic variables found to be important in other studies are included. The results support the traditional notion and suggest that the electoral structure begins to have a discernible impact on the level of black representation once the black population reaches 10 percent of the total municipal population. While one socioeconomic variable, the relative income of the city's black population, is found to affect the election of blacks, its impact is greater than that of the electoral structure only when the black population is less than 15 percent.

The notion that at-large elections for city council seats are discriminatory toward black Americans was once conventional wisdom in political science. Given that racially polarized voting patterns are not uncommon in American cities and that blacks are a minority which is usually concentrated within heavily segregated areas of a city, it has often been asserted that black candidates are far more likely to be elected to city councils through single-member district (or ward) electoral systems than through at-large arrangements. Recently, however, this traditional notion has come under serious question. Indeed, the literature now contains a complete range of possible conclusions about the discriminatory impact of at-large elections.

Susan MacManus has argued, for example, that the traditional view is "inaccurate" and "must be abandoned" (1978, pp. 158, 160). In her analysis of data for 243 central cities of Standard Metropolitan Statistical Areas (SMSAs), she finds that electoral arrangements have little or no direct impact on the election of blacks. When she imposed controls for various socioeconomic characteristics of cities, she discovered that the relationship between electoral format and black representation "virtually disappears" (1978, p. 159). Understandably, MacManus concluded that there is a need for "a reevaluation of the relative impact of electoral procedures" (1978, p. 160). Additional support for this revisionist argument is provided in an earlier, more limited study by Leonard Cole, whose examination of 16 New Jersey cities led him to conclude that the traditional notion had been "exaggerated" (1974, p. 26). Like McManus, Cole argues that the socioeconomic characteristics of a city's population are more important determinants of black electoral success. Albert Karnig (1979) has also raised some questions about the relative significance of electoral arrangements. Although Karnig is among those who have marshaled evidence in support of the traditional notion (Karnig, 1976; Karnig and Welch, 1978; see also Kramer, 1971; Jones, 1976), he has more recently argued that although the electoral system is not an unimportant variable in understanding the election of black council members, it is considerably less important than other variables which reflect black political resources, especially the socioeconomic status of the black community (1979, pp. 143-45, 148).

Several other recent studies, however, reach sharply contrasting conclusions. Theodore Robinson and Thomas Dye included various socioeconomic indicators for both the total population and black population in their investigation of councilmanic representation in central cities, yet concluded that the electoral framework is the "single most influential" variable. According to them, "At-large elections, independently of any other socioeconomic or structural variable, significantly reduce black representation on city councils" (1978, p. 139). In a study focusing on only southern cities, Margaret Latimer investigated socioeconomic variables for the black, white, and total populations of cities but still found that it

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We wish to express our appreciation to Theodore P. Robinson and Thomas R. Dye for providing data used in this analysis and to John K. Wildgen for assisting with the preparation of the data.

was the electoral system that had "a preeminent causal effect" on the election of blacks (1979, p. 79). And Delbert Taebel, although not incorporating socioeconomic information into his analysis, has reported that the electoral arrangement is an important factor when blacks constitute between 20 and 40 percent of a city's population (1978, p. 149).

The conflict in these conclusions is of considerable interest for both theoretical and practical reasons. On the theoretical side, the revisionist position suggests that the electoral framework is really little more than a relatively unimportant structural dimension which happens to intervene between more explanatory socioeconomic factors and black representation. The electoral framework had appeared to be one structural variable that would surely survive the onslaught of "environmental determinism," but this now seems to be a matter for debate. On the practical side, the issue is enormously important to those wishing to bring about a more equitable proportion of blacks on city councils, for the electoral structure of a city is certainly more readily changed than the socioeconomic characteristics of a city's population. Changes in electoral structures may be mandated by courts, for example, and blacks have often resorted to litigation in their efforts to substitute single-member districts for at-large elections. Indeed, this is probably one of the litigation issues for which political scientists are most frequently called upon to testify as "expert witnesses" (see, e.g., Davidson, 1979), and many of the studies in this area have been cited in judicial opinions (see, for example, the dissenting opinion of Justice Thurgood Marshall in *City of Mobile* v. *Bolden*, 48 LW 4436, at 4449-4450, n. 3 [1980]).

Given both the theoretical and practical import of this issue, it seems especially important to clarify the impact of electoral arrangements on the election of blacks to city councils. In this article we employ an analytic design which we believe to be more appropriate, and even more powerful, than those used previously. This alternative approach, which avoids some significant methodological problems and conflicts present in the other studies, allows us to demonstrate more clearly the kind and degree of impact that different electoral arrangements and socioeconomic factors have on the election of blacks.

An Alternative Strategy

The disparity in conclusions across the various studies may be partly a function of different methodological choices. Two dimensions in particular—the measurement of the dependent variable and the criteria adopted for selecting the cities to be included in the analysis—may contribute to the inconsistent results. The dependent variable has in all cases been a measure of what Hanna Pitkin calls "descriptive representation," the correspondence between the percentage of a city's population which is black and the percentage of seats on a city's council held by black people (1967, pp. 60-91). Two different operationalizations have been employed, however. The measure most frequently used has been a ratio, i.e., the council percentage divided by the population percentage. None of the studies using this measure has concluded that the electoral system is an unimportant variable. The revisionist findings of both MacManus and Cole, however, are based on a different operation. They (as well as Taebel, who did not control for socioeconomic factors) calculate the *difference* between these percentages, i.e., population percentage subtracted from council percentage. Objections may be raised to both measures. The ratio has the undesirable consequence of scoring every city without a black person on its council as zero, regardless of the proportion of its population that is black. The subtractive measure avoids this particular problem, but will also produce quantitative equivalencies in cases that many analysts may feel to be qualitatively quite different. In particular, one city's score on this index may register the maximum degree of underrepresentation possible for that city (e.g., council percentage = 0; population percentage = 10), yet be identical to the score for another city in which the underrepresentation may be quite far removed from its theoretical maximum (e.g., council percentage = 35; population percentage = 45).

This latter consideration introduces another methodological issue: the criteria for determining which cities will be included in the analysis. All but MacManus and Latimer have established some black population threshold as a requirement for inclusion. The rationale, of course, is that there must be at least a minimum level of black population before the black community can have a realistic chance of electing one of its own to the council under any electoral arrangement. There has been no consensus on what that threshold should be, however. Analysts appear to have made little more than intuitive judgments on this matter, judgments that may reflect concern for sample size as well as minority population. Most have adopted a fixed percentage, requiring the black population to be at least 5, 10, or 15 percent of the total population. The only exception to the fixed percentage has been Taebel's requirement that the black population be equal to at least one-half of the population of a single-member district, assuming the entire council is elected from districts (1978, p. 144).[1]

These methodological considerations undoubtedly influence what one discovers about the effects of different electoral systems. Unfortunately, there are no clearly objective nor widely agreed-upon guidelines for making these determinations, and the interpretation of findings must consequently be conditional to the research strategies adopted. Fortunately, however, a slight shift in approach provides another, more appropriate way to address this topic, one which also circumvents these difficult methodological choices.

There is a distinct similarity between these studies of descriptive racial representation and studies which examine the translation of votes for a political party's candidates into seats in a legislative body for that party. Both are primarily concerned with proportionality, in one case between population and seats, in the other, votes and seats. In the partisan studies, however, proportionality has been treated as a *relationship* between two variables across elections, rather than as a variable itself (e.g., Tufte, 1973). If this same approach is applied to the data on racial correspondence—treating proportionality as a relationship across cities rather than as a dependent variable—then the measurement and threshold issues discussed above virtually disappear. The proportion of council members who are black may simply be regressed onto the black proportion of the population. The resulting equation will provide an estimated slope coefficient, or "swing ratio" (Tufte, 1973, p. 542), for this relationship. As long as the intercept for the regression equation is fairly close to zero, this coefficient may be interpreted as a measure of proportionality (see, e.g., Rae, 1971, pp. 89-90). If the slope coefficient has a value of 1.0, the relationship is directly proportional. A slope greater than 1.0 would mean the proportion of blacks on city councils generally exceeds the proportion of blacks in the population, while a slope of less than 1.0 would mean the council proportion is generally lower than the population proportion.

There are definite advantages to addressing the substantive issue in this fashion. Not only does this obviate the measurement debate over division or subtraction, it avoids the major problems associated with each of these options as well. Under this approach, proportionality is a relationship across a set of data points, each of which reflects the specific black proportions of the population and the council for a city. The fact that all cities without a black council member do not have the same black population percentage is taken into account in estimating this relationship. All cities without a black council member are not, therefore, treated as empirically equivalent in this procedure, as is the case with the ratio measure. Likewise, all cities with the same deviation from proportional representation are not treated as empirically equivalent, as is the case with the subtractive measure. This procedure is sensitive to the fact that cities with identical or similar deviations may have widely varying percentages of blacks in their populations. Treating proportionality as a relationship across cities also obviates the need for a threshold. The use of a threshold is based on an assumption that only when the black population of a city exceeds a certain level can blacks expect to hold a seat on the council. In a regression-based analysis, this a priori assumption may be treated as an empirical question. If a threshold is indeed discovered, it may be taken into account by estimating the appropriate nonlinear swing ratio. In addition, if the electoral format is an important variable, this approach will allow one to estimate empirically the point at which structure actually begins to make a difference in how proportionately blacks are represented.

A regression-based analysis also permits one to examine the impact of electoral arrangements in the conceptually most appropriate way, as a *specifying variable* which establishes conditions under which the seats/population relationship varies, rather than as an independent variable with a direct impact. The traditional theoretical argument asserts that the major factor affecting the election of black council members is the relative size of the black electorate (although, due to the absence of voter registration figures by race in all but a few cities, the actual meaure has been the black proportion of the population). The influence of this variable, however, is viewed as conditional. Its influence is expected to vary across different electoral structures, having a much greater impact within the district format than in at-large arrangements. This may be tested by inserting electoral structure into the regression equation as a series of dummy variables that interact with the black population percentages. If the traditional notion is correct, the slopes for the seats/population relationship should be quite different across the election types, and should remain so when socioeconomic factors are also included in the equation.

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[1] The threshold issue is related to the measurement issue in that the lower one sets the threshold requirement, the more important the difference in measurements becomes. It has been suggested that whether the ratio or subtractive measure is adopted is empirically unimportant, because essentially the same relationships appear regardless of which measure is used (Karnig and Welch, 1979, p. 466; and 1978). This is likely to be true, however, only if the threshold is set sufficiently high. Based on the Robinson-Dye data, the correlation between the two measures is only .45 across all cities (no threshold). Among cities that are at least 5 percent black in population, the correlation increases to .72. When the threshold is set at 10 percent or 15 percent, it increases to .83 and .92 respectively. Clearly, if one is willing to adopt one of the higher thresholds, the measurement issue is largely moot. If theoretical considerations or concerns for sample size dictate that either no threshold or a low threshold be adopted, however, the measurement issue is quite serious.

The results of a regression-based analysis of this type are reported below. The data are taken from the *Population-Policy Data Base: U.S. Cities and Suburbs, Standard Metropolitan Statistical Areas* (PPDB), compiled under the direction of Thomas R. Dye. The information on electoral structure and number of black council members was collected during October 1976, by means of a telephone survey of city attorneys and clerks in the largest city of each SMSA (N = 243). All population figures and socioeconomic indicators were derived from the 1970 Census of Population. Because we are interested in the impact of different electoral structures on the black *minority*, the following analysis is based on the 239 central cities in which blacks constitute less than half of the total population.

Impact of Electoral Format and Socioeconomic Factors

Before comparing the impact of electoral structures and socioeconomic factors, we need to estimate the simple bivariate relationship between the black percentage of the population and the black percentage of the council. The data for the 239 cities result in the following regression equation:

$$BCC\% = -.498 + \underset{(.041)}{.593}BP\% \text{ with } \bar{R}^2 = .471$$

where *BCC%* is the black percentage of the council and *BP%* is the black percentage of the population. The intercept, —.498, is quite close to zero allowing us to treat the slope as a measure of proportionality. Using this equation, one can generally expect blacks to be "underrepresented" on city councils, with the amount of that underrepresentation becoming more acute as the black proportion of the population increases. The percentage of seats on a city council held by blacks tends to be only about .60 of the black percentage of the population.[2]

Electoral Format. The existing literature on the election of blacks to city councils has been concerned, first and foremost, with the possible impact of different electoral arrangements. These have traditionally been classified into three groups: systems that elect all of the council members at-large, systems electing all members from districts, or mixed systems which use some combination of at-large and district elections. It has been argued that this classification scheme is not sufficiently discriminating, that at-large elections with place systems and/or district residency requirements should be distinguished from the "pure" at-large situation (MacManus, 1978). Although these differences are present across at-large elections, their conceptual linkage to the issue of vote dilution may be quite tenuous. Ultimate control over the outcome in these modified arrangements still remains with a *citywide* electorate (see Davidson, 1979, p. 337).[3] In addition, this more extensive classification may in some cases produce an empirical problem—the number of observations for each of the modified forms may be too limited for reliable analysis. For example, in the McManus study, based on 243 cities, the Ns for the different categories of the modified types were only 6, 7, and 16. Given these problems with the more extensive classification, we have adopted the traditional trichotomy for this analysis. This results in 128 at-large systems, 36 districted systems, and 75 mixed arrangements.

When the electoral format is introduced into the regression equation as dummy variables and interaction terms, the different types of elections have a rather dramatic impact on proportionality (Figure 1). The slope coefficient for districted cities is virtually equal to 1.0, whereas the slope for at-large systems is only about .5. The impact of mixed systems, not surprisingly, falls between that of the other two. Clearly, a black minority is more likely to attain proportional representation through an electoral system that does not have any at-large elections.[4]

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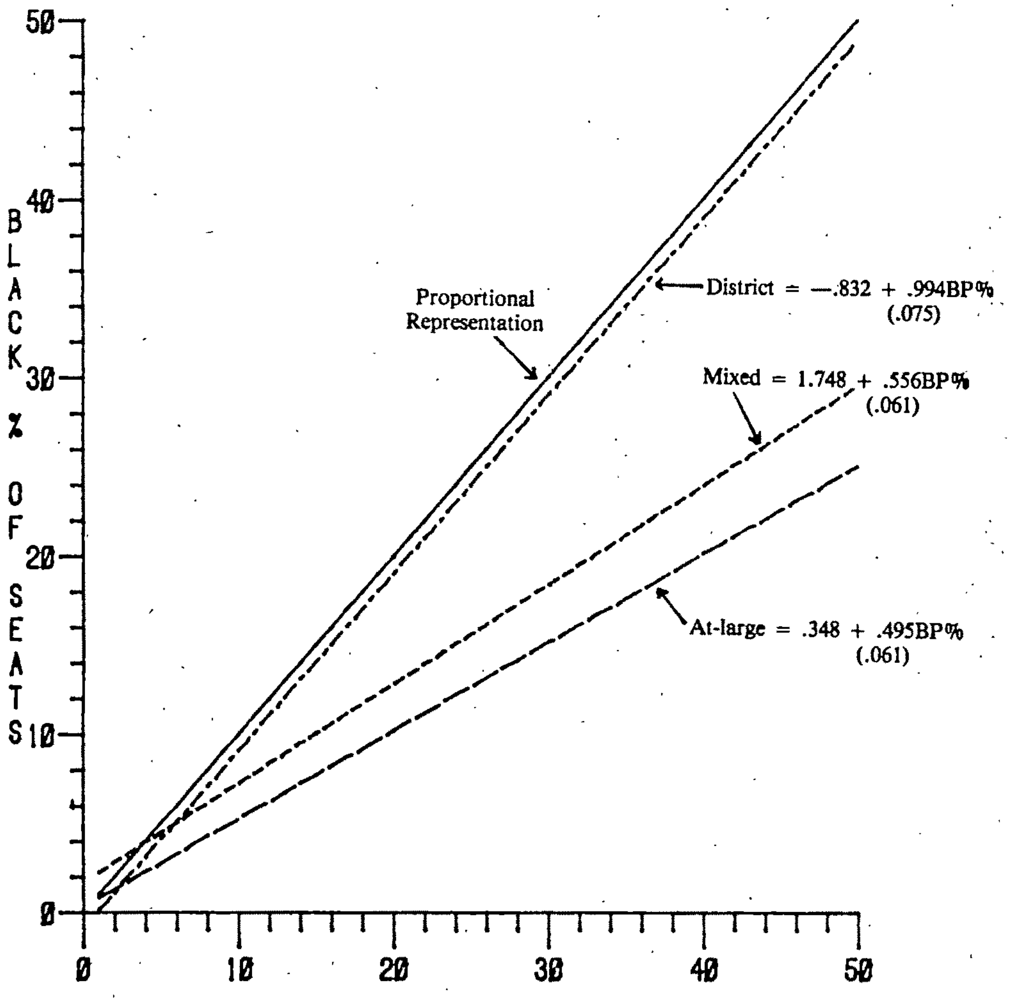
[2] It was suggested above that the notion of a threshold could be empirically tested through nonlinear regression. Logit transformation indicates that a linear fit is quite appropriate in this case, however, as the slope for the $\log_e$ of the population odds is .829.

[3] Residency requirements do result in council members whose residencies are geographically dispersed. Given control over the ultimate selection by the citywide electorate, however, this requirement does not assure that blacks will be elected to the council except in the extremely unlikely situation that all possible candidates in a "residency district" are black. If the city electorate is polarized along racial lines, the white majority may easily select a white candidate from a majority black residential district. Evidence from several studies suggest, in fact, that such requirements have not resulted in more proportionate black representation (see Karnig and Welch, 1978, p. 15; Robinson and Dye, 1978, p. 137; and MacManus, 1978, p. 157).

Figure 1 not only shows that there is a tendency for blacks to be less proportionately represented in at-large systems than in districted systems, it also shows that the electoral structure begins to make a difference even when blacks constitute a fairly small percentage of the population. In order to estimate the point at which the electoral format can be expected to have an impact, we have added 90-percent confidence bands around the regression lines for both the districted and at-large cities. These bands are displayed in Figure 2, which shows that there is no overlap between them beginning at a point just below 10 percent. Thus, as a conservative estimate, one could con-



Source: Compiled from Population-Policy Data Base, Florida State University, and 1970 Census of Population.
Note: The overall adjusted R^2 is .540 and the within group adjusted R^2s, which legitimately can be compared given the similar standard deviations for the black population percentages (for districted, 13.7; mixed, 12.3; at-large, 13.2), are .816 for districted cities, .510 for mixed cities, and .344 for at-large cities.

Figure 1. Relationship between Black Percentage of Population and Black Percentage of City Council in Different Electoral Formats

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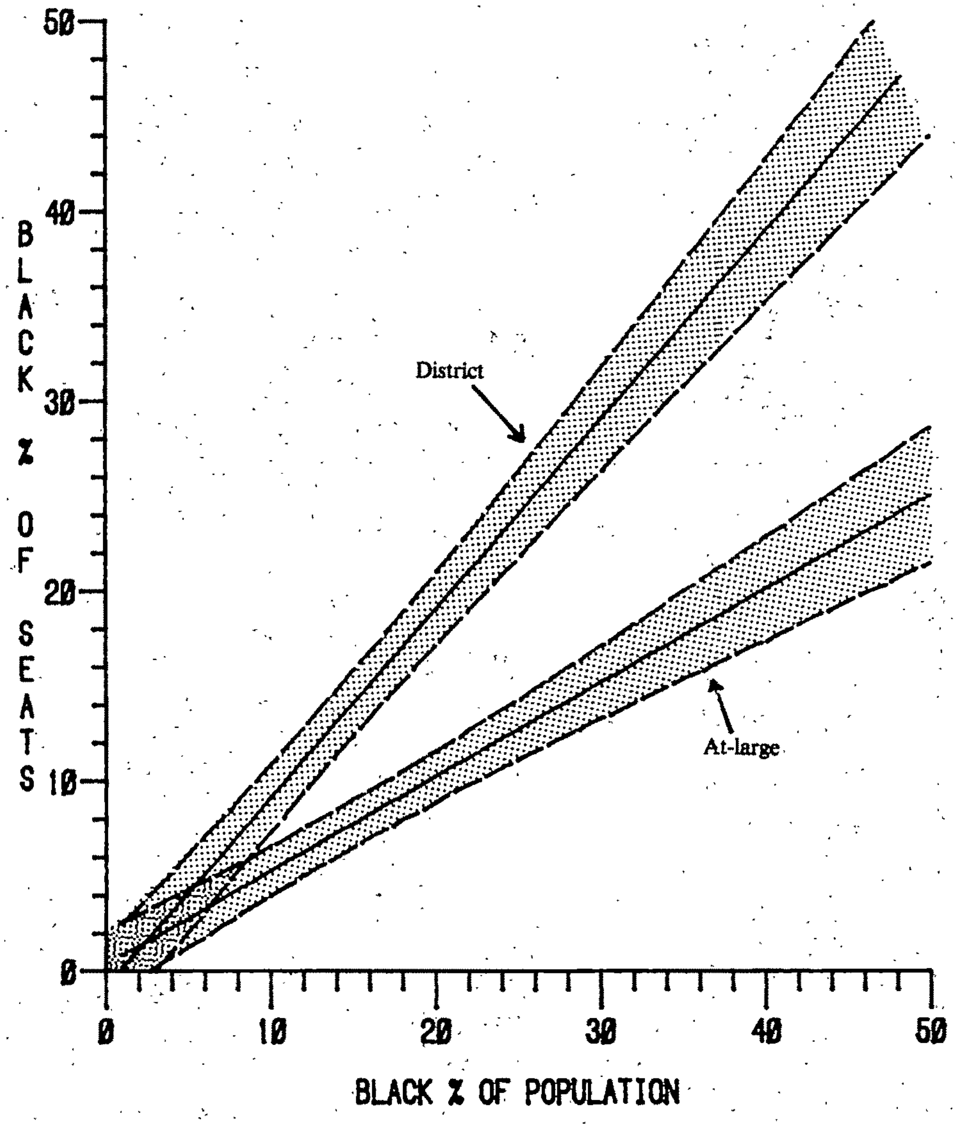
[4] Tests for nonlinear fits show that the reported linear fits within all three categories are quite appropriate.

sider 10 percent to be a threshold or critical point at which at-large elections can be expected to have an adverse impact on black representation.[5]

Socioeconomic Characteristics. While the above analysis certainly supports the traditional notion concerning the effect of different electoral structures, the question remains whether that inference will withstand controls for various socioeconomic factors. MacManus (1978) found that there was no relationship between electoral format and black representation when controls were imposed for the size, growth rate, and income, educa-



Source: Compiled from Population-Policy Data Base, Florida State University, and 1970 Census of Population.

Figure 2. Estimated Seats/Population Relationships with 90-Percent Confidence Bands in Districted and At-Large Electoral Formats

tional, and occupational characteristics of a city's population. Cole (1974) also focused on income, educational, and occupational characteristics in arguing that electoral arrangements have only a minimal impact.

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[5] Although Taebel focuses on the 20 percent figure as a "tip-point," electoral arrangements clearly begin to have an impact in his analysis when the black population exceeds 10 percent (1978, p. 150).

In order to test this revisionist argument, we have added to the regression equation the five variables for which MacManus controlled: population size (in ten thousands), rate of population change from 1960 to 1970, median family income, median school years completed by those over 25 years of age, and the percentage of the labor force employed in white-collar occupations. The results, reported in Table 1, do not support the alternative argument. The slopes for the seats/population relationship across the different electoral formats are affected only minimally, while the independent effect of these additional variables on the percentage of blacks on a city council is negligible when the black population percentage is also in the equation. It might be argued, however, that it is more reasonable to expect these environmental factors to have a conditional rather than uniform impact. In districted cities, the black population percentage may virtually be all that matters in explaining the level of black representation, while in cities with at-large elections the population characteristics may have a discernible effect. This does not appear to be the case either, however. Table 1 also contains the results of the regression analysis in which these environmental variables are entered as interaction terms. The slopes for the three seats/population relationships are again virtually unaffected, and the regression coefficients for the socioeconomic factors continue to suggest only minimal impact, if any.

While our findings clearly contradict those of MacManus (and Cole), it may be possible to reconcile the conflict. MacManus suggests that proportional racial representation is more likely to be found in "smaller cities, characterized by growth, greater wealth, and more highly educated populations" (1978, p. 160). These are also the types of cities that are more likely to have relatively small percentages of blacks in their populations. For example, the 25 cities in this analysis which are under 100,000 and above the mean on growth, income, and education have an average black population percentage of only 4.3, compared to 15.8 for the remaining cities. When the black percen-

Table 1. Seats/Population Relationships under Different Electoral Formats with Uniform and Conditional Impacts of Socioeconomic Characteristics

	Uniform Impact[a]	Conditional Impact[b]		
		Districted	Mixed	At-Large
Intercept	–	–.920	–10.208	11.630
Percent Black Population (Districts)	1.010	1.033	–	–
	(.096)[c]	(.086)		
Percent Black Population (Mixed)	.556	–	.598	–
	(.078)		(.080)	
Percent Black Population (At-large)	.512	–	–	.474
	(.054)			(.062)
Total Population (10,000)	.000	–.008	–.003	.110
	(.131)	(.034)	(.024)	(.037)
Population Growth Rate	.023	.012	.019	.031
	(.019)	(.040)	(.025)	(.033)
Median Family Income	.000	–.001	.000	.000
	(.001)	(.001)	(.001)	(.001)
Median School Years	–.471	4.877	1.391	–1.472
	(.930)	(2.676)	(1.588)	(1.269)
Percentage White-Collar	.010	–.130	–.114	–.038
	(.099)	(.348)	(.162)	(.151)

Source: Compiled from Population-Policy Data Base, Florida State University, and 1970 Census of Population.

[a] The slopes for the population percentage variable in the mixed and at-large cities are the actual estimates of the seats/population relation for these groups. The intercepts for districted, mixed and at-large cities are .956, 3.667, and 2.307, respectively, and the adjusted R^2 is .534.

[b] The overall adjusted R^2 is .545 and the within group adjusted R^2 s are .842 for districted cities, .506 for mixed cities, and .405 for at-large cities.

[c] The standard errors of the reported slopes are provided in parentheses.

tage of a city's population is small, the subtractive measure can register no more than a minimal level of underrepresentation, even if the council is all white. It may therefore be possible to restate MacManus' conclusion to hold that blacks are "better" represented in these cities not because they are small, growing cities with relatively well-educated and wealthy populations, but because there simply are not very many blacks in these cities and, given the subtractive measure, they are therefore fairly well represented by definition. MacManus' overall findings regarding the impact of environmental factors may also simply be reflecting the fact that there is a strong negative association between the black population percentage and the subtractive measure ($r = —.51$, using the data for the 239 cities in this analysis). Indeed, when we examine the five bivariate relationships between the environmental indicators and MacManus' subtractive measure of proportionality, controlling each time for the black population percentage, the partial correlations range from .0 for occupation to .10 for income. It is highly questionable whether these variables really have an independent impact of their own, separate from their association with the black population percentage.

It has been argued, however, that the relevant socioeconomic indicators are not those for the entire population of a city, but those for the black community within a city. Noting that an expanded black middle class means more blacks with "skills, time, and money for effective electoral politics," Karnig argues, "As blacks possess private-sector resources more competitive with those of whites, they are more likely to obtain reasonably proportional public-sector representation" (1979, p. 145). In Karnig's analysis, the variable which most influenced how proportionately blacks were elected was the black/white income differential; the closer black median income approached white median income, the more closely the percentage of seats held by blacks approached the black population percentage.

Consistent with the strategy employed thus far, we have added an income differential variable (the ratio of black median income to the median income for the entire city) to the regression equation used in Figure 1. (Because the census does not report the black median income for 15 of the cities, all of which have black population percentages less than 1.0, the following analysis is based on 224 rather than 239 cities.) Once again, the electoral format is of major importance. The slopes for the seats/population relation within each category of electoral structure remain virtually unchanged (districted = .999 [s_b = .103]; mixed = .555 [.075]; at-large = .501 [.055]). The regression coefficient for the income ratio is a not unimpressive 8.218, but its standard error (4.820) suggests that the estimate is not very reliable.

It may be more reasonable to expect a conditional rather than uniform impact for the income differential variable as well. Effective citywide campaigns are generally assumed to be more expensive and to require more extensive organizational support than district-based campaigns, and the relative "skills, time, and money" reflected by this variable may have the greatest impact, therefore, in at-large settings. To estimate the conditional effects, we have added this variable to the regression equation as a set of interaction terms. The results, which are quite intriguing, are reported in Table 2. The impact of the electoral structure on how proportionately blacks are represented remains, but a conditional impact for the income ratio is also apparent. The seats/population relationships within each of the electoral format categories are again virtually unchanged, although the additional variable does have the effect of separating out the intercepts for the three groups. The regession coefficients for the income ratio, however, do show a conditional impact on the black council percentage. The coefficient for the at-large setting is a very impressive 17.583, while that for the districted cities is —20.153, although this last value is not very reliably estimated (s_b = 12.142).

The information contained in Table 2 suggests that both electoral format and the income ratio have an effect on the level of black representation. This is consistent with Karnig's findings, although he further argues, based on a comparison of standardized regression coefficients, that the relative income of the black population has a *greater* effect than election structure on how proportionately blacks are represented. There is a problem, however, in trying to discuss standard deviation unit changes (the basic metric for the standardized coefficients) in a variable such as election format. It is difficult to imagine changing the electoral format by one standard deviation; these changes are, rather, from one structural type to another. An alternative way of making this comparison, which avoids this problem, is to estimate the expected consequence of changing the value of one variable while holding the other constant. When this is done, we discover that the relative impact of the two variables can be expected to differ dramatically, depending on the percentage of blacks in the city's population.

Figure 3 illustrates the different impacts of the two variables. The solid line serves as a baseline, and represents the relation between population and seats for cities with (1) at-large elections and (2) an average income ratio for at-large cities (.673). From this baseline the expected consequence of changing to district elections while

Table 2. Seats/Population Relationships under Different Electoral Formats and Conditional Impact of Income Ratio

	Districted	Mixed	At-Large
Intercept	12.690	−2.473	−11.733
Black Population Percentage	1.009 (.081)	.554 (.063)	.503 (.061)
Income Differential	−20.153 (12.142)	6.180 (7.257)	17.583 (5.802)

Source: Compiled from Population-Policy Data Base, Florida State University, and 1970 Census of Population.

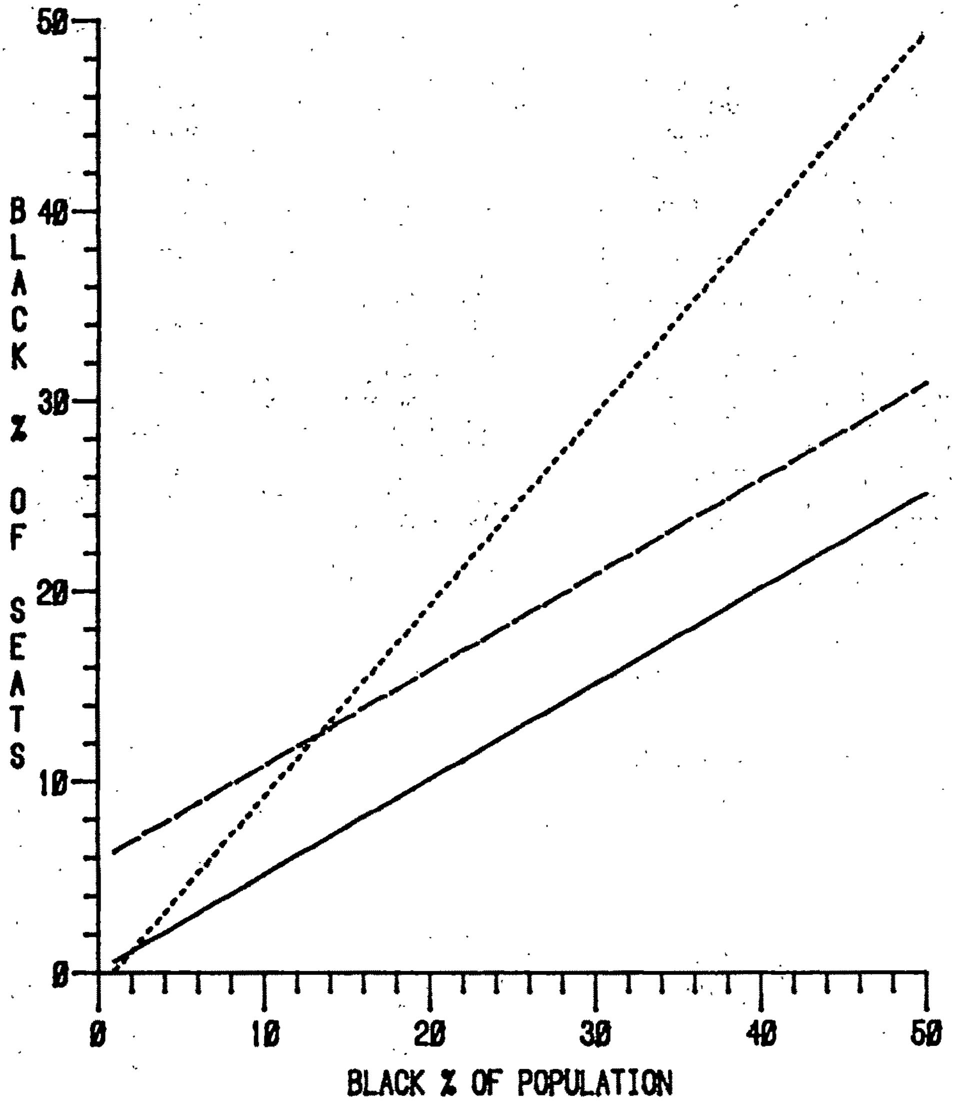
Note: The slope and intercept estimates are for the impacts of their respective variables within the particular electoral format groupings. The standard errors of the reported slope coefficients are in parentheses immediately below each term.

maintaining an average income ratio (the dotted line) can be compared to the expected consequence of increasing the income ratio to 1.0, an increase of almost three standard deviations above the mean, while retaining the at-large arrangement (the broken line). It is apparent from Figure 3 that both the structural and income changes increase the expected percentage of black council members across the entire range of black population percentages, but the relative implications of these changes are dependent upon that population percentage. At lower population percentages, achieving income equality is expected to add more than will a change in the electoral format, but at higher percentages changing the electoral format will have a much greater impact. For example, in cities where blacks comprise only 10 percent of the population, increasing the income ratio from .673 to 1.0 would be expected to increase the black percentage of the council by 5.9, compared to an expected increase of only 1.6 if the electoral structure were the variable that changed. If the black population percentage were 50, however, increasing to income equality would again be expected to add 5.9 to the council percentage, whereas a change to election by districts would be expected to add 24.3. From the data reflected in Figure 3, one could conservatively estimate that once the black population percentage reaches about 15, the electoral format has more impact than the relative income of the two racial groups.

Conclusion

The evidence reported above strongly supports the proposition that at-large elections, at least in comparison with district-based electoral systems, tend to "under-represent" black people. Districted systems may themselves be gerrymandered to reduce the level of black representation, of course (see, e.g., Engstrom and Wildgen, 1977), but as a general matter the black community can expect more proportionate representation through these systems than if at-large elections are used. From this analysis, we can conservatively estimate that the electoral format begins to make a difference when the black population constitutes around 10 percent of a city's population, and that the underrepresentation occurring in at-large cities becomes more acute as the black population proportion increases. This relationship between the electoral structure and black representation was found to be unaffected when various socioeconomic factors that have previously been suggested as important determinants of black representation were also investigated, although one socioeconomic variable—the relative income of the black population—did have an important impact on the percentage of black council members. This variable, in fact, appears to be a more influential determinant of black representation than is electoral format when the black population of a city is relatively small, but when blacks begin to constitute a fairly substantial minority of a city, then the electoral structure begins to be the more influential determinant. From the above analysis, we can estimate, again conservatively, that once the black population reaches about 15 percent of the total population, the electoral structure will have the greater influence.

The notion that differential electoral frameworks seriously affect how proportionately blacks are likely to be represented on city councils should not, as some have suggested, be abandoned. The evidence reported above strongly supports the proposition that, in comparison with single-member districts, at-large elections seriously reduce the level of black representation. This is not a novel conclusion, but it remains an important one, because at-large elections are a prevalent, yet alterable, structural dimension in American municipalities.



Source: Compiled from Population-Policy Data Base, Florida State University, and 1970 Census of Population.

Key: ——— Cities with at-large elections and average income ratio.
——— Cities with district elections and average income ratio.
——— Cities with at-large elections and income equality.

Figure 3. Comparing the Impacts of Changes in Election Format and Relative Black Income on the Election of Blacks to City Councils

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Personal Attribute Models of the Voting Behavior of U.S. Supreme Court Justices: Liberalism in Civil Liberties and Economics Decisions, 1946-1978

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The prevailing view among students of judicial politics is that judges' background characteristics or personal attributes cannot provide satisfactory explanations for variation in their decision-making behavior. Parsimonious attribute models reported here account for 70 to 90 percent of the variance in the voting of postwar Supreme Court justices in split decisions concerning civil rights and liberties, and economics. Seven variables representing six meaningful and easily interpretable concepts achieve this success. The concepts are Judge's Party Identification, Appointing President, Prestige of Prelaw Education *(economics only),* Appointed from Elective Office, Appointment Region *(civil liberties only),* Extensiveness of Judicial Experience, *and* Type of Prosecutorial Experience. *The impressive performance of these models is attributed to superior measurement, operationalization, and model building; to a greater similarity between personal attribute models and more fully specified ones than has been assumed; and to the possibility that the attitudes which intervene between the personal attributes and the voting of judges are causally very closely linked to voting.*

There is a substantial consensus among most prominent students of judicial politics that the background characteristics or personal attributes of judges cannot provide satisfactory explanations of their decision-making behavior. Goldman and Sarat state this consensus well (1978, p. 374):

> In general, the background-behavior studies of aggregates of judges have not satisfactorily established clear-cut linkages between most background variables and decision-making. The reason for this is that taken literally, the sociological model of decision-making does not make sense. . . . What is essentially being argued is that the fact that one has certain attributes means that one will have had certain socializing experiences that have stimulated the development of certain attitudes and values or even conceptions of the judicial role. But in reality each of the background or attribute variables is too crude to be associated with the same or similar experiences; hence each of the variables is not easily linked to just one set of attitudes and values. There are some weak-to-moderate associations between a few attributes and certain types of behaviors; however, the background-behavior model has not been conclusively proven, and for the reasons just suggested it is unlikely that it can be.

Very similar arguments have been put forward by Schubert (1964, p. 446), Grossman (1966, p. 1562), Becker (1970, p. 79), and Murphy and Tanenhaus (1972, pp. 107-09).

These scholars agree that personal attributes have limited explanatory potential because they are only indirect causes of judicial decision making. A variety of important attitudinal variables intervene (see Figure 1, below). It is these intervening variables that should account for most of the variation in decision behavior.

Despite the persuasiveness of the above arguments, it appears that scholars have assumed what Ulmer (1973b, p. 627) called the "risk of premature closure" in rejecting attempts to explain the voting behavior of Supreme Court justices through the use of judges' background characteristics or personal attributes. This article reports parsimonious attribute models which explain 70 to 90 percent of the variance in the justices' liberalism in the two case areas, civil rights and liberties and economics, which have received paramount attention from the modern Supreme Court. It demonstrates that satisfactory "attribute" models of judges' decision-making behavior can be developed, at least for the justices of the United States Supreme Court.

The data analyzed in this article accompany Ryan and Tate (1980). The data for the 1946-69 terms were originally collected by Glendon Schubert; those for the 1969-72 terms by Richard E. Johnston; those for the 1973-77 terms by Andrew Van Esso under the supervision of John Ryan. The Schubert data were supplied by the Inter-University Consortium for Political and Social Research. Financial support has been provided by the National Science Foundation through APSA and by the American Judicature Society. I am grateful to all these individuals and agencies for their assistance. However, none of them bears any responsibility for the analyses reported here.

The Analysis

Justices. Limitations on available, comparable decision data restrict this analysis to the 25 justices serving on the Supreme Court from the beginning of the 1946-47 term to the end of the 1977-78 term. The group includes all but two of Franklin Roosevelt's seven appointees and the appointees of subsequent presidents through Gerald Ford.

Decision Behaviors. In the period since the end of World War II, the United States Supreme Court has devoted most of its efforts to resolving cases concerning two broad substantive issues: civil rights and liberties, and economics. Over two-thirds of all decisions rendered since 1946 have been unambiguously classifiable into these two categories. No other issue accounted for as much as one-sixth of all decisions. (See Ryan and Tate, 1975, p. 11.) Almost every case decided in these two categories can be classified as having a "liberal" or "conservative" outcome, depending upon whether the Court's majority favored the claims put forward by civil liberties claimants or economic "underdogs." Similarly, the votes of the individual justices can be classified as liberal or conservative according to whether they favored or opposed the legal demands of civil liberties claimants and economic underdogs.

Such classification is by now routine in studies of judicial decision making. Specifically,

> in a civil liberties case, justices who were in favor of granting (or granting more of) the claimed civil right were classified as supporting the *liberal* position; justices who were opposed to granting (or who wanted to grant less of) the claimed right were classified as supporting the *conservative* position. . . . *Liberals* in economics cases support unions over management, government regulation of business activities, workers claims against employers, and small businessmen over large corporations. *Conservatives*, then, do the opposite (Ryan and Tate, 1975, p. 13).

Cases which could not be clearly classified as either civil rights and liberties or economics cases were excluded from the analysis, as were civil rights and liberties and economics cases in which the outcome could not be classified as liberal or conservative.

The availability of legally respectable liberal and conservative positions to individual justices depends upon the Court's decision being nonunanimous, which is usually the case. For this reason, the decision data analyzed here are drawn from the justices' voting in all split decisions in civil rights and liberties and economics cases reported from October 1946 through September 1978 (N = 2327). From these data, two simple measures of Supreme Court justice decision behavior have been calculated:

1. Percent liberal of all civil rights and liberties case votes cast by the justice (%LIBCL);
2. Percent liberal of all economics case votes cast by the justice (%LIBECON).

The number of cases falling into the substantive areas and summary statistics for these dependent variables and their relationship are reported in Table 1.

The use of these percentages deserves some discussion. Since they do not represent judicial responses to identical sets of case stimuli, comparing them requires the assumption that each justice's particular set of, say, civil rights and liberties cases constitutes an essentially random sample of all possible civil rights and liberties cases to which he could have responded. Under this assumption, each justice's proportion of liberal votes in the sample cases is an unbiased estimate of the proportion of liberal votes he would have cast in the universe of all possible civil rights and liberties cases. Such estimates are subject to sampling error, of course, but are directly comparable.

Clearly it is not possible to demonstrate unequivocally the validity—or invalidity—of the above assumption. I will compare Supreme Court justices' voting percentages because the percentages are superior to any other measurement of the voting ideologies of the justices which might have been appropriate for this analysis. The percentages are simple, robust measures of the behavior of the justices across their entire careers. They constitute macro-level measures of the central tendencies of the justices' voting. In addition, the percentages appear to have strong face validity as measures of what scholars and lay observers of the Court mean when they label particular justices "liberals," "conservatives," or "moderates."

There are several reasons for using these percentages to compare the behavior of justices with non-overlapping or only partially overlapping careers. Such a practice is consistent with the literature on the relation between judges' characteristics and judicial decision making. Bowen (1965), Johnston (1976), Lindstrom (1968), Rowland and Carp (1978), Ulmer (1973), and Vines (1964) all used percentage measures to compare the voting of judges deciding different sets of cases, while Goldman (1966, 1975) used a simple linear transformation of a percentage score incorporating a middle classification position for partial support of the liberal position. In fact, of the backgrounds-decisions studies surveyed for this study, only those of Nagel (1961, 1974) used different measurements of dependent decision be-

Table 1. Liberalism in Voting on Civil Rights and Liberties and Economics Decisions in the U.S. Supreme Court, 1946-1978

	Civil Rights and Liberties Cases (% LIBCL)		Economics Cases (% LIBECON)	
Justice	Votes (N)	% Liberal	Votes (N)	% Liberal
Black	1044	73.3	740	85.4
Reed	282	11.0	305	45.2
Frankfurter	487	46.6	452	21.0
Douglas	1230	93.4	772	82.1
Murphy	87	94.3	110	96.4
Jackson	218	36.7	208	13.9
Rutledge	89	80.9	113	96.5
Burton	367	20.4	373	30.3
Vinson	208	17.3	217	41.5
Clark	685	25.3	532	65.2
Minton	181	16.0	177	52.0
Warren	743	79.1	510	82.0
Harlan	795	23.3	477	23.3
Brennan	1120	81.4	553	74.5
Whittaker	195	26.3	169	17.2
Stewart	1038	45.8	464	35.6
White	884	41.5	346	58.4
Goldberg	167	89.2	100	66.0
Fortas	248	84.3	102	67.6
Marshall	569	84.2	154	67.5
Burger	450	16.2	120	20.0
Blackmun	420	26.0	109	32.1
Powell	351	30.8	93	36.6
Rehnquist	352	4.5	96	15.6
Stevens	148	54.7	39	48.7
	Cases (N)		Cases (N)	
COURT	1452	50.6	875	60.5
Mean (25 Justices)		48.1		50.9
Standard Deviation		30.2		26.3
Correlation (r) with % LIBCL		1.000		.7972

Source: Computed from data accompanying John Paul Ryan and C. Neal Tate, *The Supreme Court in American Politics: Policy Through Law,* 2nd ed., Washington, D.C.: American Political Science Association, 1980.

havior. Even so, Nagel's measure still required assuming that the sets of decisions heard by different courts constituted essentially random samples of all the decisions which could have been heard by the courts concerning a particular issue.

Not only are the percentage measures consistent with the prevailing practice in the literature, they also are highly correlated with a variety of alternative measures used in comparing the justices' behaviors. Thus the results obtained using them would converge strongly with results obtained using any of these alternative measures. For example, the simple correlations between the percentage measures reported in Table 1 and Schubert's "composite scale rankings" for the 20 justices analyzed both here and in his *The Judicial Mind Revisited* (1974, p. 60) were —.94 for civil rights and liberties percentages-C scale rankings and —.88 for economics percentages-E scale rankings. The composite scale scores were derived from Schubert's analysis of a series of "dominance matrices" which sought to determine the relative ordering the 20 justices *would have exhibited* had they all voted on the same C and E scale cases.

If one analyzes like correlations for the groups of 9 justices who actually sat together and decided essentially the same cases in the 10 "natural courts" covered by Schubert's analysis, one finds a similar result. The mean correlations between the *overall* voting percentages reported in Table 1 and the justices' ranks on Schubert's C and E scales for the 10 natural courts are —.89 for the civil rights and liberties percentages-C scale rank scores, and —.92 for the economics percentages-E scale rank scores.

The percentage liberal scores used here correlate highly with the C scale and E scale ranks of the 10 different groups of Supreme Court justices represented in Schubert's natural courts. This indicates that a justice's voting record across his entire career is an acceptable estimate of his voting at different times during that career, despite differences in cases and colleagues. Beyond this it is not possible to go. One can never demonstrate conclusively that a justice with a conservative voting record during the late 1960s and 1970s might not have been a moderate or even a liberal had he encountered the cases and colleagues of the late 1940s and early 1950s. To require such a demonstration is simply to prohibit multivariate analysis of the behavior of Supreme Court justices, since one will never be able to be sure of the relative liberalism or conservatism of more than the 9 justices serving at any one time.[1]

Independent Variables. The independent attribute variables used here to predict Supreme Court justices voting behavior were selected according to availability and use in previous research. Information on the justices' personal attributes was coded from readily available sources such as biographical directories (e.g., *Who's Who in America*) and the data collections of scholars such as Schmidhauser (1959), Krislov (1972), and Abraham (1974, 1975).

All significant attributes which theory or previous research suggest as relevant to Supreme Court decision making can be operationalized from available data. As far as possible, *all* characteristics found to be potentially important in previous research are investigated. Indeed, because the treatment of some characteristics has varied across previous studies, several version of some traits (age, for example) have been created for consideration for inclusion in the prediction equations. Table 2 summarizes the attributes investigated.

The attributes are divided into four major categories. The first category includes traits relating to the birth, upbringing, and education of the justices.[2] Previous research suggests that, among these traits, religion should be expected to have the greatest impact on the justices' voting behavior in civil liberties and economic cases, and that the magnitude of its impact should be greater for civil liberties than for economics (see Ulmer, 1973b; also see Nagel, 1974; Goldman, 1975; Bowen, 1965; Vines, 1964). Scattered evidence can be found to support expectations that each of the other variables in this category will also be related, weakly, to the justices' voting in either civil liberties, economics, or both.[3]

The second category in Table 2 lists the justices' career characteristics. Ulmer's research (1973b) suggests that federal administrative experience should be negatively related to a justice's liberalism on civil liberties issues; Johnston's study (1976) indicates that having been either a federal judge or prosecutor should be negatively related to a justice's liberalism on both civil liberties and economic issues.[4] Lindstrom's work (1968) showed prior judicial experience to be negatively related to a justice's liberalism on civil liberties questions. However, Vines (1964) showed that having held federal elective or judicial office is positively related to southern district judges' decisions on desegregation issues. Most research focusing on non-Supreme Court judges suggests that there are few important relationships between judges' career traits and voting (see Goldman, 1975; Nagel, 1974; Bowen, 1965).

The third category in Table 2 consists of measures of the justices' age and tenure. *Appointment* and *End Tenure Years,* essential for calculating the other age and tenure variables, were also considered in the analyses in order to detect secular trends in Supreme Court justice voting.

A number of researchers have found relation-

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[1] The problem is essentially one of equivalence of measures across the "cultures" represented by differing judicial careers. Students of comparative politics recognize that there are no easy solutions to such problems. But they also recognize that difficulties in establishing equivalence of measurement do not justify abandoning comparative research. (See Holt and Turner, 1970; Przeworski and Teune, 1970; Warwick and Osherson, 1973.)

[2] It proved impossible to code reliably a more direct measure of family status than the education status index. The index is very similar to that used by Bowen (1965, p. 13) to measure family status.

[3] See Bowen (1965, p. 199) for the impact of region and status and Nagel (1974, pp. 266-67) for the impact of prestige of legal education and size of town and Vines (1964) for the impact of southern origins on civil liberties voting.

[4] Exploratory analyses of these data revealed the apparent nature of the statistical interaction which Johnston's "judge of prosecutor" measure may have been tapping. Justices who had been prosecutors were less liberal than non-prosecutor judges, but prosecutor-justices with judicial experience were substantially more liberal than prosecutor-justices without such experience, though still less liberal than non-prosecutor justices. Since this interaction effect can be meaningfully interpreted in terms of the moderating influence of judicial service on the basic civil liberties conservatism induced by prosecutorial experience, it is represented in the *Type of Prosecutorial Experience* index.

Table 2. Supreme Court Justices' Personal Attributes Investigated with Their Operational Definitions

Category 1: Birth, Upbringing, Education

1. Year of birth: last 3 digits coded.
2. Region of birth: 1 = South or border state; 0 = other.
3. Religion: 1 = Non-Protestant; 0 = Protestant.
4. Size of town of upbringing: 1 = urban area; 0 = other.
5. Educational prestige measures
   a. Prestige of prelaw education: 1 = high; 0 = average.
   b. Prestige of legal education: 1 = high; 0 = average.
   c. Education status index: sum of 5a and 5b; range 0-2.

Category 2: Career Characteristics

6. Judicial experience measures
   a. Any judicial experience? 1 = some; 0 = none.
   b. Extensive judicial experience (5 or more years)? 1 = Yes; 0 = No.
   c. Extent of judicial experience index: 2 = 5 or more years; 1 = some, less than 5 years; 0 = none.
   d. Some judicial experience? 1 = some, but less than 5 years; 0 = none or more than 5 years.
7. Compound experience measures
   a. Served as either federal judge or federal prosecutor? 1 = yes; 0 = no.
   b. Type of prosecutorial experience index: 2 = none; 1 = prosecutors with judicial experience; 0 = prosecutors with no judicial experience.
8. Ever hold elective office? 1 = yes; 0 = no.
9. Ever serve in Department of Justice? 1 = yes; 0 = no.
10. Office held at appointment
    a. Federal administrative office (cabinet, etc.)? 1 = yes; 0 = no.
    b. Judicial office? 1 = yes; 0 = no.
    c. Elective office? 1 = yes; 0 = no.
    d. No office held? 1 = yes; 0 = no.
11. Appointment region: 1 = South or border state; 0 = other.

Category 3: Age and Tenure

12. Age at appointment: in years.
13. Appointment year: last 3 digits coded.
14. Year tenure ended (1978 for those still serving): last 3 digits.
15. Tenure on Supreme Court: in years.
16. Age at mid-tenure: in years.
17. Age cohort: 1 = birth post-1900; 0 = birth pre-1901.

Category 4: Partisanship

18. Justice's party identification: 1 = Democrat; 0 = non-Democrat.
19. Appointing president's party identification: 1 = Democrat; 0 = Republican.
20. Party identification indexes:
    a. Party index 1: 3 = justice and appointing president Democrat; 2 = justice Democrat, appointing president Republican; 1 = justice non-Democrat, appointing president Democrat; 0 = justice non-Democrat, appointing president Republican.
    b. Party index 2: 5 = justice and appointing president Democrat; 4 = justice Democrat, appointing president Republican; 3 = justice Independent, appointing president Democrat; 2 = justice Independent, appointing president Republican; 1 = justice Republican, appointing president Democrat; 0 = justice and appointing president Republican.

Continued on next page

Table 2. (continued)

Category 4: Partisanship (continued)

21. Appointing president:
    a. Roosevelt: 1 = yes; 0 = no.
    b. Truman: 1 = yes; 0 = no.
    c. Eisenhower: 1 = yes; 0 = no.
    d. Kennedy: 1 = yes; 0 = no.
    e. Johnson: 1 = yes; 0 = no.
    f. Nixon: 1 = yes; 0 = no.
    g. Ford: 1 = yes; 0 = no.

Source: Operational definitions devised by the author.

ships between age (variously operationalized) and judicial voting, particularly concerning civil rights and liberties (see Ulmer, 1973b; Lindstrom, 1968; Goldman, 1975). However, others (Nagel, 1974; Bowen, 1965) have found age to be of little value in predicting how judges vote. On balance, the evidence appears to suggest that one should expect to find a relationship between a measure of age and Supreme Court voting behavior, especially in civil rights and liberties. The same is *not* true for tenure: none of the studies cited here demonstrated any substantial or consistent impact of length of service on judicial voting in any type of decision.[5]

The final category in Table 2 is "Partisanship." Many studies have found judges' party identifications to be among the most important influences on their voting across all kinds of issues. (See Johnston, 1976; Lindstrom, 1968; Goldman, 1975; Nagel, 1974; and Bowen, 1965; plus many of the "pioneering studies": Schubert, 1959; Nagel, 1961; Ulmer, 1962; and Goldman, 1966.) Although Ulmer's study (1973b) did not find the justice's party identification to be an important predictor of criminal case voting, it is anticipated here that it will be a significant predictor of Supreme Court justice voting in civil rights and liberties and economics cases.

The correlation (r = .67) between the party identifications of the justices and those of their *Appointing Presidents* makes it problematic that both variables will make independent contributions to the explanation of the justices' voting (but see Lindstrom, 1968, pp. 70-71). The "Party Index" attempts to measure the joint effects of the two identifications.

Because appointing presidents are often very self-conscious about the need to appoint justices who have the correct ideology or "real politics" (Scigliano, 1971), the significance of this variable for shaping Supreme Court decision making would seem obvious. Surprisingly, however, none of the backgrounds-decision studies cited above makes an effort to include the identity of the appointing president as a predictor of judicial voting. This exclusion does not seem justified. The potential significance of *Appointing President* is suggested by some of the conclusions of a recent massive study of the influence of background characteristics on the voting behavior of federal district judges:

> This study presents updated information to reaffirm our belief that the appointing President is a variable of considerable importance in influencing the behavior of trial judges. For example, the percentage of Johnson appointees voting on the liberal side of judicial questions is some 21 points higher than for Nixon appointees for the same years. Our investigation further suggests that the variables of political party affiliation and appointing President are not entirely synonymous. That is, some Presidents have obviously selected (or been able to select) district court judges with greater ideological care than others. . . . Thus . . . to a fair degree, Presidents tend to get out of their judicial appointees the kind of decisions they want from them (Rowland and Carp, 1978, pp. 27-28).

It is anticipated that the appointing president will be a significant influence on Supreme Court voting behavior in civil liberties and economics cases.

To include all the background variables listed in Table 2 in prediction equations for the civil liberties and economics voting behavior of the 25 justices would not have been possible, even if it were wise. The number of variables would have exceeded available degrees of freedom. Furthermore, several of the variables were quite substantially correlated, either because they represented merely alternative ways of coding the same basic characteristic, or because they reflect true "real world" covariances. To include such variables would have led to intolerable multicollinearity in the prediction equations. Beyond such limiting technical considerations, however, was the necessity for parsimony. Good personal attribute explanations of Supreme Court voting behavior should rest upon as few, and as potent, interpretable independent varibles as possible.

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[5] But see Lamb (1976) for some suggestive, though ultimately inconclusive, results on increasing conservatism with aging among court of appeals judges, and Ulmer (1973a, 1979), who shows that a justice's degree of liberalism may vary somewhat across the terms of his tenure. In fact, Ulmer suggests that such variation may even follow distinct curvilinear patterns for some long-serving justices. Of course, the fact that a variable has a variance does not prohibit meaningful analyses based on its central tendency, and it is that sort of analysis which is the focus of this article.

Statistical Procedure. Stepwise regression analysis, including both forward inclusion and backward elimination procedures, was used to assist in determining which attributes were sufficiently potent and stable to merit reporting in the final regression models of "Civil Liberties Liberalism" (%LIBCL) and "Economic Liberalism" (%LIBECON). However, the final composition of these models was in no way the choice of the computer's stepwise algorithms. On the basis of previous research, I assumed that the final models would contain measures of partisanship, particularly justice's party identification and appointing president, religion, some kinds of career experience indicators, and, perhaps, age. I did not automatically accept models emerging from the stepwise selection process which failed to include measures of these attributes. Rather, I estimated the statistical properties of alternative models including these attributes but excluding previously included attributes which were collinear and thus competing with these attributes. In addition, I computed models including alternative measurements of those attributes which *had* alternative measurements and compared them for their statistical potency and stability.

A variable found important in previous research was excluded from the final regression equations only when all model-building and testing procedures suggested that it had no explanatory power or that its explanatory power was a spurious result of its correlation with other, more potent and stable attributes. These final equations, presented in Table 3, included only predictors which were substantively appealing, statistically robust, and minimally collinear; which were highly statistically significant (probability of F for the regression less than .001); which contained only predictors with t values whose absolute magnitude exceed 1.25; and which yielded the maximum adjusted R^2.

Table 3. Personal Attribute Models of Liberalism in Voting in Civil Rights and Liberties and Economics Cases for U.S. Supreme Court Justices, 1946-1978

Liberalism on Civil Rights and Liberties Cases (% LIBCL)				Liberalism on Economics Cases (% LIBECON)			
Independent Variable	Beta	B	t	Independent Variable	Beta	B	t
Party Identification	.48	29.1	4.58	Party Identification	.73	38.4	4.78
Appointing President:				Appointing President:			
Truman	−.54	−43.9	−5.65	Truman	−.26	−18.5	−1.92
Johnson	.14	14.9	1.45	Nixon	−.19	−13.4	−1.34
Nixon	−.24	−19.2	−2.27	Prestige of Prelaw Education	.19	10.0	1.37
Appointment Region	−.34	−22.2	−3.14	Appointed from Elective Office	.34	27.0	2.20
Extent of Judicial Experience	.17	6.3	1.89	Extent of Judicial Experience	.31	9.9	2.33
Type of Prosecutorial Experience	.56	24.2	5.95	Type of Prosecutorial Experience	.29	10.7	1.97
Intercept		12.0				4.3	
R^2		.87				.72	
Adjusted R^2		.82				.61	
Standard Error of Estimate		13.0				16.5	
F (d.f.)		16.12 (7,17)				6.27 (7,17)	
Probability of F		<.00005				<.00005	
Determinant of Correlation Matrix of Predictors		.45				.44	

Source: Computed from data accompanying John Paul Ryan and C. Neal Tate, *The Supreme Court in American Politics: Policy Through Law,* 2nd ed., Washington, D.C., American Political Science Association, 1980.

Findings

Each attribute model contains seven independent variables. Five variables, *Party Identification, Extent of Judicial Experience, Type of Prosecutorial Experience, Truman Appointee,* and *Nixon Appointee,* are included in both models, thus reflecting the substantial zero-order correlation of these two forms of judicial liberalism. Both models are statistically robust and predictively successful. The model for %LIBCL explains 87 percent of its variance; that for %LIBECON 72 percent of its variance.

Neither model's explanatory power rests to any appreciable degree upon the relatively limited residual degrees of freedom remaining after the seven variables are introduced into the regression equations: the Adjusted R^2 for %LIBCL is .82; for %LIBECON, it is .61. Both models are highly statistically significant, since the probability of their F coefficients is less than one in 20,000. Finally, neither equation is plagued by serious multicollinearity.

Given the inevitable crudity of measurement of the independent variables and the prevailing scholarly understanding that such attributes must be limited in their ability to account for variations in judicial decision making, the levels of statistical explanation achieved by the models must be regarded as impressive.

Civil Rights and Liberties Liberalism. The model depicts the justices' voting on civil liberties cases as a product of their career experiences and partisan affiliations. Its coefficients predict the most liberal score for the fictitious justice who is an experienced judge without prosecutorial experience, and who is a Democrat appointed from a nonsouthern state by Lyndon Johnson. Conversely, its most conservative score is predicted for the also fictitious justice who was a prosecutor without judicial experience, and who is a Republican (or Independent) appointed from the South by Harry Truman.

The %LIBCL model contains seven independent variables, but actually represents only five distinct attributes, since two of the independent variables are dichotomies measuring *Appointing President*. The most significant of these variables, relatively speaking, are *Party Identification, Truman Appointee,* and *Type of Prosecutorial Experience*. It is worth noting that the %LIBCL model does *not* include religion as an independent variable. This finding is surprising, since in the literature Protestantism was consistently associated with conservatism on civil liberties, and since religion had the highest zero-order correlation with %LIBCL. However, extensive analysis of these data has made it clear that, once the impacts of the variables listed in Table 3 have been taken into account, religious affiliation adds little or nothing to the explicability of %LIBCL.

The attributes included in the model for %LIBCL all have sound and interpretable relationships with civil liberties liberalism. Party identification is shown to affect the behavior of America's top judicial elite in the same way it affects that of other political elites and the masses. The influence of the appointing presidents' presumed concern with the values of their appointees is clearly documented. Judges appointed from the southern states are shown to be less favorable toward civil liberties claims than their northern counterparts. And finally, the important interactive efforts of two types of career experience—judicial and prosecutorial—are documented: nonprosecutors are shown to be much more favorable toward civil liberties claims than their prosecutor colleagues, as one would predict, while, among prosecutors, those with some judicial experience are likewise more favorable than those without it, reflecting the moderating influence of sitting on the other side of the bench. Although the most important impact of judicial experience occurs in the just-mentioned moderating effect it has on the behavior of prosecutors, its influence is not limited to that group of justices. The regression coefficient for *Extent of Judicial Experience* demonstrates that the longer a justice's prior service as a judge, the greater his willingness to support civil rights and liberties claims, regardless of his other characteristics.

Economic Liberalism. The overall relationship between civil liberties and economic liberalism is fairly strong (r = .797). Thus one should expect considerable congruence between the regression models for these two kinds of behavior. The % LIBECON model in Table 3 shows that congruence. But it also is sufficiently different from the %LIBCL model to make a separate attribute analysis of economic liberalism worthwhile.

The economic liberalism model predicts the most liberal score for the mythical justice who is a Democrat appointed from elective office, but with extensive judicial and no prosecutorial experience, who attended a high-prestige prelaw school and was not appointed by Harry Truman or Richard Nixon. The most conservative score, conversely, is predicted for the again mythical Republican (or Independent) justice who attended an average-prestige undergraduate school, was not appointed from an elective office, had prosecutorial, but not judicial, experience, and was appointed by Harry Truman.

The substantive interpretations of the relationships in the economic liberalism model are generally clear and meaningful. The most consistent dimension related to American partisanship since the 1928-36 realigning election period has been that measured by the economic liberalism index used here. It is thus highly appropriate and consistent with previous research to find that the Supreme Court justices' party identifications are the most important influences on their voting behavior on economic issues. The relationship for appointment from elective office is also meaningful. Mass electorates are more liberal on economic questions than elites. Thus lawyers who have found favor with a mass electorate are likely to be more liberal than their counterparts who continue to labor mostly in the service of the socioeconomic elites. The relationship between %LIBECON and *Prestige of Prelaw Education,* which at first glance may appear to go in an unexpected direction, may also be comfortably interpreted as reflecting the economic conservatism of the self-made man who lacked the initial advantage of a high-prestige education.

The other relationships in the economic liberalism model reflect, with differing magnitudes, relationships in the %LIBCL model. The influence of appointing presidents is again revealed. So are the impacts of judicial and prosecutorial experience. The interpretations of the latter are not as clear-cut in the case of economic liberalism as they were in the case of civil liberties liberalism. But it is clear that judicial experience is a liberalizing influence in the Supreme Court justices' voting on economic issues. It seems likely that judges are more removed from service to the interests of the economically privileged than most non-judge lawyers, and that the longer their service on the bench, the more they find the protection of the interests of economic underdogs to be a central part of their judicial duties. Prosecutorial experience is, on the other hand, a conservatizing influence. Perhaps the effort to convict defendants, who are overwhelmingly economic underdogs, leads prosecutors to have little sympathy for the claims of economic underdogs outside criminal cases. More generally, prosecutors spend most of their time defending the position of the "haves" against the criminal attacks of the "have nots." Such experience would logically engender sympathetic attitudes toward economic "top dogs."

Discussion

The success of the personal attribute models presented here is quite impressive. Statistically, there can be little doubt that they overcome the supposedly inherent limitations on the explanatory potential of background models discussed by the authors cited earlier. If an attribute model can, contrary to prediction, produce strong rather than weak or moderate associations with judicial voting behavior, is one justified in rejecting the argument that attributes are only indirect causes of judicial behavior? I think not. The models reported here refer only to one relatively unique court, and only to the 25 justices serving on that court in the period since the end of World War II. It is not clear that similarly successful models can be constructed for the judges of lower fededral courts or state appeals courts. Thus the findings reported above must be regarded as suggestive, but hardly conclusive, on the utility of attribute models in explaining the decision making of judges in general. Despite this caveat, it is worth exploring why the attribute models presented here achieve very positive results.

Leaving aside all theoretical considerations, it is clear that these models are predictively successful because of the data and measurement procedures used. This research paid more attention to identifying and including significant measurable attributes relating to the justices' birth and socialization, career, age and tenure, and partisanship than have many previous studies. Equally important, it paid close attention to the operationalization of indicators for the attributes included. Multiple indicators of important attributes were constructed and tested for their predictive efficacy and statistical stability; indicators reflecting potential nonlinear or non-additive effects were constructed and tested for their contribution. Because previous attribute models have often been based on the most readily available data and have used crude and sometimes unimaginative operationalizations of potentially crucial variables and only straightforward linear and additive regression models, it is not surprising that their results should have been less impressive than those reported here.

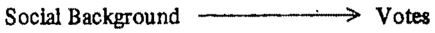
Skeptical readers may doubt that better measurement and model-building alone could produce these results. In addition, the theoretically inclined reader may assert that attribute models still fly in the face of good theory concerning judicial decision making, no matter how statistically potent they may be. In answer to both, I would argue that at least two other factors may underlie the explanatory success of the attribute models. First, attribute models may not (or need not) be as far removed from properly specified models of judicial decision making as has usually been assumed in the literature. Second, judges' attitudes such as socio-political values or role orientations, which have been assumed to be crucial intervening variables between their attributes and decisions, may be such proximate causes of judicial decisions that they are, for many analytical purposes, all but indistinguishable from decisional behavior. If this should be so, then attribute models could provide highly statistically satisfactory explanations of judicial behavior even though they failed to include the unmeasured (and often unmeasurable) attitudinal variables which intervene between the attributes and decisions in the models.

Figure 1 details three models of judicial voting which Murphy and Tanenhaus (1972, p. 109) put forward to illustrate why social background variables are likely to be only weakly related to decisional behavior such as voting. Figure 1a, which represents a simplistic, straightforward link between social backgrounds and votes, is the model which, Murphy and Tanenhaus contend (1972, p. 108), underlies studies of the impact of social backgrounds on judicial voting behavior. Such a model "so oversimplifies the real world as to be of slight use." Reality, they hypothesize, might better be represented by more intricate models such as those in Figures 1b and 1c.

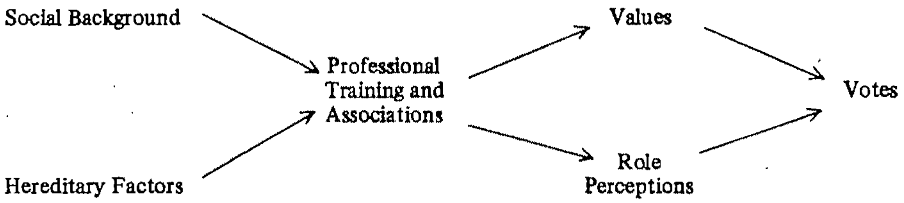
Certainly the models in Figures 1b and 1c represent richer and more theoretically appealing explanations of judicial decision making than that in Figure 1a. However, it is a mistake to conclude that attribute models must reflect the simplistic logic of Figure 1a. The models tested in this research included not only measures of what Murphy and Tanenhaus appear to mean by "social background" (e.g., size of town of upbringing, educational prestige), but also indicators of "hereditary factors" (birth region, age) and a broad range of measures of the justices' "professional training and associations" (judicial and prosecutorial experience, holding of elective office, etc.). Thus the models in Table 3 are not so far removed from "realistic" models of judicial voting as might have been assumed.

The key difference between the attribute models studied here and the most elaborately specified model in Figure 1c is the former's exclusion of the two categories of attitudinal variables, values and role perceptions, and the decision processes variable category. The exclusion of decision processes is entirely proper for the dependent variables studied here. Decision processes may well intervene between a judges' attitudes, values, and role perceptions and his decision to case a particular vote or even a few votes in a direction not indicated by these characteristics. However, over the hundreds or even thousands of votes cast by the Supreme Court justices included in this study, the influence of decisional processes should be minor and self-canceling. If decisional processes caused a justice to cast a conservative vote on one case when other factors would have produced a liberal vote, they would probably cause him to cast a liberal vote when other factors would have produced a conservative vote on another case. The result would be to remove the influence of

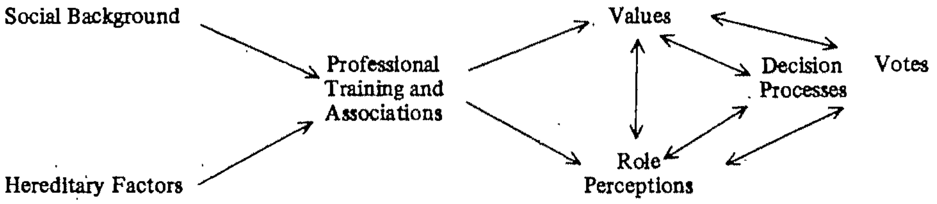
(a) Social Background as Simple Direct Cause of Votes



(b) Social Background and Other Attributes as Indirect Causes of Votes



(c) Social Background and Other Attributes as Indirect and Remote Causes of Votes



Source: Slightly adapted from Walter F. Murphy and Joseph Tanenhaus, *The Study of Public Law,* New York, Random House, 1972, p. 109.

Figure 1. Potential Models of Judges' Voting Behavior

decision processes from models of the career voting behavior of the Supreme Court justices.

The failure of the attribute models considered to include measures of the justices' values and role perceptions is potentially more serious from a theoretical standpoint. There is good evidence to suggest that the voting behavior of Supreme Court justices reflects their values, perhaps fairly closely (see Schubert, 1974; Spaeth, 1979, for example). And while the evidence for the impact of role perceptions on decisions is more mixed (see Vines, 1969; Howard, 1977), recent research (Gibson, 1978) indicates that the interaction of values and perceptions of roles may be very important in explaining at least some kinds of decisional behavior for at least some judges.

Unfortunately, there is no practical way of including measures of the Supreme Court justices' values and role perceptions in this analysis. The first problem is operationalization of these attitudinal measures. Studies of the values of a substantial number of Supreme Court justices have invariably operationalized values from behavioral indicators, usually votes. While criticism has been leveled at the inferential leap involved in defining values from votes, there is no reason to object to this inference as long as the inferred values are not then used to "explain" the judicial voting used to document their existence. Since it is my avowed objective to explain differences in justices' career voting behavior, the usual method of inferring values from votes cannot be used. Some independent measures of values would be required.

It is difficult to imagine what independent measure of judicial values could be used in this study. Attitude surveys, often used to measure the values (and role perceptions) of sitting lower court judges, have their limitations when most of the justices being analyzed are dead. An alternative method, inferring values from written works of judges (Danelski, 1966), might be applicable in principle, but would require a research investment which is well beyond the resources of most researchers if applied to 25 justices.

The independent operationalization of role perceptions poses no more tractable a problem. Dead justices cannot respond to role inventories even if living ones should prove willing to do so, and the content analysis of written materials in search of role perceptions would be just as impractical as for values.

Given the impossibility or the extreme difficulty of operationalizing values and role perceptions for the Supreme Court justices analyzed here, to argue that attribute models are unsatisfactory unless they include such variables is to reject the possibility of useful research on the explanation of Supreme Court justice voting behavior. It does more to establish and protect intellectual sacred cows than to advance our knowledge of judicial behavior. However, before abandoning this discussion of values and role perceptions, one should note once again that the models reported in Table 3 do include at least one attribute—*Appointing President*—which may more or less directly index the values and role perceptions held by Supreme Court justices. The evidence suggests (see Abraham, 1974; Scigliano, 1971) that presidents pay close attention to the values and role perceptions which their potential appointees are likely to bring to the bench. Scigliano (1971, pp. 125-48) concludes that they are much more often than not successful in appointing justices who will display presidentially desired values or role orientations on the Supreme Court. It seems likely, therefore, that the presence and importance of the appointing president measures in the models of Table 3 reflects the values and role perceptions of justices selected by self-conscious presidents. If this can be accepted, the attribute models studied here end up being very like the more theoretically appealing model depicted in Figure 1b, and far removed from the straightforward model of Figure 1a.

The preceding paragraphs have demonstrated that attribute models of judicial decision making need not be so simplistic or atheoretical as has often been assumed. Their explanatory potency may reflect their de facto theoretical appropriateness. There is another possible explanation for the statistical potency of the attribute models. It may be that attitudes reflecting values and role perceptions, the intervening variables ostensibly excluded from attribute models, are so closely linked in a causal sense to voting that one can serve as an operational indicator of the other. The discussion has already noted that judicial values have often been inferred from judicial voting, while the evidence indicates that role perceptions and values are ordinarily substantially correlated (see Howard, 1977; Gibson, 1978). If votes and attitudes are so closely linked that one may be inferred from the other, it would appear that for most analytical purposes they may be indistinguishable. A model which purported to explain votes would be just as good in explaining values. And since attributes are the proximate causes of values and role perceptions in Figure 1b, one might expect attribute models of values/votes to be quite successful predictively.[6]

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[6] Analolgous situations arise in the literature on mass voting behavior. For example, a recent attempt to develop a nonrecursive causal model of American presidential voting choice actually presented models of the causes of voter evaluations of the candidates, since the latter was such a powerful proximate cause of voting choice as to make separate analyses of vote superfluous

The substantial explanatory powers attached to the models in this study may reflect a close causal linkage between judicial values and judicial votes. If values could be independently measured and included in voting models, they might account for virtually all the variance in judicial voting, with the influence of attributes being almost totally channeled through values. Since they cannot be independently measured for inclusion in the models, the influence of attributes is transmitted directly and powerfully to judicial voting behavior.

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Political Decision Making in Face-to-Face Groups: Theory, Methods, and an Empirical Application in Switzerland

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This article presents a model of decision making and introduces a new theoretical variable to the literature, namely, the modes of decision making. The theoretical focus is on the face-to-face group, and the article also develops an innovative methodology for studying this type of decision-making behavior. Variation in the decision modes is explained as a function of the strategic considerations of individual decision makers. These considerations are affected by a set of four independent variables: structure of the decision group, substance of the conflict, context of the conflict, and the decision process. The data, drawn from observations of decision-making groups in Switzerland, are tested with discriminant analysis and a simulation. In both cases total correct classifications exceed 55 percent, indicating that there is a meaningful structure relating variation in the decision modes to the theoretical framework.

How the political scientist should study decision-making behavior remains a perplexing and unresolved issue. The discipline has witnessed the rise (and fall?) of numerous competing approaches, ranging from the exclusively macro-level systems approach and its now much-maligned "black box" to the spate of economic models focusing on the individual, rational decision maker acting largely in isolation. Until fairly recently, political scientists have appeared somewhat wary of studying decision making at a level of analysis that falls somewhere in between these macro- and micro-analytic approaches, namely, at the level of the "face-to-face" group. In other disciplines, notably social psychology, a great deal of research on face-to-face groups has been conducted. Kirkpatrick (1976) points out the utility to political scientists of such a theoretical focus in a special issue of the *American Behavioral Scientist* which focuses on small group theory, while Golembiewski (1970) contains a collection of essays on precisely this topic. Other examples of useful research in this area include Fiorina and Plott (1978), Hinckley (1979), Sullivan (1976), and Verba (1961).

The purpose of this article is to explore the possibility of developing a general theory of political decision making in face-to-face groups. In an earlier study (1980b), our theoretical concern was focused largely at the macro-level of whole political systems: we attempted to demonstrate that what was understood conceptually as "consociational democracy" was in reality a much more complex set of decision-making norms and behavior than heretofore assumed by the theory. In the present context we want to demonstrate the extent to which our theoretical framework can contribute to a broader understanding of a general class of political decision-making behavior in committees.

Data: Background and Description

Our data are drawn from observations of the Free Democratic party of the Berne Canton in Switzerland. The basic research method was participant observation. As is so often the case with research of this nature, access to the decision-making groups was largely the fortuitous result of the social scientist happening to be in the right place at the right time. As a member of the FDP, Jürg Steiner was able to secure the much-needed access not only to all the relevant committee meetings, but also to interview data, recorded minutes of meetings, and to other internal and external flows of communication in the party. During the 21 months in which he carried out his observations (January 1969-September 1970), Steiner observed the decision-making behavior in 111 committee meetings. These meetings ranged in size from the very large party conventions (N = 150) to the very small bureau of the executive committee (four members). Consequently, we are not dealing solely with small face-to-face decision-making

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We would like to acknowledge the financial and institutional support of the Swiss National Science Foundation, the Institute for Research in Social Science at the University of North Carolina, Chapel Hill, and the Research Initiation Grant Program of Michigan State University.

groups, but with large, even cumbersome ones as well.

Our units of analysis are individual, openly articulated conflicts in the groups. At least two participants had to express explicit disagreement on a particular point before the disagreement could qualify as a conflict. If a conflict reappeared in a later meeting of the same committee, it was treated as a second case because under these new circumstances the actual decision-making process might very well differ from that of the first meeting. In all, we obtained data on 466 such conflicts.

Dependent Variable: Modes of Decision Making

One of the central problems confronting studies of face-to-face decision-making groups is the selection of an appropriate dependent variable. Most studies focus on the decision outcome itself, attempting to explain the group decision as a function of individual preference orderings. Predicting such outcomes generally requires that the researcher have a fairly extensive knowledge of individual preferences prior to and during the decision process. In real-world settings it is often extremely difficult, if not impossible, to obtain such information; recurrent interviewing of participants can seriously disrupt the normal course of political business in any decision-making group.

In this study we will introduce a dependent variable that has received relatively little systematic attention in the professional literature, namely, the modes of decision making. Rather than focusing our attention on the substantive outcome of a decision, we will look instead at the decision mechanism by which that outcome was achieved. The assumption is that the decision outcome will often be strongly influenced by the decision mode. Thus the innovativeness of group decisions may depend on the mechanisms by which groups arrive at their decisions. In addition, the decision modes may have implications for a wide range of variables that are important from the perspective of normative democratic theory, such as the willingness of group members to participate in further meetings and the level of hostility and antagonism in such groups.

Typology

We have developed our typology of decision modes along three dimensions: (1) Whether or not the substance of the conflict was resolved, (2) the degree of formalization of the decision, and (3) the persistence of dissent within the group. When a conflict arises and disagreement is openly articulated, the first question one needs to ask is whether or not the substance of that conflict is resolved in that particular meeting. Although "deciding not to decide" is obviously a decision of sorts, it is limited to the procedural matter of leaving the conflict unresolved. Since the substance of the conflict remains unresolved, the possibility exists that it will be brought up again later; it is also possible that postponement is tantamount to "killing" a proposal, much as occurs at times in the U.S. Congress when a bill is referred back to a committee. Regardless of the specific reasons for postponement or the eventual outcome of a subsequent conflict concerning the issue, we consider these unresolved conflicts as *no decisions*. Obviously, this term has a much narrower definition than the term *nondecision* originally used by Peter Bachrach and Morton S. Baratz (1962).

If the substance of a conflict is resolved by the group, it may be accomplished in a formalized or informalized manner. The possible advantages of an informal decision mechanism include the fact that it reduces the clear distinction between winners and losers, which can at times help to preserve harmonious intragroup relations, and it allows for subtle manipulation of the decision-making process. An informal decision occurs when one (or more) of the participants simply interprets what he/she considers to be the essence of the discussion, and then succeeds in obtaining the tacit support of the other participants for this interpretation. We have labeled this mode of decision making *decision by interpretation* and we can distinguish three ways in which such interpretations are made: (1) by the committee chairperson in a final summary, (2) by the secretary in the minutes, and (3) by individuals in the decision-making process who tacitly interpret the preference of the group and then direct the discussion in such a way that a decision is implicitly made. One of the central characteristics of a *decision by interpretation* is that, while it does resolve the substance of a conflict, it does not give any clear indication of whether the disagreement in the group persists or vanishes in the course of discussion. This decision mode also provides substantial leeway for those group leaders who are adept manipulators of the decision-making process.

A formal decision mode is one in which the participants who have expressed an opinion in the course of a discussion are required to take a visible stand on the issue when a decision is made. If dissent among group members persists, we will have a *majority decision:* a vote will be taken that will distinguish a winning side from a losing side. The criteria for winning may be a simple plurality, an absolute majority or a qualified majority such as two-thirds of the votes. If, however, the disagreement disappears at the end of the discussion, the resulting decision will be achieved through *amicable agreement.* Such an agreement may be

Table 1. Typology of Decision Modes About Manifest Conflicts in Face-to-Face Groups

	Resolved Conflict		Unresolved Conflict
	Formalized Decision	No Formalized Decision	
Persistence of Dissent	Majority Decision	Decision by Interpretation	No Decision
Disappearance of Dissent	Amicable Agreement		

Source: Compiled by the authors.

given through an oral statement or a gesture such as a nodding of the head. The agreement may also be expressed through a unanimous vote. Obviously, amicable agreement means only that the manifest disagreement has disappeared; whether or not latent conflicts persist among the group members is not considered.

Of these four modes of decision making, three should be quite familiar from everyday usage and various treatments in the professional literature. Of course, different labels have been applied to the mechanisms but *majority rule, no decision,* and *amicable agreement* are not fundamentally new concepts. *Decision by interpretation,* however, is a concept that we developed while working within this theoretical context. As we have shown elsewhere (1980a), we feel that such decisions occur frequently in a variety of contexts.

Theoretical Framework

Having sketched the important characteristics of the four modes of decision making, we move now to a consideration of our theoretical framework. Obviously, our task is to explain under what conditions specific decision modes will be employed in face-to-face decision-making groups. Before we can select the independent variables that we will use in explaining this dependent behavior, however, we must specify the theoretical framework from which we will subsequently derive our explanatory variables. We begin with a set of axiomatic assumptions concerning the general behavior of politicians in face-to-face groups and build our theoretical framework deductively, ultimately arriving at a point from which we can select a set of relevant independent variables and a number of hypotheses relating these variables to variation in the modes of decision making.

In general, we assume that politicians are individual value-maximizers. Moreover, given a set of preferences or a rank-ordering of values, these individuals will seek to maximize those values that they consider to be in their best interests. In this basic sense we share the microanalytic, rational-actor focus of most economic decision-making models. However, our theoretical approach departs significantly from these economic models in our conceptualization of the range of values that politicians maximize and the degree to which these values are assumed to remain constant over time and across issues. Specifically, we assume that the range of values can be accurately identified and subsumed under four general headings: *power, group solidarity, rectitude,* and *time.* In addition, we assume that the individual weights assigned to these values will vary from actor to actor and, for each actor, from situation to situation.[1] This assumption introduces a dynamic component to our theory since we assume that no single value will always be the primary goal for every politician in every decision-making situation. The research task is then to specify under what conditions certain values are likely to be more important than others, and hence how that particular value structure is likely to affect the selection of a decision mode.[3]

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[1] The validity of this assumption is supported in the works of Lehner and Schütte (1976) and MacCrimmon and Messick (1976), for example.

We have also assumed that the selection of a decision mode by a face-to-face decision-making group depends to a significant degree on the values that the members of the group try to maximize. So, for example, one might think of an individual politician who would like to see a most preferred outcome reflected in the group decision. If this politician is concerned only with winning a particular conflict (maximizing power), and if he/she believes the necessary majority support exists, he/she might press for a vote. If, on the other hand, one fears that such a vote might lead to divisive intra-group strife, one might try to "interpret" the decision; this would allow one to achieve the objective of "winning" the conflict while at the same time facilitating the preservation of group solidarity by partially concealing the identities of those members who are in the losing minority. A decision maker who feels that the necessary support for a preferred outcome is lacking might pursue a strategy of *no decision* in the hope that postponement would provide sufficient time to muster that additional support. If the individual preferences are relatively weak and concerns with group solidarity are strong, it seems likely that *amicable agreement* will be the desired strategy. If participants are tired and/or frustrated after a lengthy decision process, calling for a quick vote may be desirable if the primary goal has become finishing the business at hand rather than finding an optimal decision outcome. In other words, the selection of a particular decision mode is assumed to be the aggregate result of the strategic considerations of individual participants in the group decision-making process. These strategic considerations, moreover, will reflect the interplay of the four values included in our model.

Of course, we anticipate that most decision-making situations will be characterized not by the dominance of a single value but by an interplay of the four. In many cases different participants will be motivated by different values in the same decision situation. Consequently, even our relatively short list of four values presents us with a potentially staggering number of logically possible combinations. When we add the further theoretical consideration that the rank-ordering of the four values can change for each individual within each decision situation, it becomes apparent that formulating hypotheses for all of the logically possible combinations would be a nearly endless and perhaps fruitless endeavor. Rather than pursuing this particular avenue in the development of our theory, we try instead to decide on the basis of our independent variables which values are likely to be most important for the largest number of participants (with some additional consideration of weighting for the more powerful group members). From this information we then make probabilistic estimates of the likelihoods of occurrence for the four modes of decision making. Although we will therefore be unable to test empirically each of the posited relationships between the values and the decision modes at the micro-level, we will be able to determine whether our more aggregate predictions based on these microanalytic linkages correspond to the observed outcomes. In the end, the fit between our model's predictions and the observed outcomes should provide some measure of the overall plausibility of the micro-level theoretical reasoning that led to the predictions. The next question, then, is: what variables influence the values that political decision makers maximize and hence, the modes of decision making that a face-to-face group employs?

Independent Variables and Hypotheses

Previous research and our knowledge of the relevant literature have helped us arrive at four sets of potential explanatory variables. These sets are organized under the following headings:

1. Structure of the decision group
2. Substance of the conflict
3. Context of the conflict
4. Decision process

It seems wholly plausible that the values that an individual decision maker chooses to maximize will depend on the attributes of the group within which the decision process takes place. Equally plausible is the notion that these values will be affected by the kind of conflict being debated. We also expect the broader context within which a conflict takes place to have an impact on these values. Finally, since we have stressed that we expect these values to change under certain conditions once the discussion unfolds, we assume that these values will be affected by the structure of the decision process.

A wide range of variables was employed for the four sets (see Table 2). For each of the variables we formulated a detailed hypothesis relating change in the variable to changes in the values and ultimately to changes in the modes of decision making. Each of these hypotheses was formulated prior to the completion of any data analysis; hence the nature of the hypothesized predictions is entirely a priori. Rather than presenting each of the independent variables and the related hypotheses, we shall illustrate our general procedure by way of a single example.

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[3] A description and analysis of these values may be found in Dorff (1978).

For our example we have chosen the variable, "size of the group." First, we concentrate on the way in which the four values may be influenced by the size of the decision group. Perhaps the most obvious impact can be seen for the opportunity costs of lost time. In a large group there are more potential speakers, which raises the prospect of a lengthy discussion. At the same time the typical group member has relatively few opportunities to intervene in the discussion, and much time will be spent simply listening to other members. This mostly passive role, particularly when extended over a lengthy meeting, may contribute increasingly to the feeling that time could be better invested for other purposes. Thus the incentive grows to reduce the opportunity costs represented by this loss of valuable time. In a small group, on the other hand, the discussion tends to be shorter and everyone can participate more fully. Consequently, the decision makers generally do not perceive the time spent in the decision process as a significant opportunity cost.

In addition to its effect on considerations of time, the size of a group probably influences the importance of group solidarity as an incentive as well. Solidarity is expected to be more important in small groups because the low number of participants will lead to a high number of interpersonal interactions and, according to George Homans' well-known hypothesis, frequent interactions should be a favorable condition for solidarity (Homans, 1961). Another argument, based on Olson's work, states that in a small group each actor can contribute in a more perceptible way to maintaining the group, whereas maintaining a large group depends much less clearly on the actions and inactions of individual group members (1965, p. 65). Thus in a small group the motivation of a rational actor to work for group solidarity should be much greater than in a large group.

We anticipate that considerations of power will have a greater impact in large groups than in small groups. In part, this is due to the effect of considerations of solidarity as noted in the preceding paragraph. In addition, the larger the number of people attending a meeting, the more that meeting takes on the character of a spectacle; the more the participants feel as though they are "performing" in this spectacle, the more power considerations will be involved. Winning or losing has a much more dramatic effect in a large group than in a small group, and scoring a point in the closeness and informality of a small group is much less spectacular than scoring the same point in the limelight of a large meeting.

We can find no logical reasons why the size of the group would have an influence on considerations of rectitude.

The causal linkage between the size of the group, the values, and the modes of decision making can now be made. The pressure for solidarity in a small group can be best met with a decision by

Table 2. Independent Variables

Structure of Decision Group

1. Size
2. Predominance of low-status participants
3. Frequency of interactions among the members
   - –Frequency of meetings
   - –Number of governmental functions of the members
4. Age of the members
   - –Biological age
   - –Political age
5. Homogeneity
   - –By language
   - –By sex
   - –By religion
   - –By education
   - –By occupation
   - –By rural background
   - –By place of residence*

Substance of Conflict

6. Valence nature
7. Reference group
8. Innovativeness
9. Personnel versus policy conflict
10. Perceived importance of conflict
    - –Participation rate at the meeting
    - –Length of the discussion

Context of Conflict

11. Time distance to parliamentary elections
12. Unlikelihood of referendum
13. Pre-parliamentary versus parliamentary stage
14. Party control of government

Decision Process

15. Formality of discussion
16. Focus of discussion
    - –Number of agenda points in meeting
    - –Number of issues within the agenda point under discussion
    - –Number of decisions in meeting
17. Timing of the discussion
18. Certainty of information
19. Number of proposals
20. Origin of proposals
21. Structure of debate
    - –Distribution of opinions
    - –Number of speakers with clearly expressed opinion

Source: Compiled by the authors.

*The directional sign for this variable is reversed from the other homogeneity variables. In this case a high positive value indicates a heterogeneous group; a high negative value indicates a homogeneous group.

amicable agreement. Two potential obstacles to this decision mode are not particularly salient in small groups, namely, time pressure and considerations of power. If amicable agreement cannot be achieved in a small group, the second best choice seems to be *no decision* or *decision by interpretation,* both of which would not disrupt solidarity significantly. A *majority decision* appears to be the least desirable choice since it may be detrimental to solidarity. In large groups, an important consideration is the time factor which can be best served with a *majority decision.* A *majority decision* also contributes to the concerns with power since it allows each participant to stand visibly by an original position. Of course, standing by one's position can result in a loss of power if that position winds up in the minority. However, since group solidarity is relatively unimportant in large groups, the majority will be largely unconcerned with what happens to the minority, so concessions will be unlikely. *Amicable agreement* should be quite infrequent in large groups since achieving such agreement is both time-consuming and premised on the salience of group solidarity.

After formulating all of our hypotheses in this manner, we proceeded to the empirical analyses.

Statistical Methods of Analysis

As a response to the categorical nature of our dependent variable, we first employed discriminant analysis. The technical nature and theoretical underpinnings of this technique have been discussed elsewhere (Dorff, 1978). For the moment we shall point out that this technique is designed to find a particular linear combination of independent variables that will maximally discriminate between two or more groups of observations.[3] That combination (or combinations) is then used to classify the individual observations in one of the categories of the dependent variable. The more clearly the categories are distinguished by the p-dimensional vector(s) comprised of the p independent variables, the more successful or accurate will be the classification procedure. The vectors provide some indication of the relative strengths (contributions) of the independent variables in distinguishing among the categories, while the classification scheme indicates the extent to which the combination(s) accurately differentiate(s) among the groups. It is in this very general sense that comparisons are often drawn between the vector of b's and the R^2 in regression analysis and the discriminant functions and the successful classification rate in discriminant analysis.

A second, major component of our analysis was a simulation in which we attempted to classify each conflict according to the decision mode most likely to be employed under the conditions specified by the independent variables. Rather than reducing the scope and precision of our predictions by choosing more than one possible outcome, we have made single-point predictions; in other words, if two decision modes are considered likely to occur but one seems more likely than the other, we have chosen the most likely decision mode as our single prediction. It should be clear, however, that this single prediction does not reflect the fact that our theory postulates a non-zero probability for the occurrence of other decision modes (although not in all cases). Making this single prediction will mean that our overall success rate wil be considerably lower than if we made more than one prediction.

Empirical Tests

Discriminant Analysis. In Tables 3 and 4 we have reproduced the results of the discriminant analysis. Three components of these results deserve special treatment: (1) the discriminant functions and the variable loadings, (2) the location of the group centroids in the reduced space, and (3) the classification table.

1. *Discriminant Functions.* Discriminant functions are similar to factors in a factor analysis in that the loadings of the individual variables on the function provide some indication of the relative contribution that an independent variable makes to the discrimination among the groups; simply stated, higher loadings on the discriminant function imply greater significance of those variables. The ten variables contained in Table 3 are drawn from a pool of 21 independent variables originally employed in the discriminant analysis (see Table 2).[4]

2. *Centroids of Groups in Reduced Space.* These figures indicate the locations of the four group means in the reduced three-dimensional space defined by the three discriminant functions.

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[3] Technically, this amounts to solving the basic characteristic equation problem:

$$(W^{-1}B - \lambda I)d = 0 \text{ when } W \neq 0.$$

[4] This reduced list of independent variables was derived from the original 21 variables through the use of a preliminary set of bivariate analyses. The criteria for selection or exclusion included both the apparent strength of the relationship and the degree to which the nature of the relationship corresponded to our original hypotheses. For a detailed discussion of each of these variables and the bivariate analyses, see Dorff (1978), Chs. 5-8.

Table 3. Discriminant Analysis Results

Variable	Standardized Discriminant Function Coefficients Function 1	Function 2	Function 3
Structure of Decision Group			
1. Size	.08	.09	–.48*
2. Predominance of low-status participants	–.55*	.40*	.48*
3. Frequency of interactions among members			
–Frequency of meetings	.02	–.39*	–.13
–Number of governmental functions of the members	–.66*	.54*	.49*
4. Homogeneity			
–By rural background	–.09	–.11	.42*
–By place of residence**	–.40*	.07	.11
Substance of Conflict			
5. Valence nature of conflict	.58*	.03	.19
Context of Conflict			
6. Time distance to parliamentary elections	–.10	–.32	.53*
7. Unlikelihood of referendum	–.05	.34*	–.46*
Decision Process			
8. Formality of discussion	.26	–.49*	–.18
9. Focus of discussion			
–Number of agenda points in meeting	.20	–.24	.43*
10. Number of proposals	.09	–.19	–.36*
	Centroids of Groups in Reduced Space Function 1	Function 2	Function 3
Majority Decision (MD)	–.33	–1.31	–.12
Amicable Agreement (AA)	.54	– .04	.60
No Decision (ND)	–.63	.28	.14
Decision by Interpretation (DI)	.29	.18	–.41

Source: Developed by the authors from data in Robert H. Dorff, "Employing Simulation Analysis in Decision-Making Theory," Ph.D. Dissertation, University of North Carolina, Chapel Hill, 1978.

*Important variables are at least half as large as the absolute value of the largest coefficient.

**The directional sign for this variable is reversed from the other homogeneity variables. In this case a high positive value indicates a heterogeneous group; a high negative value indicates a homogeneous group.

The greater the relative distance between the means, the more the groups are differentiated along that dimension. When we combine the directional sign (+ or —) for the group mean with the directional sign for the independent variables on the discriminant function, we have an indication not only of the strength or contribution of the variables but also of the direction of the impact on the likelihood of each of the four decision modes. In order to assess this impact, we must examine the three functions in order to determine which groups are being most clearly discriminated.

A glance at the centroids for function 1 reveals that it discriminates most clearly between ND (—.63) and AA (.54), with a secondary discrimination between MD (—.33) and DI (.29). Table 3 reveals further that an increase in the predominance of low-status actors pushes the discriminant function in a negative direction. Consequently, as more low-status participants are involved, the function takes on an increasingly negative value and ND and MD become much more likely to occur while AA and DI become less likely. In the initial formulation of our hypotheses we had anticipated just such a result: low-status participants, being less concerned with group solidarity (AA) and too inexperienced to be able to manipulate the decision-making process (DI), are likely to vote often and to postpone decisions. When we look at the discriminant function as a whole and try to characterize it according to the major variables contributing to it, we find that the function takes on its highest positive value (thus favoring AA and DI) under the following conditions: (1) *group structure* characterized by a predominance of high-status actors who have shared a place of residence (Berne) and who have not accumulated numerous governmental functions, and (2) the *substance* of the conflict characterized as a valence issue. The finding concerning the number of governmental functions runs counter to our expectations. We hypothesized that as the number of these functions increased, the likelihood of interaction among group members outside of this group's meetings would increase. In so doing, this would lead to greater concerns with solidarity. The other findings are in accordance with our expectations.

The second discriminant function distinguishes almost exclusively between MD (—1.31) and the three other modes of decision making. In this case five variables make the most significant contribution to the discriminant function. The relative explanatory power of the second and third functions is less than the first since, much as in a factor analysis, they pick up only that information not contained in the first function. When we look once again at the entire function and attempt to characterize it according to the five variables, we find that the function takes on its highest negative value (i.e., favoring MD) when: (1) *group structure* is characterized by frequent meetings, a predominance of high status actors, and a low group average for governmental functions, (2) the *context* is one in which a referendum is certain, and (3) the *decision process* is formal. Under these conditions, after having "controlled" for the information contained in the first discriminant function, the results indicate that MD would be the most likely outcome.

The primary contribution of the third discriminant function is the discrimination between AA (.60) and DI (—.41), a feature that completes the discrimination process by distinguishing between the two decision modes that previously remained relatively close together in the multidimensional space defined by the independent variables. The fact that eight variables make significant contributions to this function is a further indication of the decreasing importance of the functions; in other words, the function is expectedly more diffuse. Nevertheless, we can still attempt to characterize this function generally according to these variables. Such an attempt reveals that the function would take on its highest positive value when the following conditions exist: (1) *group structure* characterized as a small group, dominated by low status actors, primarily homogeneous with regard to rural residence, and with a high average number of governmental functions, (2) a *context* consisting of impending or recently held elections and a high possibility of a referendum on the issue, and (3) a *decision process* that is relatively unfocused (many agenda points) with only a few proposals on the isue. In such situations it is likely that AA would be employed and unlikely that DI would result. With the notable exception of the status variable, these results bear a close correspondence to the hypotheses that we formulated prior to the analysis.

3. *Classification Table.* This is the most important component of the discriminant analysis. In Table 4 we have reproduced the results of the classification procedure based on the preceding discriminant functions. For each observed decision an overall score on each of the discriminant functions is calculated. The three scores for each observation are then compared in order to predict the value of the dependent variable on the basis of the observed values of the independent variables. The classification table then indicates both the predicted and actual outcome for the observations, as well as an overall measure of percent correctly classified. The elements on the main diagonal of Table 4 are the numbers and percentages of cases correctly classified for each of the four groups.

In the present context we shall focus only on the total number of cases correctly classified rather than on the individual groups. Because of missing values for 30 of our 466 decisions, 436 cases were available for classification. Of these 242 or 55.5 percent were correctly classified according to the single mode of decision making that was employed. The question now concerns the establishment of a suitable baseline or chance model against which to compare this result in order to determine how well our model fared. In other words, what rate of successful classification might we expect if we had no theoretical model, and how does our model then perform in terms of predictive capability?

The baseline that we have chosen is derived from LaPlace's Rule of Insufficient Reason, whereby one simply guesses each category the same number of times as every other category. Our primary reason for selecting this baseline is the fact that it incorporates no prior knowledge of the marginal distribution of the dependent variable, as do other possible baselines.[5] Since prior probabilities were set equal (i.e., .25) in the discriminant analysis, these predictions were not based on prior knowledge of the actual distribution of the decision modes. Similarly, the predictions in our simulation are a priori and not based on the marginal distribution of the four decision modes. Consequently, the expected proportion of correct predictions that we employ as our baseline is 25 percent.[6]

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[5] For example, we might guess each category only as often as its observed frequency, leading to a baseline figure of 28.4 percent [(.124) (.124) + (.206) (.206) + (.305) (.305) + (.365) (.365)]. Alternatively, we could simply guess the modal category (decision by interpretation) each time and achieve a 36.5 percent successful classification rate. The use of such information concerning the marginal distribution of the dependent variable seems inappropriate for evaluating predictions made a priori, as suggested by Hildebrand et al. (1976).

Having selected an appropriate baseline figure, we must now devise some means of evaluating our model's performance as compared to the chance model. Such a procedure can be derived from the general model for defining a proportionate reduction in error measure (PRE) (Hildebrand et al., 1976, 1977). By modifying the PRE measures slightly, we calculate the difference between the actual percentage of correct predictions obtained from the analysis and the number of correct predictions that one would expect from chance alone, and then divide this difference by the number of correct predictions expected from chance.[7] This calculation then provides us with a measure of the proportional increase in accuracy attributable to our model.

Taking the 55.5 percent successful classification rate, and employing 25 percent as the appropriate baseline, our proportional increase in accuracy measure is calculated:

$$\frac{55.5 - 25}{25} = \frac{30.5}{25} = 1.22.$$

According to this measure, we are able to improve our classification by 122 percent over what we would expect from chance alone.[8] This increase represents a substantial improvement in our ability to classify the original observations according to the mode of decision making actually employed.

A closer examination of the important variables in the three discriminant functions reveals that group structure is a key determinant of the difference among decision modes; in all three functions variables reflecting characteristics of group structure are the most numerous. But the substance and context of the conflict, as well as the decision process, also contribute to the variation in likelihood of the decision modes. This conforms to our general model of decision making discussed earlier: group structure is the first variable that enters our flow process, and hence may be considered the most important simply because it sets the stage for the actual decision making that follows; this structure is then mediated by the type of issue being considered, the context within which the conflict takes place, and the ensuing decision process. Although discriminant analysis cannot deal ade-

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[6] (.25) (.124) + (.25) (.206) + (.25) (.305) + (.25) (.365) = .25 or 25 percent.

[7] A similar and perhaps more familiar measure is based on Goodman's and Kruskal's Lambda and is discussed in Weisberg (1978, p. 562). See also Hildebrand et al. (1976, pp. 519-21).

[8] In addition to classifying the entire data set we also performed a split half-sample analysis. This standard cross-validation procedure, conducted for both the pooled and unpooled versions of the discriminant analysis, indicated that the overall classification results are perhaps somewhat overstated as a consequence of being maximized to this specific data set. However, since the pooled version results varied only between 42 percent and 63 percent, our estimate of 55.5 percent seems to be quite reasonable. For a discussion of this cross-validation procedure see Dorff (1978, pp. 139-41).

Table 4. Classification Results

		Predicted Outcome			
Actual Outcome	Total No. of Cases	Majority Decision	Amicable Agreement	No Decision	Decision by Interpretation
Majority Decision	43	30 *69.8%*	4 9.3%	4 9.3%	5 11.6%
Amicable Agreement	93	12 12.9%	47 *50.5%*	16 17.2%	18 19.4%
No Decision	134	16 11.9%	17 12.7%	76 *56.7%*	25 18.7%
Decision by Interpretation	166	22 13.3%	28 16.9%	27 16.3%	89 *53.6%*

Correctly Classified = 55.5%

Source: Developed by the authors from data in Robert H. Dorff, "Employing Simulation Analysis in Decision-Making Theory," Ph.D. Dissertation, University of North Carolina, Chapel Hill, 1978.

Note: The italicized percentages on the main diagonal represent the percent of cases correctly classified into each of the four categories.

quately with the flow of this process, it does appear to highlight the relative temporal importance of each set of variables. In the next step we shall try to incorporate this flow process more systematically in our analysis through the use of simulation.

Simulation Analysis. In addition to discriminant analysis, we also constructed a simulation for analyzing our data. The purpose of this simulation was to create, a priori, a classification procedure that would predict the decision mode on the basis of a reduced set of independent variables drawn from our original pool of variables. The primary strength of simulation analysis lies in its ability to handle the "flow process" nature of our model. In other words, the dynamic branching effects that we believe most accurately characterize this type of decision-making process will be replicated within the context of our simulation. Once again, we can evaluate the model's performance on the basis of the total number of cases correctly classified. We remind the reader of our earlier discussion of the single-point nature of these predictions and the implications for assessing the overall accuracy of the predictive model.

On the basis of the discriminant analysis and general correspondence with our original theoretical propositions we selected seven independent variables for our simulation.[9] The variables and the sets from which they are drawn are as follows:

I. Group Structure
  1. Size of the Group
  2. Political Status of the Group Members
II. Substance of the Conflict
  3. Position versus Valence Issues
  4. Reference Group of the Conflict
III. Context of the Conflict
  5. Time before Parliamentary Elections
  6. Likelihood of a Referendum on the Issue
IV. Decision Process
  7. Origin and Number of the Proposals

For the sake of clarity we have briefly stated our hypotheses in Table 5.

The first variable that enters the process is *group structure*. From the moment a decision group convenes, it takes on relatively fixed structural characteristics. In particular, each group will be either small, medium, or large and will consist solely or predominantly of high-status or low-status actors. From our theory and the individual hypotheses contained in Table 5 (Hypotheses 1 and 2), we would expect that group structure alone would not suffice for the prediction of a single decision mode. Our theory presumes that certain tendencies should be evident under specific structural conditions but that an actual outcome will remain largely indeterminate until further information becomes available. In other words, while certain structural combinations will clearly favor one or two decision modes over the others, group structure alone should rarely (if ever) be sufficient for determining a single decision mode. Therefore, our simulation would make note of the specific characteristics of the group structure but would pass directly on to the next variable (substance) rather than taking the path to the predictions.

Once the meeting is convened, the group is confronted by an agenda of issues. In each decision-making situation, then, the actual *substance of the conflict* begins to interact with the characteristics of the group structure. In our model we distinguish between conflicts dealing with actual goals (position) and conflicts dealing with means (valence). Further, we consider the group to which a conflict refers an important determinant of the way in which the decision-making process unfolds and, ultimately, of the decision mode actually employed. However, we also expect that the criterion of position-valence will be somewhat more important than the reference group, although the interaction between the two remains crucial. Our assumption follows from the argument that the position-valence nature of a conflict will be the primary characteristic of the substance; political decision makers will consider this factor first, and only secondarily will they consider the reference group to which a particular issue refers.

In general, we also assume that the combined information regarding structure and substance will not suffice for specific predictions. Our reasoning here is that the group structure and substance of a conflict, while of primary importance in determining the resulting decision mode, will not always lead in the same direction; hence some indeterminacy must remain. Moreover, most combinations of these two sets of variables will serve only to narrow the range of possible decision modes (for example, from four to three to two) rather than to restrict that range to a single mode which we could then predict. The exception to this indeterminacy occurs for small groups comprised solely of high-status actors or solely of low-status actors.

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[9] Once again the basis for reducing the number of independent variables consists of a combination of the results of empirical tests (bivariate and discriminant analyses) and the degree to which the suggested relationship corresponded logically to our original hypotheses. If, for example, a variable displayed some discriminating power but the direction was difficult to justify in terms of our theoretical framework, we chose to exclude that variable from the simulation. The selection of these variables is discussed in detail in Dorff (1978, pp. 285-95).

The *context of the conflict* is the third factor to affect the flow of the decision-making process. The two variables that we employ are the time interval to the parliamentary elections and the likelihood of a referendum. One might argue that the time before the elections should enter the process earlier than we have stated; however, the logic of our theory indicates that concern with elections will become salient only if the particular issue being considered is somehow relevant to the upcoming elections. Concerns with electoral impact should normally become salient only after the substance of a conflict is apparent and the preliminary discussion has taken place.

The final variable concerns the *decision process*. Once a meeting is convened, an issue determined, and the impact of contextual variables ascertained, the actual way in which the discussion proceeds will help determine which values will change and which will remain fixed. As we have pointed out earlier, we do not assume that the values for every actor will remain invariant; in

Flow Chart of Decision-Making Process

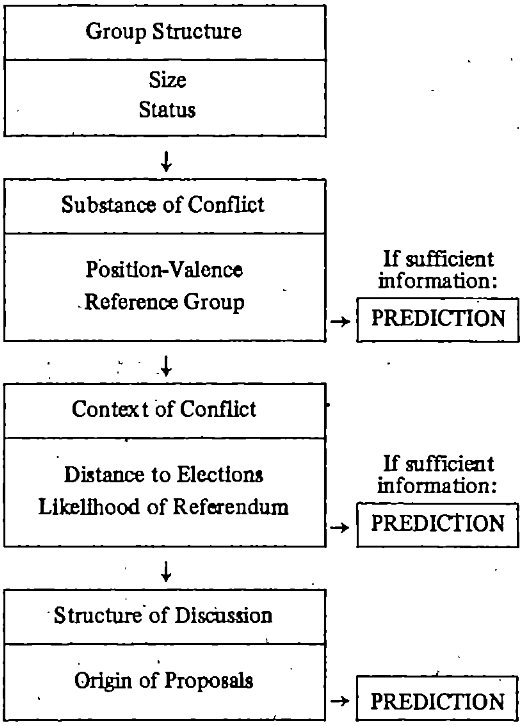

Table 5. Hypotheses for Independent Variables in Simulation

	... the more the probability of ...			
Independent Variables	majority decision	amicable agreement	no decision	decision by interpretation
1. The smaller the *size* of the decision group ...	decreases	increases	is unaffected	is unaffected
2. The larger the portion of high-*status* actors in a decision group ...	decreases	increases	is unaffected	is unaffected
3. The more a conflict deals not only with means but also with *ends* ...	increases	decreases	increases	decreases
4. The more a conflict *refers* to matters of the decision group itself ...	is unaffected	increases	decreases	increases
5. The closer the time distance to the next parliamentary *election* ...	increases	increases	decreases	decreases
6. The greater the likelihood of a *referendum* about the issue under consideration ...	increases	is unaffected	decreases	decreases
7. The more the high-status actors in the group participate in the *articulation* of proposals ...	decreases	increases	is unaffected	increases

Source: Compiled by the authors.

fact, we make the explicit assumption that these values change from actor to actor and, for each actor, from situation to situation. We anticipate that the origin of the proposals (whether it originates with a high- or low-status participant) will be a major factor in effecting change in these values and, hence, in the decision mode that is employed.

Limitations of space simply do not allow us to provide an extensive treatment of our simulation model. Rather, we shall provide a very simple illustration of the model's performance.

For our illustrative example we have chosen one of the simplest cases because it can be described briefly. In this case only three variables are needed before we can make a prediction; the reason for this relative paucity of information lies in the fact that all three variables point to the occurrence of a single decision mode. Therefore, the combined interaction of the three variables should make this single decision mode occur quite often.

The three characteristics associated with this class of decisions are: a small group (seven or less) comprised solely of high-status actors who are discussing a valence issue. A quick glance at Table 5 reveals that our hypotheses anticipate that all three conditions strongly favor *amicable agreement,* while both *no decision* and *decision by interpretation* seem largely unaffected. All three variables indicate that a *majority decision* is highly unlikely under these conditions. Therefore, we make the following predictions: *amicable agreement* is the most likely outcome and is our single-point prediction; *no decision* and *decisions by interpretation* occupy an intermediate position in which they are less likely to occur than *amicable agreement* but not entirely unlikely; *majority decisions* are predicted not to occur.

Our simulation, then, makes the preceding predictions and then compares them with the actual outcomes of decisions made under this particular set of circumstances. As it turns out, ten of the observed decisions were made under these conditions. Of these, six were made by amicable agreement, two by no decision, and two by decision by interpretation. For this set of decisions we have therefore achieved a successful classification rate of 60 percent. Again, however, we stress the fact that this figure does not reflect the accuracy with which we predicted the *non*-occurrence of *majority decisions* nor the seemingly correct predictions concerning the intermediate likelihoods of both *no decision* and *decision by interpretation.* While most of our predictions were considerably more difficult to make than these, and of course some were considerably less accurate, this example should illustrate the nature of the prediction process and how the results were obtained.

The entire classification process covered 436 cases. From our universe of 466 decisions we had to omit 30 cases that had missing values on some of the seven independent variables included in the simulation. For 25 of the remaining 436 decisions our prediction was *indeterminate,* leaving us with 411 cases for which we have actually made a single-outcome prediction. In 237 cases (57.7 percent) our predictions were correct. Employing our previous standard of evaluation we find that this figure represents a substantial improvement in our ability to classify each decision according to the actual mode of decision making that was employed. If we adopt a baseline of 25 percent, which presumes that we have no a priori knowledge of the distribution of the four modes, our 57.7 percent correct classification rate constitutes a 131 percent proportional increase in accuracy in our ability to predict the modes of decision making.

Conclusion

The results of the analysis indicate that our theoretical framework makes a significant contribution to an understanding of these group decision-making processes. The relative accuracy and compatibility of the classification results demonstrate that a substantial amount of the variation in the modes of decision making can be explained by the independent variables included in our model. But what contribution does this study make to a more general theory of decision making in face-to-face groups?

First, an examination of our independent variables would show that almost all of them are of a highly general nature. With the exception of the referendum variable, all could be applied to committee decision-making processes in a wide variety of national contexts, and the hypotheses we formulated were not simply case-specific.

Second, our dependent variable is equally broad in its range of potential applications. During the spring and summer of 1979, Robert Dorff conducted a series of 12 decision-making experiments employing graduate and undergraduate students. The groups consisted of either five or ten students. Preferences were induced monetarily and participants received a modicum of instructions—they were told only that they had to choose one of five decision outcomes on each of an indefinite series of decisions. No instructions were given concerning the decision mechanism. Yet in these experimental groups we observed a significant use of *majority decision, amicable agreement,* and even *decision by interpretation; no decision* was of course not allowed. Moreover, the experimental control for group size also resulted in clear support for our hypothesis: the larger groups voted much more often on precisely the same items as small groups, which were much more inclined to employ amicable agreement. These experiments clearly indicate that a theoretical focus on the modes of decision making has a broad applicability and could be useful in any political context where decisions are made in face-to-face groups. The precise refinement of our theory to deal with this more general class of behavior is the goal of our current research efforts.

Third, the contribution of our theoretical framework is not limited solely to its general applicability but includes the fact that it provides a more tractable and meaningful way of studying real-world committee decision-making behavior. Because we were able to gather almost all of our data through participant observation without grossly disturbing the decision-making groups, our methodology should open the door to more frequent and in-depth analysis of such groups. In this way, perhaps, some of the gap between the study of experimental and real-world groups might be narrowed.

In further work we have also examined some of the consequences of the modes of decision making. In particular, we have studied the effects of the decision modes on (1) the willingness of political actors to participate in meetings, (2) the level of antagonism and hatred in the group, and (3) the potential innovativeness of group decisions. Finally, since so much political decision making takes place in face-to-face groups, it is important to see what kind of group decision-making behavior we consider more or less democratic. After all, process as well as outcome is important to an understanding of political decision making. A focus on the modes of decision making should allow us to deal with these issues as well.

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Congressional Staff Turnover and the Ties-That-Bind

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Individual members of the U.S. Congress work with staffs ranging in size from a minimum of 18 to a maximum of well over 100. Each member has come to preside over a personnel system and, consequently, may best be understood as an enterprise *manager. In this article we examine these congressional enterprises in order to gain some insight about the autonomy of individual staffers, the stability of these member-centered enterprises, and generally, the responsiveness of staffers to member objectives. While it is difficult to observe or measure responsiveness directly, some purchase on the question is gained through an investigation of the causes and consequences of staff turnover; this is the major empirical focus of the research reported here.*

Government authority, however much constrained by law or custom, is exercised by people. Analysis of that exercise must concern itself with who those people are, how they came to positions of power and influence, and how long they last. In this article we focus on the rates and patterns of turnover among congressional staff. We do so in an effort to assess the consequences for power and accountability in the legislative process resulting from the enormous growth in size of congressional staff (Fox and Hammond, 1977; Malbin, 1976, 1980; Machowsky, 1978; Brady, 1979; Commission on Administrative Review, 1977). Our basic concern is to establish whether staffers are autonomous political actors and, if so, to what extent they operate outside the control of their nominal superiors, or, alternatively, are bound reasonably closely to the elected members. Turnover is not the only indicator by which to gain insight on this issue, of course, but it is highly useful, both for the immediate purpose and more generally as one way to characterize a political order.

At one level, the comings and goings of public officials are indistinguishable from petty gossip. On other occasions, as Michels, Mosca and Pareto have pointed out, they can comprise the foundation of a wide-ranging theoretical argument. The circulation of elites—whether by election or appointment—has often been taken as a hallmark of responsiveness to shifting majorities or changing priorities. The price for this sort of responsiveness, however, is often a reduction in experienced-based expertise, a consequent decline in effectiveness, and the unhappy prospect of neophytes left in charge of complex and vital matters. A Jeffersonian revolution every 20 years may well be a purifying nostrum for the body politic, but it may also debilitate the effective prosecution of government affairs.

Turnover, then, is a generic property of personnel systems, broadly construed, with systemic responsiveness and performance often serving as both cause and effect. Despite the seriousness and pervasiveness of the matter, however, we have few comprehensive examinations of the phenomenon of turnover, and consequently it is not easy to determine whether any particular magnitude we might observe constitutes much or little.[1] In turn, therefore, we are at something of a loss to know what to make of the numbers. For instance, there have been five presidents between 1960 and 1980. Only one previous 20-year segment of American political experience, the uncertain antebellum years of 1840-1860, witnessed more frequent turnover in that office. Does that matter? Twentieth-century Supreme Court justices have a mean tenure of just over 14 years. Is that high or low in relation to our normative concerns?

Stanley et al. (1967), and more recently Heclo (1977), have calculated turnover rates for executive branch personnel. Cabinet secretaries last some 40 months on average (except for Nixon's appointees who managed only about 18 months), but the usual figure given for under- and assistant secretaries is only 22 months. There is, moreover, great variation from agency to agency. Commerce Department officials have brief tenure, presumably because they tend to hasten back to the busi-

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The authors thank the John M. Olin Foundation and the Center for the Study of American Business at Washington University for financial support of this research, and gratefully acknowledge the research assistance of Roberta Quigley and Frederick Waldstein. An earlier version of this paper, entitled "Congressional Staff Turnover: Its Causes and Consequences," was presented at the annual meeting of the Southern Political Science Association, Gatlinburg, Tennessee, 1979.

[1] Among the exceptions to this general assertion are Schlesinger (1966), Hyneman (1938), Prewitt (1970).

ness world. Agriculture and Labor officials would seem to have fewer alternatives; in any event, they remain in office considerably longer. Heclo makes a major point of the fact that the senior political appointees are veritable birds of passage while the top career officials and the "non-career" supergrades have far more longevity and, presumably, more understanding of how Washington games are played.

On the congressional side, much has been written in recent years about turnover, or the lack of it, among members of Congress (Fiorina, 1977a, 1977b; Ferejohn, 1977; Mayhew, 1974; Burnham, 1975). For a time it appeared that incumbent senators were successfully insulating themselves from electoral challenge, though that trend seems now to have reversed (especially in light of the 1980 election). House members continue to enjoy relative security, and the "case of the vanishing marginals" has occasioned both analytic and normative attention.[2] Even while electoral defeat seldom strikes incumbent representatives, however, voluntary retirements of various kinds have left the House with a noticeably less experienced membership than it has had for several decades (Bibby, Mann, and Ornstein, 1980, Tables 1-7 and 3-7; *Congressional Quarterly Weekly Report,* January 12, 1980, 79-82). The consequent weakening of traditional norms and the fading of institutional memory generate severe problems in the legislative process so that, even with high incumbent security, turnover is great enough to undermine what Polsby (1968) and others thought was a thoroughly professionalized institution.

The amount and significance of personnel turnover obviously varies across time and among institutional settings, and it is essential therefore to establish what context provides the relevant setting if the analysis of turnover is to be illuminating. In the congressional case, we would suggest that a focus on the individual members is not completely satisfactory. Contemporary members operate in multiple organizational settings: personal offices, committees and subcommittees, leadership structures, party groups, state delegations, and single-purpose caucuses, to name the more salient examples. A recent organizational survey counted, as of 1978, 990 congressional subunits possessing staff complements either from a direct appropriation or member contributions.[3] Individual members work with staffs ranging in size from a minimum of 18 to a maximum of well over 100. Each member has come to preside over a personnel *system* of greater or lesser complexity and scope. The member of Congress may best be understood, therefore, as an *enterprise,* and the analysis of phenomena like personnel turnover or elite circulation must be analyzed in terms of the dynamics within those member enterprises.[4]

This study will provide an initial charting of this recent organizational development. Our basic premise, as noted, is that the House (Senate) is no longer best conceived of as a body of 435 (100) individuals, or even as a structurally differentiated (committees, subcommittees) collection of legislators, but rather as a collection of organizations or member-centered enterprises. Legislative politics, consequently, consists of relationships both among and within these enterprises, much as contemporary economic activity consists of relationships among (competition, collusion) and decision making within firms (Williamson, 1975). Through a specific focus on turnover within enterprises, our study provides a partial test of hypotheses concerned with enterprise stability and enterprise responsiveness to its nominal head, the member of Congress.

Staff Turnover: Categories of Explanation

Turnover, though normally counted and measured in the aggregate, is the product of a host of individual choices. Discussions are often couched almost entirely in impersonal terms, treating both the members of Congress and the staff as collectivities and considering primarily the systemic consequences of turnover. We must, however, give appropriate recognition to the fact that we are dealing with people, with individuals who are driven by ambition, who respond to incentives, and who run the gamut of personalities and motivations. In this section, we briefly examine three broad categories of explanation of staff turnover: (1) individual motives and ambitions, (2) relationships with members of Congress, and (3) situational features. These categories and interpretations are derived from extended interviews with nearly 100 House and Senate staffers during 1978-1979.

Individual Motives. Individual motivation in its substantive detail is, of course, unique to each case, but some more generic categories of motivation may be discerned and some illustrations may be offered as to how they operate. First of all, there is the matter of ambition. The proportion of those who join congressional staffs but who aspire to eventual careers outside the Hill community is surely rather large. Some are looking for the credentialing experience, others hope to parlay their contacts and experience into private-sector fame and fortune, while a few hope to run for office themselves. Indeed, it is rather rare for young staffers to look upon their present role as offering a lifetime career. Staff roles in Congress and elsewhere in public life are generally too loosely defined, compared—say—to teaching or medicine, to permit outsiders to contemplate spending their working lives there. While some of this ambition may be directed toward advancement within the staff system, the most characteristic form of ambition among staff is external, oriented toward career success outside the staff structure, and this provides an important explanation for high turnover.

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[2] For a different and more somber view of the prospects of incumbents, see Erikson (1976).

[3] The details are found in Salisbury and Shepsle (1979).

[4] For a related point of view with respect to the House of Representatives, see Loomis (1979).

If ambition is a driving force leading to turnover, the tangible benefits of staff employment work in the opposite direction. The pay is good and, for the younger staff especially, it is often far higher than alternative employment would bring. Retirement benefits are excellent, and the superiority of the congressional retirement system quite often attracts executive agency personnel and holds them for extended periods of service. The pace of life for committee staffers is somewhat less frantic than that on personal staffs, and job security, while never assured, has seldom been severely challenged in recent years for most staffers most of the time.

Nevertheless, staff positions of all kinds are inherently frustrating. They preclude, by definition, personally labeled accomplishment. They require, if not entire anonymity, at least a willingness to subsist on glory reflected from the principal. Only a few insiders will know that an eloquent speech, a crucial amendment, or a clever maneuver was actually the hard work of a staffer, not the member of Congress whose name gave it both identity and political significance. Clearly, people differ in their willingness to pursue careers of being "on tap, not on top."[5] The norms of professionalism may mitigate the frustrations of anonymity and hold a staffer in a position for an extended time. So may whatever job security there is, for with security the congressional staffer comes to resemble any other bureaucrat. With the lack of acknowledged accomplishment there is a commensurate lack of responsibility, so that the staffer does not routinely have to make the tough policy decision or implement the risky political strategy. For the temperamentally risk-averse, the staff role may well be rather comfortable.

If we were to conjecture about how individual motivations combine with experiential factors to encourage a staffer to leave or remain on a particular staff, we would anticipate, in the first few years of employment, a fairly high tolerance for the frustration, the heavy and unpredictable workload, the limited rewards (other than pecuniary ones), and relatively unpleasant working conditions. The most common initial motivation for work on the Hill possesses all the characteristics of an investment; it is a stepping stone from which to launch a career elsewhere. The likelihood that a staff member will leave increases with each passing year. There is, therefore, a fairly steady egress of people who have obtained—say—two to four years of credentialing experience. The probability that a staff employee will not remain another year hits a maximum after about four years, and thereafter begins to decline as responsibilities and rewards increase and the employee recognizes the attractions of serving 20 years in order to cash in on a generous retirement (often at a fairly young age). If, in fact, a staffer's turnover likelihood is curvilinear in his or her Hill experience, all other things equal, then an interesting connection between staff turnover and growth is implied. Increases in neophyte staffers, which come with expansion of the staff system, increase the pool of most probable "turnovers" so that growth and turnover go hand in hand, though possibly with some lag. We examine the evidence on this implication below.

Relationships with Members. Much is often made of the distinction between personal and committee staff.[6] Certainly there are differences in rules and regulations pertaining to these different categories. Yet too much is sometimes made of this nominal distinction. The fact that individual staffers are on different payrolls camouflages a central feature of the employment relation that holds for the wide majority of congressional staffers namely, that they are *member-specific resources*. While this is obvious in the case of personal staff, it is less apparent but nonetheless true for most committee and subcommittee staff, too. Staff, whether personal or committee, are hired to serve the policy, electoral, and even ideological objectives of the members who lead the various congressional enterprises—personal offices, committees, subcommittees, etc. (Kampelman, 1954). Occasionally, the authority to employ staff is delegated, as when a member gives an Administrative

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[5] This classic phrase was used to characterize the personnel who were to assist the president in staff roles in the 1937 Brownlow Commission's Report on Administrative Management. See Karl (1963).

[6] The standard reference is Kofmehl (1963). For a lucid overview, see Patterson (1970).

Assistant (AA) responsibility for hiring or a chairperson allows other committee members to nominate committee employees. But in nearly every case, a staff employee is associated with a member-patron. In recent years, new rules in each chamber (S. Res. 60 and H. Res. 5) have made de jure what had been a fairly widespread practice of converting nominally collective staff resources into member-specific resources. These rules gave individual members (in the Senate) and subcommittee chairpersons (in the House) access to committee funds to hire staff who, though nominally employed by the whole committee, are in fact under the instruction and direction of individual members.

Thus, although there are exceptions, the overwhelming majority of individual staffers in every nominal category are tied to a principal or patron. Consequently, while some employment conditions are shared by all staffers, many depend rather on the idiosyncracies of principals. It follows from this that the personal disposition of staffers to stay or leave is heavily dependent on ties that bind them to their principals, the members of Congress. Since the large majority of senior staff are, or feel themselves to be, closely linked to specific members, either because the staff appointments resulted from some quasi-patronage connection or because the intimate working relationship between staffer and member breeds a personal interdependence, such ties may not be readily reestablished when the member in question moves on. To the extent that staff is thus closely tied to the fortunes of individual members, their turnover will likewise follow the movements of members, not only movement out of the Congress but within it from one chamber to another or from one committee or subcommittee to another.

Situational Features. We have suggested that both staffers' ambitions and their attachments to a boss affect the likelihood that their employment relationship will be severed. This decision, however, is bound to involve some assessment of alternative opportunities as well. Obviously, if continued employment is no longer feasible—for example, if the member leaves the Congress or fires the staffer—then there is no decision to be made; turnover must occur. But a good deal of turnover is caused by voluntary, calculated decisions made by staff members. Thus, it is a function of both motives *and* opportunities. That is, ambition and opportunity are complementary. We have already discussed ambition; here we focus on opportunity.

1. *Member turnover:* Member turnover not only provides the occasion for turnover of the staffs of those members who have moved on; it provides new opportunities for other staffers as well. New members of Congress, new committee and subcommittee chairpersons, and new partisan and institutional leaders recruit staff. While some of this recruitment is normally drawn from previous associations, particularly in campaigns,[7] a substantial portion comes from the existing pool of employed staffers. Some of these staffers have lost a boss, as in the case of the AA whose member retires or loses an election and who then shops around for another member to serve.[8] Other staffers are on the lookout to improve their financial position and upgrade their job responsibilities. Member turnover provides these individuals with new employment opportunities.

2. *Staff growth:* While we have sought to distinguish growth and turnover, the former creates pressure for the latter in two distinct ways. First, as noted above, new recruits coming from outside the staff system, often outside the Washington community, are the most likely to depart after a short credentialing stay. Thus, we may expect, with some lag, to observe growth-induced turnover of this type. Second, staff expansion provides expanded opportunities for *existing* staff. Some turnover is thereby directly induced which, in turn, has a lagged "echo" as new staffers are recruited to fill these recently vacated positions—new staffers some of whom, themselves, depart after a brief stay.

3. *Institutional change:* One of the bases of staff growth which, in turn, has turnover effects is change in institutional structure. In particular, both the House and the Senate have witnessed heady doses of structural change in the 1970s—creation and elimination of standing committees, growth in subcommittees, new rules for selecting

[7] Loomis (1979, p. 48) reports an unpublished study of the new members of the 94th Congress that "showed that over two-thirds (68 percent) of administrative assistants and almost half (47 percent) the legislative assistants had worked in their freshman's campaign."

[8] Perhaps the most amusing illustration of "shopping around" involved the staff of Representative Newton Steers (R-Md.) after his election defeat in 1978. The entire Steers staff advertised in Washington area newspapers, offering themselves as a package to new members of Congress. The advertisement read, in part:

> Super staff seeks legislator interested in innovative constituent service, problem-solving legislation to replace super boss hit from behind in the Nov. 7 election. Staff called excellent by area newspaper. Comes complete with legislative assistants, special projects, case and staff assistants, and beautiful black secretary. The complete set—$288,000.

committee and subcommittee chairpersons and members, administrative decentralization of committee business to subcommittees, etc. Many of these changes constitute situational features that produce opportunities or necessities for staff movement.

4. *Executive branch growth:* One of the ambitions staffers often express is the desire to "go downtown." Those who have been closely associated with legislation may seek positions in an executive agency where they will have the authority to implement and administer programs dear to their hearts. For the more senior staffer, an assistant secretaryship for this or that policy area, head of congressional liaison for a federal agency, or service on a regulatory commission is neither an unusual nor an unreasonable ambition. Even in the absence of growth, therefore, normal executive branch turnover will have repercussions for the congressional personnel system. When the executive branch grows, either adding personnel layers to existing agencies or creating new agencies, congressional staff comprise an obvious recruitment pool. A special case of this exogenous force for turnover occurs with partisan changes in the executive branch.

5. *Change in partisan control of the executive branch:* In the inaugural years of 1961, 1969, and 1977, when partisan control of the presidency changed, there was substantial entry into and departure from the executive branch as new personnel joined the Kennedy, Nixon, and Carter administrations, respectively, while Eisenhower, Johnson, and Nixon-Ford appointees, respectively, sought alternative employment. The effect on Capitol Hill personnel of partisan change in the executive branch is twofold: (1) congressional staff are obvious candidates for administration jobs; and (2) executive branch emigrés find on Capitol Hill an obvious market for their substantive and political skills.

6. *Growth in the Washington community:* The Washington commercial property market is booming with law and accounting firms, trade associations, and other interest groups scrambling for office space. Washington recently surpassed New York as headquarters for more national associations than any other city. The "Washington Community," that admixture of public and private sectors whose elements are concerned with the substance and politics of government, is a growing concern. And with that growth come opportunities. Indeed, for many congressional staffers, employment on the Hill is taken in anticipation of a vice-presidency for governmental affairs with some corporation or association located in the "K Street Corridor" downtown. Also on the rise is the number of specialized law firms and lobbying enterprises largely comprised of former congressional staffers.[9] Thus, a growing Washington community produces yet another set of stimuli leading to congressional staff turnover, providing opportunities for and skimming talent from the legislative staff.

In sum, situational factors combine with staff motives and member-staff relationships to account for staff turnover.[10] In the next section, we put some empirical meat on this explanatory frame.

Empirical Results

In the analysis that follows, we examine the continuity of an employment relationship between a staffer and a member-patron and/or between a staffer and an organizational unit. We trace this relationship over time and, in each instance, endeavor to make clear exactly how we counted or measured. Unless otherwise noted, we rely on the *Congressional Staff Directory* listings which are not without their inconsistencies and occasional lapses.[11] The directories, in turn, depend on the members of Congress and the ways in which they choose to identify and label staff members. The vagaries of titles and definitions of responsibility preclude some of the measurements we would have wanted to make. As a consequence, it is important to underscore not only the normal disclaimer owing to measurement imprecision, but also the fact that these data have not been collected with our questions in mind. Nevertheless, a body of data routinely collected in the same format on an annual basis for nearly two decades is not without its advantages for the questions we wish to pursue.

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[9] See "Practicing Law in Washington—An American Growth Industry," *National Journal,* February 4, 1978, pp. 172-79. A recent article in the *Wall Street Journal* (January 15, 1980 p. 1) reports on a study by Boyden Associates, Inc., which found that 20 percent of the top Washington-based executives of big corporations and trade associations were formerly employed on congressional staffs.

[10] One significant situational factor, partisan change in the control of the House or Senate, has not come into play during the time period of our data. With the 1980 election resulting in Republican control of the Senate, staff turnover will be quite large, apart from the effects caused by member turnover, as old majority committee staffs shrink and old minority committee staffs grow. This development will require closer scrutiny.

[11] Let us here register both a confession and a caveat. We rely throughout our analysis on the annual listings of permanent staff compiled by the *Congressional Staff Directory.* We do this because these listings are available since 1962 and because we hope that however much additional variation there may be, the annual rosters will reflect roughly comparable methods of counting. We are fully aware, however, that month-to-month rosters would reveal more variance, and that the highly flexible lists of investigative staff would reveal further elements of personnel use. Individuals are shifted from one payroll to another according to many different needs, especially during the period of reelection campaigns. We hope and believe that it is still reasonable to treat our questions with the data we herewith present, but it must be recognized that our numbers, like those of other studies we have cited, fall well short of the firm empirical baseline measurements we would prefer.

Motivations and Ambitions. To examine questions of individual motivation and ambition properly requires individual-level data of a type we do not have available. There is, however, one implication of our earlier discussion on which aggregate data can be brought to bear. That is the relationship between turnover and growth. If, as we argued above, turnover, conceptualized as the probability of not serving another year on a given staff, is highest among neophyte staffers for whom Hill employment is a credentialing or an employment opportunity-producing experience, then committee staff growth should be expected to induce turnover. That is, growth in year t should, at years t_1, t_2, and so on, have a turnover "echo." Second, growth in any given year should have a contemporaneous effect on turnover by providing new employment opportunities for current staffers.

The data with which to test the two parts of this hypothesis consist of a committee staff turnover time series for each chamber measured by the percentage of committee staff listed in the *Congressional Staff Directory* for year t who are not listed with the same committee in year t_1 and a committee staff growth time series measured for each year (t_1) as the percentage change from the preceding year (t), the latter as reported by the Clerk of the House and the Secretary of the Senate.[12] Thus for each chamber we have two separate time series reflecting, respectively, the aggregate experience that chamber's committees have had regarding turnover and growth.

The contemporaneous correlation of growth with turnover and "echo" effects are given in Table 1. In addition, because after the Watergate hearings the Senate had some sizable reductions in staff size, the three correlations for the Senate are recalculated with 1976 and 1977 data deleted; these are reported parenthetically. Both the contemporaneous and lagged correlations are significantly different from zero for both chambers and the simple regressions of chamber staff turnover against chamber growth, in either the contemporaneous or lagged form, yield significant positive coefficients for the growth variables. Moreover, it would appear that the House and the revised Senate associations are similar in magnitude, with contemporaneous associations substantially larger than the lagged relationships. On the basis of this simple analysis, then, we find support for the hypothesis that growth and turnover are associated. While neither the magnitudes of the regression coefficients nor the amount of variance explained is overwhelming, they are nevertheless statistically significant and positively related as anticipated in our argument. The relationship itself, however, appears to have changed over time. Prior to 1970, committee staff turnover was relatively small and was uncorrelated with committee staff growth (the latter was also relatively small). Since 1970, both turnover and growth have been both large and highly correlated. Statistically, it appears that the relationship between turnover and growth has become quite significant as their respective magnitudes have grown large. Thus, the linear hypothesis (upon which the Pearson r is premised) constitutes a misspecification—the table entries understate the current association and overstate the one in the earlier period. These results, however, are quite compatible with, though a modification of, the qualitative idea that growth alters both current employment opportunities (the contemporaneous effect) and the relative proportions of neophytes and experienced staffers (the lagged effect).[13] Since many other factors affect turnover in addition to growth, we could hardly expect more compelling empirical results.

There is little more we can say at this time about personal motivations and their effect on the turnover decision. There is one other body of data, however, that sheds some light on the ambition hypothesis, viz., that staffers are outward looking, using a few years of Hill employment ultimately to acquire a position off the Hill. For two post-election transitions, 1966-67 and 1976-77, we looked at the personal staff of departing Congress members to determine how many of them left with their bosses and how many remained on the Hill in other capacities. These data are given in Table 2, which gives the proportion of departing members' staffs who were not listed on some other personal or committee staff in either cham-

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[12] The actual time series are not displayed here because of space limitations, but are available from the authors.

[13] Of course, both turnover and growth could be spuriously related through other variables, but fancier statistical models to uncover such effects hardly seem appropriate in light of the crude data we are employing. We acknowledge and thank the anonymous referee of an earlier draft who called our attention to the inconstancy of the relationship over time.

Table 1. Turnover and Growth Hypothesis, 1962-1978

Association of Growth with Turnover	Pearson Correlation	
	House	Senate*
Contemporaneous Correlation	.50	.20 (.61)
One-period Lagged Correlation	.33	.24 (.31)
Two-period Lagged Correlation	.26	.70 (.37)

Source: Congressional Staff Directory, various years; *Report of the Clerk of the House,* various years; *Report of the Secretary of the Senate,* various years.

*The correlation between growth and turnover, with post-Watergate years (1976, 1977) deleted, is reported parenthetically.

ber in the following year. There appears to be no temporal effect, but moderate-to-strong chamber and party effect, with Republican and House staffers tending to remain on the Hill more frequently than their counterparts. It is possible, though implausible, that after more than a year's lag, some rejoin Hill staffs; but the bottom line is that, among personal staff, most descend from the Hill on the occasion of their patron's defeat or retirement to seek employment elsewhere. This is spectacularly true of Senate Democratic personal staffers, who leave the Hill at better than an 80 percent rate. For some staffers, it might be argued that these results do not reflect, as we have contended, a *motivation* to seek employment elsewhere after a credentialing experience; rather, they reflect the *unemployability* of these staffers elsewhere on the Hill. Surely this is so for some and, in fact, we have already observed that ambition and opportunity are complements. Yet the magnitudes in Table 2 are so large that we cannot but believe that most of the departures from the Hill on the occasion of defeats and retirements reflect a preference for alternative employment, not the paucity of Hill opportunities.

Member-Staffer Relationships. In remarking earlier that the personal disposition of staffers to stay or leave is heavily dependent on the ties that bind them to their principals, the members of Congress, we underscored the empirical importance of the relationship between member and staffers. Here we examine several of the empirical manifestations of this ties-that-bind hypothesis.

A crude indicator of the hypothesis in question was given in the previous table. When members leave the House or Senate, not only does their personal staff turn over (by definition), but on average 66 percent of the House staffs and nearly 78 percent of the Senate staffs leave the Hill altogether. Of those that remain, we found it remarkable to discover how many are associated with House members who cross chamber boundaries. In Table 3 we examine the two most recent elections to determine the frequency with which former House members elected to the Senate recruit their new staffs from among their old. These data indicate that the ties that bind appear to be quite strong.

The other side of the coin considers the staff recruits of newly elected members. We examined, for the years 1967 and 1977, the extent to which new representatives and senators select staffers with prior Hill background. While it appears somewhat more common than in the past to recruit experienced personal staff (especially in the Senate), it is further testimony in support of the ties-that-bind hypothesis that most recruitment by new members comes from *off* the Hill, often from campaign staffs and previous acquaintances in the district or state (despite warnings from congressional party leaders to seek out experienced staff assistance)—a point recently confirmed by

Table 2. Personal Staff of Departing Members Who Also Leave the Hill (Percent)

	House		Senate	
	1967	1977	1967	1977
Democrats	72	64	82	82
N =	(295)	(289)	(56)	(207)
Republicans	49	63	64	78
N =	(61)	(147)	(28)	(153)
Total	68	64	76	78
N =	(356)	(436)	(84)	(360)

Source: Congressional Staff Directory, 1966, 1967, 1976, 1977.

Table 3. House Personal Staff Following Representative to the Senate (Percent)

	1977	1979
Democrats	63	79
N =	(28)	(33)
Republicans	43	57
N =	(14)	(56)
Total	58	65
N =	(42)	(89)

Source: Congressional Staff Directory, 1976, 1977, 1978, 1979.

Loomis (1979) for the House. In both 1967 and 1977, new House members, Republicans and Democrats alike, chose only about one-fourth of their staffs from among those with previous Hill experience. In the Senate, the figures were 19 percent and 30 percent, respectively, again with no significant partisan differences.

An examination of the ties-that-bind hypothesis with regard to *committee* staff provides a somewhat different picture. Tables 4 and 5 provide mean staff turnover for the period 1962-78 for each standing committee in the House and Senate, respectively, as drawn from the *Congressional Staff Directory* of each year. We have broken down the computation according to whether the turnover occurred in years in which the identity of the committee chairperson changed, in years bracketing such change (thus allowing for both anticipatory and lagged turnover), and in years with no chairperson change. The test recorded in the first column consists of comparing whether turnover recorded in the second column is less than the maximum of columns (3) and (4). To illustrate, the House Agriculture Committee is consistent with the ties-that-bind hypothesis since turnover in non-chairmanship change years (16 percent of the 287 staffers listed in the *Congressional Staff Directory* in non-chairmanship change years were not listed in the following year) was less than the maximum of turnover in chairmanship change years (24 percent) or "bracketed" years (22 percent)—in fact, it was less than either. Only the Veterans' Affairs Committee in the House and the Agriculture, Armed Services, Foreign Relations, and Interior committees in the Senate fail this test (Senate Judiciary could not be tested since there were no chairmanship changes during the period in question). However, the test is not a very stringent one. For example, comparing (2) and (3) and considering the data to be consistent with the ties-that-bind hypothesis if (3) exceeds (2) by at least five percentage points—a seat-of-the-

Table 4. Ties-That-Bind Hypothesis: House Committee Staff, 1962-78

		Mean Committee Turnover[a]					
	(1) Consistency with TTB Hypothesis[b]	(2) Years with No Chairperson Change		(3) Years with Chairperson Change		(4) Year of, Year before, Year after Chairperson Change	
		%	N	%	N	%	N
Agriculture	Yes	16	(287)	24	(24)	22	(104)
Appropriations	Yes	15	(100)	89	(18)	42	(38)
Armed Services	Yes	11	(296)	13	(60)	9	(185)
Banking and Currency	Yes	43	(298)	35	(20)	58	(105)
Budget	Yes	20	(122)	23	(66)	21	(122)
District of Columbia	Yes	27	(226)	89	(9)	48	(31)
Education and Labor	Yes	17	(446)	56	(27)	27	(64)
Foreign Affairs	Yes	16	(361)	40	(60)	26	(163)
Government Operations	Yes	18	(205)	27	(30)	21	(96)
House Administration	Yes	29	(135)	47	(45)	35	(127)
Interior	Yes	14	(224)	26	(50)	19	(141)
Interstate	Yes	18	(379)	22	(18)	22	(55)
Judiciary	Yes	13	(200)	50	(18)	27	(51)
Merchant Marines	Yes	9	(209)	38	(64)	18	(167)
Post Office	Yes	20	(382)	27	(56)	15	(144)
Public Works	Yes	16	(277)	23	(117)	19	(308)
Rules	Yes	15	(84)	30	(33)	23	(83)
Science and Astronautics	Yes	25	(309)	26	(27)	19	(72)
Small Business	Yes	27	(45)	38	(26)	31	(71)
Standards	Yes	15	(47)	25	(4)	36	(14)
Un-American Activities	Yes	19	(238)	33	(48)	23	(146)
Veterans Affairs	No	16	(218)	10	(31)	12	(82)
Ways and Means	Yes	15	(483)	23	(30)	14	(107)

Source: Congressional Staff Directory, various years.

[a] Each entry gives the mean staff turnover, by committee and leadership change contingency, for the period 1962-78.

[b] TTB Hypothesis: Turnover occurs contemporaneously with, in anticipation of, or in response to committee leadership changes.

Table 5. Ties-That-Bind Hypothesis: Senate Committee Staff, 1962-78

		Mean Committee Turnover[a]					
	(1) Consistency with TTB Hypothesis[b]	(2) Years with No Chairperson Change		(3) Years with Chairperson Change		(4) Year of, Year before, Year after Chairperson Change	
		%	N	%	N	%	N
Aeronautics and Space	Yes	19	(144)	30	(23)	27	(51)
Agriculture	No	17	(173)	0	(7)	0	(22)
Appropriations	Yes	10	(448)	16	(176)	13	(323)
Armed Services	No	14	(249)	11	(9)	11	(28)
Banking and Currency	Yes	18	(280)	37	(41)	27	(111)
Commerce	Yes	29	(631)	26	(68)	41	(129)
District of Columbia	Yes	34	(116)	61	(23)	51	(51)
Finance	Yes	17	(246)	33	(6)	20	(20)
Foreign Relations	No	18	(465)	9	(34)	9	(111)
Governmental Affairs	Yes	16	(180)	67	(24)	37	(62)
Interior	No	27	(355)	9	(11)	21	(28)
Judiciary	NA	14	(181)	–	(0)	–	(0)
Labor and Public Welfare	Yes	33	(368)	40	(50)	28	(120)
Post Office	Yes	28	(186)	54	(28)	36	(81)
Public Works	Yes	20	(306)	42	(12)	23	(40)
Rules	Yes	25	(132)	27	(49)	26	(98)
Veterans Affairs	Yes	30	(87)	81	(16)	38	(50)

Source: Congressional Staff Directory, various years.

[a] Each entry gives the mean staff turnover, by committee and leadership change contingency, for the period 1962-78.

[b] TTB Hypothesis: Turnover occurs contemporaneously with, in anticipation of, or in response to committee leadership changes.

pants difference-of-means test—we find that six committees in the House and six in the Senate fail the test.

More damaging to the ties-that-bind hypothesis, even if turnover in years of chairperson change tends to exceed that in other years, is that despite considerable variation, there is relatively little committee staff turnover even in years of chairperson change. For 13 of the 23 House standing committees and 9 of the 16 Senate standing committees for which there were data, less than one-third of the staff turned over when the committee chairmanship changed hands. While these data hardly force us to embrace the notion of an entrenched bureaucracy, it does appear that committee staff turnover is far less dependent on member-patron relationships than personal staff turnover. Committee staffers tend, with some frequency, to survive chairperson changes (though there is an occasional "big" number in column 3) so that their turnover rates are considerably more independent of member turnover than a strict reading of the ties-that-bind hypothesis would require.

There are some qualifications to this moderately negative reading of committee staff turnover vis-à-vis the ties-that-bind hypothesis. As noted earlier, a number of staff are nominally appointed by the chair, but, in fact, are chosen by others. Thus, some minority staff and/or subcommittee staff are listed in the *Congressional Staff Directory* as full-committee staff (apparently appointed by the chair) when, in fact, they are not. Since they do not leave when a chairperson departs, they are counted as evidence against the ties-that-bind hypothesis when they probably should not be counted at all. A second mitigating circumstance is the capacity of a staffer to serve loyally his or her boss, the chairperson, while at the same time demonstrating to other senior comittee members (and future chairpersons) a capacity for loyalty, ability, and general value to *any* chair of the committee. That such a staffer is retained after a committee leadership change may not constitute a strong violation, and is certainly in the spirit, of the ties-that-bind notion. Here, loyalty, ability, and capacity for good working relationships involve a staffer and several patrons. Thus, while the ties that bind between *committee* staff and chair are probably somewhat stronger than what has been revealed in Tables 4 and 5, they still remain less conspicuous and less constraining than in the case of *personal* staff.

Presumably, even if committee staff turnover were not very high in years of chairperson changes, one might derive some confidence in the superior position of elected officials and the subservient, responsive position of staff so long as staff *leaders* were replaced when committee chairs changed hands. One can see from the data in Tables 6 and 7 that only some confidence is warranted. Sixteen of 22 House committees and 14 of 17 Senate committees pass the weak consistency test described above. But only six of the House committees and seven of the Senate committees *always* experienced staff director changes in conjunction with chairmanship changes. Thus, turnover of neither committee staffs nor committee staff directors is sufficiently frequent or sufficiently related to committee leadership changes to disabuse us of the notion that committee staffs have some of the characteristics of a permanent bureaucracy.

Situational Factors. In this last empirical subsection, we examine a few of the situational factors tht affect staff turnover decisions, though others must await more extensive empirical inquiry. Our focus is chiefly on committee staff.

Consider, first, an institutional change hypothesis. The two houses of Congress have undergone some major changes in the last decade (Ornstein, 1975; Dodd and Oppenheimer, 1977). In the House, beginning in 1971 but especially after the 1974 election, there has been tremendous acceleration in a secular change having its roots in the early twentieth-century revolt against Speaker Cannon—decentralization and the impoverishment of leadership. The major consequences of

Table 6. Ties-That-Bind Hypothesis: Changes in House Staff Directors, 1962-78

		Mean Committee Turnover[a]					
	(1) Consistency with TTB Hypothesis[b]	(2) Years with No Chairperson Change		(3) Years with Chairperson Change		(4) Year of, Year before, Year after Chairperson Change	
		%	N	%	N	%	N
Agriculture	Yes	7	(15)	0	(2)	17	(6)
Appropriations	No	13	(16)	0	(1)	0	(3)
Armed Services	Yes	7	(14)	67	(3)	22	(9)
Banking and Currency	Yes	7	(15)	0	(2)	17	(6)
Budget	Yes	25	(4)	0	(1)	33	(3)
District of Columbia	Yes	19	(16)	100	(1)	33	(3)
Education and Labor	Yes	13	(16)	100	(1)	67	(3)
Government Operations	Yes	7	(15)	50	(2)	33	(6)
House Administration	Yes	21	(14)	33	(3)	33	(9)
Interior	No	13	(15)	0	(2)	0	(6)
International Relations (Foreign Affairs)	Yes	13	(16)	100	(1)	33	(3)
Interstate	No	0	(16)	0	(1)	0	(3)
Judiciary	Yes	6	(16)	100	(1)	33	(3)
Merchant Marines	Yes	14	(14)	67	(3)	44	(9)
Post Office	Yes	14	(14)	33	(3)	25	(8)
Public Works	Yes	0	(13)	25	(4)	9	(11)
Rules	Yes	14	(14)	33	(3)	33	(9)
Science and Astronautics	Yes	0	(16)	100	(1)	33	(3)
Small Business	Yes	0	(2)	100	(1)	33	(3)
Standards	No	0	(10)	0	(1)	0	(3)
Un-American Activities	Yes	9	(11)	0	(2)	17	(6)
Veterans Affairs	No	7	(15)	0	(2)	0	(5)
Ways and Means	No	6	(16)	0	(1)	0	(3)

Source: Congressional Staff Directory, various years.

[a] Each entry gives the proportion of years, by committee and leadership change contingency, in which there was turnover of the staff director for the period 1962-78.

[b] TTB Hypothesis: Turnover occurs contemporaneously with, in anticipation of, or in response to committee leadership changes.

Table 7. Ties-That-Bind Hypothesis: Changes in Senate Staff Directors, 1962-78

	(1) Consistency with TTB Hypothesis[b]	Mean Committee Turnover[a]					
		(2) Years with No Chairperson Change		(3) Years with Chairperson Change		(4) Year of, Year before, Year after Chairperson Change	
		%	N	%	N	%	N
Aeronautics and Space	Yes	15	(13)	50	(2)	33	(6)
Agriculture	No	7	(15)	0	(1)	0	(3)
Appropriations	Yes	15	(13)	25	(4)	22	(9)
Armed Services	Yes	19	(16)	100	(1)	67	(3)
Banking and Currency	Yes	7	(15)	100	(2)	33	(6)
Budget	No	33	(3)	–	(0)	–	(0)
Commerce	Yes	19	(16)	100	(1)	100	(2)
District of Columbia	Yes	8	(13)	50	(2)	40	(5)
Finance	Yes	6	(16)	100	(1)	33	(3)
Foreign Relations	Yes	13	(16)	0	(1)	33	(3)
Government Affairs	Yes	7	(15)	100	(2)	40	(5)
Interior	Yes	6	(16)	100	(1)	33	(3)
Judiciary	No	18	(17)	–	(0)	–	(0)
Labor and Public Welfare	Yes	20	(15)	100	(2)	40	(5)
Post Office	Yes	15	(13)	50	(2)	17	(6)
Public Works	Yes	33	(15)	50	(2)	50	(6)
Rules	Yes	0	(14)	33	(3)	11	(9)
Veterans' Affairs	NA	–	–	–	–	–	–

Source: Congressional Staff Directory, various years.

[a] Each entry gives the proportion of years, by committee and leadership change contingency, in which there was turnover of the staff director for the period 1962-78.

[b] TTB Hypothesis: Turnover occurs contemporaneously with, in anticipation of, or in response to committee leadership changes.

this trend, especially in the post-1970 House, are (1) a decline in the power of institutional, party, and committee leaders, (2) the growth of subcommittees in both numbers and power, and (3) the more widely dispersed distribution of subcommittee chairmanships owing to Democratic Caucus rules. This growing autonomy of the individual member is found in the Senate as well, where most majority party members have come to possess multiple committee and subcommittee chairs. As well, there was a major reorganization of the Senate committee system in the mid-1970s. These institutional changes, in and of themselves, shake up staff relationships, thereby inducing turnover. But, in addition, they have secondary effects on other relationships. For example, *member* turnover of the mid-1960s among both Democrats and Republicans, though quite high, was far less consequential for committee staff turnover than the somewhat lower turnover of the 1970s (in the House, at least), since, during the latter decade, many more members had responsibility for hiring and firing committee staff.

The gist of this line of argument is that institutional change is a proxy for and/or sensitizes changes in factors and relationships which affect staff turnover. To examine this hypothesis, Table 8 arrays turnover statistics by committee and chamber, partitioning the data at the year 1970.[14] Thus, for the periods 1962-69 and 1970-78, we computed the mean committee staff turnover for each committee in each chamber. Although the differences in means are not always large, turnover during the decade of institutional flux (1970-78) exceeded that for the more institutionally quiescent period (1962-69) for 18 of the 20 House committees that existed in both periods, and for 14 of the 16 Senate committees. A more stringent test—that turnover in the latter period should exceed that in the former period by more than five percentage points—finds 13 of 20 committees in the House and 10 of 16 committees in the Senate compatible with the institutional change hypothesis. A parallel analysis for changes

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[14] We chose to break the data at 1970 since the 1960s were far more stable, in terms of change in institutional structure and practice, than the 1970s. The year 1970 is also conveniently the midpoint of our time series.

Table 8. Committee Staff Turnover (Percent)

Committee	House Mean Turnover		Senate Mean Turnover	
	1962-69	1970-78	1962-69	1970-78
Agriculture	14	18	0	23
Appropriations	7	22	3	17
Armed Services	8	10	7	18
Banking and Currency	29	34	12	25
District of Columbia	26	30	26	55
Education and Labor (Labor and Public Welfare)	25	15	24	42
Foreign Affairs (Foreign Relations)	4	26	23	14
Government Operations	9	24	8	32
Interior	10	20	26	27
Interstate and Foreign Commerce (Commerce)	17	19	18	34
Judiciary	12	22	15	13
Post Office and Civil Service	18	23	32	39
Public Works	12	20	18	22
Rules	13	24	23	25
Science and Astronautics	10	35	16	24
Ways and Means (Finance)	16	15	15	19
Veterans' Affairs	12	16	NA	38
Budget	NA	21		
House Administration	14	39		
Merchant Marine	9	19		
Small Business	NA	31		
Standards of Official Conduct	0*	19		
Un-American Activities	22	NA		

Source: Congressional Staff Directory, various years.

*Based on only two years.

in committee staff directors or chief clerks reveals that, in the House, staff director turnover in the period of flux exceeded that in the period of quiescence for 12 of 16 committees where there was a difference (there was no difference in four committees); in the Senate, the period of flux dominated the period of quiescence in 11 of 14 committees (two showed no difference).[15] In sum, there is reasonably strong support, though not without exceptions, for the institutional change hypothesis. Times of institutional rearranging do induce staff turnover.

Before turning to a discussion of the implications of staff turnover, we examine two final situational factors. The first—the partisan change hypothesis—argues that staff turnover is higher in years following a partisan change in the executive branch. Capitol Hill has long been a source for executive branch recruitment and, while such recruitment goes on continuously, it should be especially noticeable when a new administration is being staffed. From our data on committee staff turnover (tables available from authors), we compared mean turnover, by committee, in years of partisan change in the White House (1969, 1977) with that of other years in the 1962-78 period. For the House, the mean turnover in partisan change years exceeds that in other years for 20 of 21 committees (there is no difference for one and the comparison could not be made for another); in the Senate, this is true for 14 of 18 committees. Requiring a five-percentage point difference in order to support the hypothesis—the more stringent test—yields support for the partisan change hypothesis in 17 of 22 House committees and 12 of 18 Senate committees. By way of qualification, however, turnover in odd-numbered years tends to exceed that in even-numbered years, generally. Elections per se, whether midterm or presidential and whether involving a partisan change in executive or not, affect turnover in membership and hence in staff. Nevertheless, even though we cannot distinguish, with statistical confidence, between partisan change years and other post-election years, the high turnover of 1969 and 1977 reminds us that changes in the executive branch have impact upon and ripple throughout the entire Washington community.

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[15] We also examined leadership staff and subcommittee staff, though data collection on this latter group by *Congressional Directory* is not reliable since committee designations were so variable and idiosyncratic. In any event, the institutional change hypothesis is strongly supported there, too.

The last situational factor is chamber-specific. House and Senate committee staffs have different turnover rates. The S. Res. 60 arrangements (allowing senators to hire personal assistants with committee funds), coupled with a "life in the fast lane" feeling about Senate staff life, lead us to expect more turnover in the upper chamber. In order to examine the hypothesis of different turnover rates in the two chambers, we computed *mean committee half-lives*. For each committee, we examined yearly staff cohorts and determined how many years were required before half the cohort of the given year was no longer associated with the committee. Thus, for example, the 1962 House Agriculture Committee staff cohort was half-depleted sometime during 1967-1968; its half-life, consequently, was recorded as 5.5 years. Computing this number for each yearly cohort for which there were available data, and then further computing the mean half-life, we were able to rank-order House and Senate committee counterparts. Table 9 contains the results. For 13 of 17 paired committees, House mean half-life is higher. In only one of these instances is the difference less than a full year. In this sense, House full committee staffs would appear to be more stable and long-lasting.

Implications and Discussion

We have examined several hypotheses regarding congressional staff turnover against data that, while far from ideal, at least provide some insight about the impact on staff turnover of personal motivations, relationships between staffers and members, and situational factors, respectively. We have found significant but not wholly unambiguous support for the hypothesis that turnover results in part from the fact that many staffers look upon their Hill service as a credentialing experience and hope to fulfill their main life ambitions elsewhere. Secondly, we have found strong support for our ties-that-bind hypothesis as personal staffers stay or leave in close tandem with the members with whom they are primarily associated. Third, we found a more mixed picture in the case of committee staffs. In general, staffers are not simply birds of passage, especially those who work for committees; but neither are they as permanent as bureaucrats in the executive branch.

Institutional change within Congress has had important effects in expanding the number of

Table 9. Committee Staff Half-Lives, 1962-78

House		House-Senate Comparison	Senate	
Committee	Mean Half-Life*		Committee	Mean Half-Life*
Armed Services	7.7	>	Armed Services	4.7
Public Works	6.4	>	Public Works	3.6
Ways and Means	6.0	>	Finance	3.7
Veterans' Affairs	5.9	>	Veterans' Affairs	1.9
Science and Astronautics	5.4	>	Space	3.3
Government Operations	5.4	>	Governmental Affairs	3.7
Foreign Affairs	5.4	>	Foreign Relations	4.9
Agriculture	5.4	<	Agriculture	7.0
Merchant Marine	4.8	–	NA	–
Commerce	4.5	>	Commerce	3.0
Interior	4.4	>	Interior	3.4
Post Office	4.3	>	Post Office	1.8
Rules	4.3	>	Rules	3.1
Education and Labor	3.9	>	Labor and Public Welfare	2.0
Judiciary	3.8	<	Judiciary	4.4
District of Columbia	3.4	>	District of Columbia	1.6
Ethics	2.8	–	NA	–
House Administration	2.7	–	NA	–
Appropriations	2.5	<	Appropriations	5.5
Banking and Currency	2.4	<	Banking and Currency	5.0
Budget	2.2	–	NA	–
Small Business	2.0	–	NA	–

Source: Congressional Staff Directory, various years.

*For each yearly staff cohort, by committee, the number of years required for 50 percent turnover was computed and then averaged over all yearly cohorts, 1962-1978.

staff positions and decentralizing the control of those positions, and these changes have led to increased turnover. We found also that staff turnover exhibited spurts following partisan turnover in the executive branch, and that generally the "fast track" of the Senate generated higher turnover rates than the House. Finally, we may note that all of these findings are reinforced by anecdotal evidence available in published form and are amply supported (as indeed they were often generated) by the extended interviews we have had during the past two years or so with staff members.

What are the consequences—for the operations of Congress and for the larger political system—of these rates and patterns of congressional staff turnover? First, what is the impact on the member of Congress? At the outset, we suggested that Congress has come to operate in a manner best understood as a collection of enterprises rather than as 535 discrete individuals. Each elected member hires and presides over the work of between 18 and 125 or so individuals, depending on chamber, state, and committee or subcommittee responsibilities. Necessarily, much of the time and energy of the members must be devoted to managing these enterprises, and, in a very real sense, the most direct effect of staff growth has been to transform the role of member from that of a legislator/politician into that of an executive manager.

What are the effects of staff turnover on these enterprises? Is it so rapid as to compound beyond endurance the management problems of the member, or render them sufficiently serious as to handicap severely the work of the enterprise; i.e., securing reelection and processing legislation? Our answer must be impressionistic but it is firm, nevertheless. Turnover among staff is not so great as to create severe management problems, and one important reason this is so is precisely why our data have shown support for the ties-that-bind hypothesis. Because staffers are hired by specific members and work under their direction, they soon come to accept a norm structure that emphasizes loyalty, of a very personal and specific kind, to be given to that member. Loyalty, in turn, implies a commitment to stay with the job, not necessarily forever or even until the member leaves, but for a "reasonable" tour of duty.

Not all staffers can accommodate themselves to this norm. Ideological commitment may dominate personal loyalty for some, and in such cases staff careers may be brief and turnover considerably higher. Serving a member of Congress is not the same thing as accomplishing a legislative agenda, though the one cannot usually be done except by means of the other. Staffers with deeply felt policy interests may find their Hill experience both frustrating and consuming, "burning out" rather quickly and returning to academe or moving to other arenas to continue their agitation. For others, the norm of loyalty is deeply personal, invested in a particular member, and not easily transferred. These staffers are observed to move in tandem with their member-patron. Finally, some staffers stay on for longer periods, surviving member turnover, partisan shifts and institutional change. This category of "professional" staff approximates the style envisioned by the sponsors of the 1946 LaFollette-Monroney Act (Galloway, 1953; Kammerer, 1951). It, too, entails some commitment to the norm of personal loyalty, but the commitment is more malleable and ultimately transferable.

We have argued that, in a member-centered legislative world, staff turnover is kept within tolerable limits by means of the development of and adherence to a norm of personal loyalty binding the staffer to his or her principal. The reciprocal of that relationship is the member's obligation to "take care of" a loyal staffer, facilitating internal advancement and extra-congressional career prospects and providing useful opportunities if necessary when the member leaves the arena. The consequence is that a sizable fraction of time and effort in the Washington world is devoted to career development and protection, by nearly everyone, to be sure, but specifically by members of Congress on behalf of their staffs. Indeed, some legislators are known for their staff networks spread throughout the Washington community; while formal ties are severed, member and ex-staffer nevertheless "keep in touch."

A further consequence of the fact that staff operate within member-centered enterprises is that disaggregation of effort, attention, and loyalties is accentuated. Rather than encouraging a focus on the party, the caucus, or even the committee, the mutual commitments of individual staffers to individual members means that on each issue consensus among both staffers and members must be *achieved* rather than emerging out of a common set of structural orientations. The dominant structural unit becomes the member-centered enterprise itself.[16] Members who are enmeshed in managerial responsibilities involving interactions with their staff are correspondingly insulated from interactions with other members. They are less likely to work closely with each other and hence less likely to develop norms and values in consequence of shared experience.

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[16] Even on the relatively consensual and "professional" House Committee on Appropriations, where staff is recruited by the full committee and its staff director, but for the most part is assigned to one of the 13 subcommittees, staffer loyalty is only nominally collective, to the full committee and the subcommittee. In large measure, staffer loyalty is to the particular subcommittee chairman. That this loyalty is easily transferable when a subcommittee chairmanship changes is a reflection of a particular kind of professionalism that marks both staff and members of this committee (Fenno, 1966). And in the Wilbur Mills years, the modest

On the other side of this coin is the active role of staffers themselves in the legislative process. One hears constant references in Washington circles to coalitions and settlements being negotiated by staffers in the name of their respective principals, and several observers have expressed alarm at the potential for abuse by these "unelected representatives" (Malbin, 1980), this "invisible force" (Fox and Hammond, 1977). It is very doubtful that much staff activity exceeds the bounds of member acceptance, but there is no doubt that a legislative process involving 535 enterprises with many thousands of individuals is very different from one in which the members act as unencumbered individuals.

If we look beyond Capitol Hill to the larger political community, staff growth and turnover has had one very important consequence. Congressional staff has become a kind of giant recruitment pool for our contemporary public life. Employment in the congressional milieu is comparatively open: no competitive examinations, no affirmative action, much spur-of-the-moment hiring. Executive agencies draw upon this source of people who, however innocent they were to start, are now experienced and knowledgeable in the ways of Washington. Interest groups do the same. So do law firms, public interest organizations, the media, and even academic institutions. Congress itself in 1980 contained nearly 60 members who once were members of congressional staffs. In a very important sense, the growth and turnover of congressional staff has served to provide an expanding pool of competent people to fill the myriad roles which modern public life requires. Former staffers provide a significant body of individuals who "know the system" at a relatively complex level and can serve as guides and counselors to those who do not yet possess that competence. Insofar as this is true—as congressional staff experience provides a basic apprenticeship for national public service in the United States—the functional significance of staff to the political order goes far beyond the traditional categories of discussion. Not only has its sheer size and its organization into member-centered enterprises transformed a nominally atomistic legislature of 535 individuals into an "industry" of nearly 1000 "political firms"; more significant, perhaps, is the fact that leaders in all walks of political life are increasingly learning the system from the vantage point of a congressional staff.

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staff of the House Ways and Means Committee, plus the staff of the Joint Committee on Internal Revenue Taxation, worked so completely for Mills, as did the other committee members for that matter, that disunity within the committee seldom appeared (Manley, 1968, 1970). Such domination of committees by their chairperson is largely a thing of the past, however, and it has become very difficult for staffers to serve a committee membership at large. Even the nominal collective loyalty on House Appropriations is sustained by the fact that staff tasks are highly routinized, and the responsibility of a "professional" is very like that of a "clerk" (which title, by the way, subcommittee staff directors on Appropriations bear).

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The Speaker's Task Force in the Post-Reform House of Representatives

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Because of changes in the issues at the center of controversy, changes in rules and norms and high membership turnover, the current House majority party leaders operate in a much less predictable environment than their predecessors did. In response to the changed environment, Speaker O'Neill has developed the strategy of leadership by inclusion, a central element of which is the Speaker's task force, an ad hoc group appointed by the Speaker and charged with passage of a specific bill. The leaders believe task forces help them perform both of their primary functions—building winning coalitions and "keeping peace in the family." By increasing the number of people working in an organized way to pass the bill at issue, the task force increases the probability of a bill's success on the floor. Work on a task force satisfies junior members' expectations of participation and fosters cooperative patterns of behavior among party members.

During the 1970s, changes in rules and norms, high membership turnover, and the rise to prominence of new party-fragmenting issues transformed the environment in which the majority-party leadership operates in the U.S. House of Representatives. The current leaders work in a much less predictable environment than their predecessors did.

Environmental change has not altered the functions which majority-party leaders are expected to perform. The leadership is charged with constructing winning coalitions on major legislation and thereby satisfying the legislative expectations of their membership and of significant actors outside the chamber. "Keeping peace in the family," as the current leaders themselves express it, is an equally important function. Centrally involved in peace-keeping are satisfying expectations of members about their individual roles in the chamber and fostering cooperative patterns of behavior among party members. The two functions are interrelated in a complex fashion. The need to promote harmony places limits upon the coalition-building strategies which can be used. On the other hand, to the extent that leaders are successful at "keeping the peace," their probability of future success in building winning coalitions is increased. The necessity of passing legislation which is controversial within the party inevitably strains party harmony. Yet, to the extent that leaders are sucessful at building winning coalitions on bills which satisfy their membership's expectations as to legislative results, they contribute to intra-party harmony.

Although the functions of leadership have not changed, successful performance requires strategies different from those appropriate in the pre-reform era. Scholars increasingly agree that congressional leadership is best understood from a contextual perspective (Jones, 1980; Cooper and Brady, 1980). The context or environment shapes and constrains leadership styles and strategies. The highly informal Rayburn style was predicated upon the power of the committee chairmen and the concomitant limited influence of rank-and-file members (Cooper and Brady, 1980; see also Ripley, 1967, 1969; Peabody, 1976; Jones, 1968). A strategy based upon bargaining with a small number of key actors obviously becomes untenable when the number of significant actors expands to include almost the entire House membership. Not only would such a strategy be unsuccessful in the short run, by thwarting members' expectations of full participation in the legislative process, it would also decrease the probability of future success.

In response to the changed environment, Speaker O'Neill has developed the strategy of leadership by inclusion. Effective leadership in the post-reform House, the Speaker believes, requires including as large a proportion of Democrats as possible in the coalition formation process. A central element of the strategy of inclusion is the Speaker's task force, an ad hoc group appointed by the Speaker and charged with passage of a specific bill. The leaders believe task forces have payoffs in terms of both of their central func-

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I would like to thank the American Political Science Association Congressional Fellowship program, which enabled me to do this research. Very special thanks are due to Representative Jim Wright and the staff of the majority leader's office for giving me a home base as well as much help and friendship during my fellowship year. All unattributed quotations are from interviews conducted by the author. I, of course, owe a major debt to the members and staff who talked with me. Finally, several anonymous reviewers and the editor of the *Review* provided very helpful suggestions.

tions. By increasing the number of people working in an organized way to pass the bill at issue, the leaders increase the probability of success on the bill. Work on a task force gives junior members "a piece of the action" and satisfies their expectations of participation. In addition, the leaders believe that task force membership has a positive socializing effect, especially on junior members. Work on a task force gives them an understanding of the problems of the leadership, and, it is hoped, will teach them the value of joint action under the aegis of the leadership.

This study examines the origins of the task force concept, the type of bills on which task forces have been used, task force membership and how task forces operate. One purpose is to describe an as yet little-known leadership tool. A second is to illuminate the relationship between leadership strategies and the environment in which leaders work by analyzing how the leaders see task forces as aiding them in coalition building and as contributing to "keeping peace in the family."

The New Environment

An understanding of why new leadership strategies became necessary requires a brief review of the transformation of the environment in which the leadership works.

Rules Changes. The 1970s saw a constellation of rules changes aimed at curbing the power of committee chairmen and at dispersing positions of influence more widely throughout the House (see Ornstein, 1975; Dodd and Oppenheimer, 1977). Each member was limited to chairing only one subcommittee. Full committee chairmen were stripped of the power to select subcommittee chairmen and that power was vested in the Democratic caucus of the committee. Subcommittees were required to have fixed jurisdiction and adequate budget and staff. Full committee chairmen and the chairmen of Appropriations subcommittees were made subject to approval by secret ballot in the Democratic caucus.

The decade also saw an immense growth in the staff resources available to members of the House. In 1967, personal staff totaled 4,055; in 1979, that figure stood at 7,067. The number of employees of House committees rose from 702 in 1970 to 1,959 in 1979 (Bibby et al., 1980, pp. 69-70). The most junior member is now entitled to hire a staff of at least 18.

The decline in the power of committee chairmen, the dispersal of influence to the subcommittee level, and the augmentation of members' staff resources make wide participation in the legislative process possible. Another set of rules changes which can be summarized under the rubric of "sunshine reforms" further increase the incentives for participation. Except under special circumstances, all committee meetings, mark-up sessions as well as hearings, must be open to the public. Since the advent of the recorded teller vote, voting on amendments in the Committee of the Whole is no longer anonymous. The increased visibility of members' actions in committee and on the floor encourages the use of these arenas for making a variety of political points and subjects members to a wider range of pressures.

Membership Turnover. Membership turnover in the 1970s has been very high by post-World War II standards. The number of Democratic freshmen and the proportion of Democratic members who were freshmen was unusually high in the 94th, 95th and 96th Congresses. The 75 freshmen elected in 1974 in the wake of the Watergate scandal constituted a little more than a quarter of the Democratic membership during that Congress. The 47-member Democratic freshmen class elected to the 95th Congress and the 42-member class elected to the 96th were larger than any class between 1960 and 1972, except for the 89th Congress class elected in the 1964 landslide. The result of this heavy turnover has been an increasingly junior Democratic membership. In the 94th Congress, 44 percent of Democrats were serving their first, second, or third term; that figure rose to 51 percent in the 95th and to 53 percent in the 96th. In contrast, between 1961 and 1974, the mean percentage of first-, second- and third-termers was only 31 percent; its highest value was 42 percent in the 89th Congress.

For the leadership, the sheer number of newcomers presents a problem, since the rules now make possible full participation by the most junior member. The leaders obviously lack longstanding relationships with these members. Yet for the leadership such relationships are crucial for making informed judgments about the probable behavior of members and provide the basis for effective persuasion.

Changes in Norms. Accompanying the change in rules and membership, has been a change in norms. Socialized to politics during the turbulent 1960s, the freshmen of the 1970s entered the House in a period of challenge to the power structure; many of the large '74 Democratic class were elected from previously Republican districts. Both personal inclination and political necessity dictated a high level of activity and immediate participation at the committee and the floor stage. Rules changes which lessened the control of seniority leaders and augmented the staff resources of junior members made such activity possible. Thus norms of apprenticeship and of deference to senior members, already weakened during the 1960s, were given a final blow.

The events of the 1970s have diminished the prestige of the committees and undermined the norm of reciprocity among committees. Previously, there existed a strong presumption in favor of the committee's bill. "Go with the committee on the floor" with the watchword; "the committee has exhaustively examined the matter and has brought its expertise to bear." The reciprocity norm is much weaker now, and there is a much greater tendency to "legislate on the floor." Younger members are simply not willing to defer to committees.

New Issues. The issues at the center of controversy in the 1970s and early 1980s—energy, environmental and consumer issues, and foreign and defense policy questions—split not just North from South in the Democratic party; they also frequently divide northern Democrats (Sinclair, 1981). High inflation has engendered a budget-cutting mood which threatens to destroy the northern Democratic consensus on traditional economic and social welfare policy. Socio-cultural issues such as busing and abortion continue to excite controversy.

Because of the change in floor procedure, many of the divisive issues come to a recorded vote over and over during a Congress. The highly emotional abortion issue is a good example. Anti-abortion members are proposing amendments barring federal funding to more and more bills and are forcing roll-call votes. The frequency with which members are compelled to take public stands on such highly charged issues cannot help but exacerbate the leadership's problems.

Impact upon Party Leadership and the Strategy of Inclusion

The changes of the 1970s have resulted in a much less predictable House environment. According to a top leadership figure, "the democratization of the House has proceeded so far that all kinds of things can come percolating up from below and that can cause problems for the leadership."

The recorded teller vote and the decline of intercommittee reciprocity have severely heightened unpredictability at the floor stage of a bill. Since neither junior status nor party affiliation nor lack of expertise bars members from offering amendments, amendments can come from anywhere; it is impossible to anticipate all that may be offered. The House, according to Morris Udall (D-Ariz.), has become a "fast breeder reactor" for amendments. "Every morning when I come to my office, I find that there are 20 new amendments. We dispose of 20 or 25 amendments and it breeds 20 more amendments" (*Congressional Quarterly*, p. 877). The number of recorded votes grew manyfold during the 1970s. During the 91st Congress, the last before the institution of the recorded teller vote, the House took 443 recorded votes; during the 95th Congress (1977-78), 1540 recorded votes were taken.

As a vote can be recorded upon request by 25 members—20 until 1979—almost any member can force a vote. Republicans have become very adept at fashioning amendments with considerable constituency appeal. Votes which a clever opponent back home can represent as favoring reparations to Vietnam, "giving away" the Panama Canal, or supporting federally funded legal aid for homosexuals present a ticklish choice even to the Democrat who would like to support the leadership.

The large number of amendments offered to many major bills strains the leadership's resources and increases the probability of the committee majority and the leadership getting "ambushed" on the floor. "We are in the process of moving from a time in the history of the House when committees had too much power, and we are moving to a point where the floor itself becomes a large, unwieldy committee," according to Paul Simon, D-Ill., "We are not doing the job of legislating as we should. We are legislating by whim. . ." (*Congressional Quarterly*, p. 878).

The heightened level of floor activity is even more of a problem than it might be otherwise, because voting behavior has become less predictable. The leadership cannot count on solid support from northern Democrats as it could on most issues during the 1960s. On the other hand, the southern Democratic contingent in the House became more heterogeneous during the 1970s. As the race issue declined in salience in the South, somewhat more nationally oriented Democrats were elected. Consequently, when the party leadership attempts to build winning coalitions, there are few Democrats who can be written off completely and few who can be counted on to be loyal across the board (Sinclair, 1981).

The Speaker's response to heightened unpredictability is the strategy of inclusion. O'Neill sees effective leadership in the post-reform House as dependent upon including as many Democrats as possible in the coalition formation process. Coping with the decrease in predictability requires more extensive vote gathering. A larger proportion of the Democratic membership must be contacted and persuaded—and this task must be done many times. The task is simply too great to be handled solely by the top leadership. Others must be brought into the process.

The leadership could and, in many cases, does rely upon the whip system for help. During the 1970s, the whip system significantly expanded in size; by 1977, it consisted of the majority whip, a chief deputy whip, three deputy whips, 22 regionally elected whips, and 10 at-large whips. Several factors militate against exclusive reliance on the whip system for aid in coalition building. The whip system is charged with conducting whip polls and, as the number of polls has increased, this has become a time-consuming task. In the 95th Congress, about 80 whip counts were conducted. In addition, because the regional whips are elected by the members of their zones, their loyalty to party and to the leadership is sometimes minimal. The addition of at-large whips, appointed by the leadership, was a response to the problematical loyalty of the regional whips. The zone whips are expected only to do the counting; the appointed whips do act as an arm of the leadership in mobilizing support.

O'Neill's strategy of leadership by inclusion is not predicated only upon the leadership's need for a loyal group of assistants to aid in coalition building. It also encompasses satisfying members'—especially junior members'—desires for participation, for "a piece of the action." If junior Democrats are involved in the vote-mobilization process via task force membership, their very considerable energies are directed towards accomplishing leadership objectives and they are given a stake in the leadership's success. This involvement, it is hoped, will gradually teach junior members the need for "followership" and the value of joint action under the aegis of the leadership.

Origins of the Task Force Concept

The basic notion underlying the task force concept—that of joint organized effort to pass a specific bill—is, of course, not new. As a senior staffer explained: "People have been doing task forces of one kind or another for a hundred years or more. Any time that there's an interested group of people, they're going to get together and work on it in some kind of common front."

Task forces consisting of interest-group representatives have for years worked with the party leadership on legislation of common interest. The leadership has traditionally worked closely with committee leaders in passing major legislation and, during the early 1970s as the number of significant actors increased, the leadership seems to have expanded the number of members included in its vote-mobilization efforts.

The informality of such traditional arrangements distinguishes them from the current Speaker's task force. A senior staffer closely involved in the process explained: "In the sense that it's now defined, which is a pretty structured kind of thing, with a chairman appointed by the Speaker and a list of members, I guess you'd have to say that it started in the 95th Congress." He continued: "We have some tough bills. We had one that we lost and we lost it because we didn't do one or two things we could have done. I remain convinced to this day that we had the votes if we had played our cards right but it would have taken organization to do that."

An early loss which the leadership attributed to lack of organization acted as the catalyst. The loss taught the new leadership team that building winning coalitions in the post-reform House requires more organized and extensive vote-gathering efforts than were needed in the pre-reform era.

The Decision to Establish a Task Force

The decision to appoint a task force is solely the Speaker's, although he may do so upon the advice of other leadership figures or of staff. He will, of course, confer with the prospective floor manager of the legislation.

Asked about the types of bills for which task forces will be formed, a senior staffer explained:

> They are significant leadership issues where the Speaker has some prestige on the line, and, much more importantly than that, where the core of the party has to have the bill. Now, that's pretty easy to pick out. You look down the list of bills which came up in the Congress and you'll find the 8 or 10 bills that really had to pass last year—you know, we had to have the budget, we had to have the debt ceiling—some elements of the party had to have Humphrey-Hawkins; CETA comes in the same bag; energy, the Democratic party had to have; a tax bill we had to have of some sort—it isn't the tax bill that we would have liked to have, but we had to have it; and you look down the list of other bills, and I would guess that there aren't any that if they had not passed they would have done as much damage to the party or to the leadership or to the House, the Democratic members of the House, as those bills if they had not gone through, so that's how it's done.

Another staffer made the same point in the imperative voice: "[Task forces] should be used only on significant legislation that is clearly part of the Democratic party program." An important consideration, of course, is "how deep the trouble is." Table 1 lists all task forces appointed through the end of 1979. All the bills are ones on which trouble was expected and ones which the Democratic party as a whole or some significant segment thereof "had to have."

President Carter's top domestic priority during the 95th Congress was a comprehensive energy program. His prestige and that of the party and the leadership rode on success in passing energy legislation. Energy policy deeply splits congressional Democrats along consumer versus producer lines; consequently, the leadership knew passage would be difficult. The Energy Organization Act is the first bill on which a full-fledged task force was appointed. It actually consisted of three subgroupings, each charged with defeating or passing a key amendment.

The Senate drastically changed the energy legislation so as to make final House approval more difficult. Liberals were extremely upset by the gas-pricing section and House approval of the conference report rested upon passage of a rule which prevented a separate vote on that section. The ex-

Table 1. Speaker's Task Forces, 1977-1979

Task Force	Chair	House Vote	Democratic Vote	Date
Energy Organization Act (HR8444)				
Natural Gas	Sharp, Ind.	199-227	72-210	8/3/77
Plowback	Gibbons, Fla.	198-223	74-208	8/4/77
User Fees	Mikva, Ill.	221-198	212- 68	8/5/77
Passage	Sharp, Ind.	244-177	231- 50	8/5/77
Social Security (HR9346)				
Fisher Amendment	Fisher, Va.	386- 38	257- 24	10/26/77
Pickle Amendment	Tucker, Ark.	196-221	74-206	10/26/77
Passage	Gephardt, Mo.-Tucker	275-146	235- 46	10/27/77
Conference Report	Gephardt-Tucker	189-163	174- 54	12/15/77
Humphrey-Hawkins (HR50)	Rose, N.C.	252-152	233- 41	3/16/78
1st Budget (S. Con. Res. 80)	Derrick, S.C.	201-197	198- 61	5/10/78
Debt Ceiling (HR13385)	Gephardt, Mo.	205-202	196- 74	7/19/78
2nd Budget (H. Con. Res. 683)	Derrick, S.C.	217-178	215- 42	9/16/78
CETA (HR12452)	Miller, Calif.- Oberstar, Minn.- Mineta, Calif.			
Obey Amendment		230-175	114-152	8/9/78
Jeffords Amendment		221-181	101-162	8/9/78
Passage		284- 50	206- 18	9/22/78
Tax (HR13511)	Gephardt, Mo.			
Kemp Amendment		177-240	37-237	8/10/78
Passage		362- 49	224- 47	8/10/78
Energy (HR5289, HR5037, HR5263, HR5146, HR4018)	Sharp, Ind.			
Previous Question on Rule		207-206	199- 79	10/13/78
Adoption of Conference Report		231-168	185- 81	10/15/78
Debt Limit (HR1894, HR2534)	Gephardt, Mo.			
Previous Question on Rule		222-197	221- 44	2/28/79
Passage		194-222	191- 73	2/28/79
Previous Question on Rule		201-199	200- 54	3/15/79
Passage		212-195	209- 53	3/15/79
1st Budget (H. Con. Res. 107)	Mineta, Calif.			
Passage		220-184	211- 50	5/14/79
Conference Report		144-260	108-152	5/23/79
Conference Report		202-196	174- 80	5/24/79
2nd Budget (H. Con. Res. 186, S. Con. Res. 36)	Mineta, Calif.			
Passage		192-213	188- 67	9/19/79
Passage		212-206	212- 52	9/27/79
Chrysler Aid (HR5860)	Blanchard, Mich.	271-136	209- 48	12/18/79

Source: Compiled by the author from task force membership lists.

pected and actual closeness of that vote made an organized task force effort necessary.

Both the presidential and the party leadership considered legislation to place the social security system on a sound financial footing among the top priority items of the 95th Congress. The steep increase in social security taxes that this entailed resulted in a far from popular bill. The social security task force consisted of subgroups charged with responsibility for specific amendments. The Fisher task force was charged with passing an amendment deleting mandatory coverage of federal workers. The overwhelming passage of the amendment makes that task force something of an anomaly. A staffer who worked with the task force explained:

> The preliminary counts didn't show anything like that. That's part of it, but if you'd asked anyone around here, "Is it going to pass?," we'd have said yes. But the crucial thing is, what if it hadn't? We had no social security bill plain and simple. It had to pass and, while most people would say the chances are 9 out of 10, it was not a situation where one was willing to take that 10-percent risk.

The steep increase in social security taxes, which the social security bill mandated, made it politically advisable to pass a tax reduction bill before the 1978 elections. That some bill would pass was never in doubt; the form of the legislation was at issue. Representative Jack Kemp and Senator William Roth had received a great deal of publicity for their proposal to cut income taxes by 30 percent and the leadership knew Kemp would offer the proposal as an amendment to the tax bill. Its obvious appeal, combined with what the leadership saw as drastic consequences if it should pass, led to the establishment of a task force charged specifically with defeating the Kemp amendment.

The reputation of the House Democratic leadership rests, in part, on its success in gaining passage of presidential priorities. It also depends upon the leadership's ability to uphold the reputation of the House as a functioning legislature on less highly visible matters. Two areas that have been especially problematic for the leadership are the biannual conflicts over the budget resolution and the recurrent fight to raise the federal debt ceiling. In a time of high inflation, building winning coalitions in these areas has become increasingly difficult. Both budget resolutions and debt-limit votes offer Congress members nearly irresistible opportunities for "grandstanding." An intensive and well-organized vote-mobilization effort on such legislation has become a prerequisite to success.

The remaining bills on which task forces were established were centrally important to major segments of the Democratic membership. Big city liberals needed the Humphrey-Hawkins Bill and the CETA bill and both seemed to be in deep trouble. Michigan Democrats as well as some others with Chrysler plants in their districts considered the Chrysler bail-out bill "a matter of life or death." Nevertheless this bill, because of its relatively narrow range, fits the criteria least well. The Michigan Democratic delegation, it was reported, "begged" the Speaker to establish a task force.

The technique, then, has generally been restricted to significant legislation which involved the prestige of the leadership and of the party as a collectivity or bills which a major segment of the party badly needed. Restricting the use of task forces to such legislation is vital if the strategy is to contribute to maintaining harmony. Use on a bill perceived by a major segment of the party as threatening its vital interests would undermine the legitimacy of the technique. Instead of teaching members the value of joint action under the aegis of the leadership, it would exacerbate intraparty conflicts.

Task Force Membership

Once the decision to establish a task force has been made, the Speaker appoints the chairman. Leadership staff may suggest one or several names, or he may ask the chairman of the committee, "Who's a bright young guy who would do a good job on this?" As Table 1 shows, the chairman is almost invariably a junior member. All task forces but the first were chaired by members first elected in 1972 or later. Dick Gephardt of Missouri chaired several task forces during his first term.

Junior members are chosen, in part, because they have more time to devote to the task than senior members with heavier committee responsibility. In addition, giving junior members an important job to do and drawing them into the leadership's orbit are integral aspects of the strategy of inclusion. One of the leaders said that an effort was made "to pick members who will stay around, who intend to make a career of the House." The chairman is usually, though not invariably, a member of the originating committee. The members chosen to chair task forces are otherwise distinguished from their contemporaries not by objective characteristics such as prior political experience but by the leaders' perception of them as responsible legislators and as leaders among their peers.

The Speaker frequently suggests some of the members of the task force, but also gives the chairman considerable discretion to include "anyone else you feel comfortable working with." In selecting members, the task force chairman works with the Steering and Policy Committee staff, who provide staff work for the task forces. The selection process has become rather elaborate. Prior to meeting with the task force chairman, the staff prepares a list of potential members with information on each. The information includes past support on similar legislation. Commitment to the legislation in question is, of course, a prerequisite to membership. Within that constraint, attention is paid to getting "regional, seniority and ideological balance."

An examination of task force membership will show the extent to which the aims of including a large number and variety of Democrats in the process and especially of getting junior members involved are actually met. The analysis is based upon membership lists of all 95th Congress task forces and the first two task forces established during the 96th.

Although broad inclusion is an objective of the task force concept, passing the bill at issue is paramount. Task forces must be kept at a working-group size. The 11 task forces under discussion averaged 28.5 members; they ranged from the 16-member CETA task force to the Social Security task force and the Energy Conference task force, each of which had 41 members.

Table 2 shows the extent to which the objective of broad inclusion was reached. During the 95th Congress, 116 members—40.3 percent of the Democrats—served on at least one task force. The table also shows that there was considerable membership overlap among task forces. One in five Democrats served on two or more task forces during the congress; one member, in fact, served on each of the nine in the 95th and on the first two in the 96th.

The strategy of inclusion refers not only to getting a large number of members involved but also to including members from all sections of the party. Overall, the task forces have been fairly representative in terms of region. In both the 95th and 96th Congresses, 74 percent of the Democratic membership was from the North and 26 percent from the South.[1] The average northern membership across the 11 task forces was 76.1 percent; the average southern membership, 23.9 percent. The regional balance varied, but only one task force was clearly unrepresentative: no southerner served on the CETA task force. The unpopularity of the program seems to have made it impossible to find southerners willing to work for the leadership position on the bill.

Table 2 presents data on the proportion of northerners and southerners who served on task forces during the 95th Congress. Southerners were less likely to have served than northern Democrats, but the differences are not massive. The leadership has done reasonably well at including southern Democrats in the coalition-building process via task force membership. In fact, a larger proportion of southern than of northern Democrats served on two or more task forces during the 95th Congress. Clearly, those southerners willing to participate get plenty of opportunity to do so.

The requirement that task force members be committed to passing the bill in question prohibits strict ideological balance just as it makes absolute regional balance somewhat difficult. As one would expect, task force members as a group are more loyal to party than non-task force members. Considering each of the 11 task forces separately, one finds that in each case, members have a higher average party unity score than nonmembers and that this pattern holds for both of the regional groupings.[2] The differences between members and nonmembers are especially great among southern Democrats.

Table 2. Regional Representation on Task Forces, 95th Congress

No. of Task Forces Member Has Served On	All Democrats	Northern	Southern
0	59.7	58.7	62.7
1	19.4	22.1	12.0
2	8.7	6.6	14.7
3 or more	12.2	12.7	10.1
N =	288	213	75

Source: Compiled by the author from task force membership lists.

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[1] Members from Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas and Virginia are classified as southern, all others as northern.

[2] *Congressional Quarterly* party unity scores are used (*Weekly Report*, December 16, 1978, pp. 3450-51). Scores have been adjusted so that absences do not affect the scores.

Table 3 shows that Democrats who served on one task force during the 95th Congress are, as a group, appreciably more loyal than those who served on none. Those serving on two or more are slightly more loyal still. The differences are especially marked for southern Democrats. Nevertheless, task force members are relatively heterogeneous ideologically. Southern Democrats who served on two or more task forces are, as a group, significantly less loyal than northern Democrats who served on none. The leadership does find it possible on specific bills to involve members who otherwise frequently defect. A few Democrats who are generally highly disloyal have served on task forces.

An interesting outgrowth of the strategy of inclusion and an indicator of the decline in committee insulation in the House is the committee makeup of the task force membership. Although the chair usually is a member of the originating committee, a majority of the task force membership is not. If the two energy task forces for which the figures would not be meaningful are excluded,[3] then on the average, only 30.6 percent of task force members are also members of the committee reporting the bill.

The subgroup of members of the Social Security task force charged with passing the Fisher amendment included a number drawn from the Post Office and Civil Service Committee. Since the Fisher amendment dealt with federal workers and thus was related to that committee's jurisdiction, the inclusion of Post Office and Civil Service Committee members on the task force was a way of recognizing their legitimate interest in that aspect of the bill. That case is, however, the exception. On other task forces, members not from the originating committee held positions on a wide variety of committees. They were simply Democrats with an interest in and a willingness to work for the bill in question.

A primary aim of the task force concept in getting junior members involved in the coalition-building process. That objective has clearly been accomplished. During the 95th Congress, 51.7 percent of the Democratic membership had begun its service in 1973 or later; on the average, 69.4 percent of the members of the 9 task forces was drawn from this junior group. Democrats first elected in 1972 through 1976 accounted for 45.8 percent of the party's members in the 96th Congress and for 60.1 percent of the membership of the first two task forces. Freshmen constituted 15.2 percent of the Democratic membership and 18.5 percent of those serving on the two earliest task forces. The first task force of the 96th Congress was set up only a month after Congress convened, yet even in this case freshmen were included; 12.1 percent of the debt limit task force membership consisted of freshmen. On the budget task force established in late spring, freshmen constituted 25 percent of the membership.

Table 4 shows that, during the 95th Congress, slightly over half the Democrats first elected in 1972 or later served on at least one task force. Junior members were also more likely than their senior colleagues to be frequent task force participants.

The leadership has been successful in getting a large proportion of Democrats generally, and of junior members specifically, involved in task force efforts. Membership has been drawn fairly proportionately from both northern and southern

Table 3. Relationship of Party Unity to Frequency of Task Force Membership and Region, 95th Congress

Group	Frequency of Task Force Membership		
	None	One	Two or More
All Democrats	68.4	78.4	79.6
N =	(171)	(56)	(60)
Northern	77.0	81.2	84.9
N =	(124)	(47)	(41)
Southern	45.6	63.8	68.2
N =	(47)	(9)	(19)

Sources: Compiled by the author from task force membership lists and from *Congressional Quarterly*'s party unity scores, *Weekly Report,* December 16, 1978, pp. 3450-51. Scores have been adjusted so that absences do not affect the scores.

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[3] The energy bill was referred to five standing committees and to the Ad Hoc Energy Committee. Of the members of the first energy task force, 64.3 percent were from Ways and Means or Commerce, the two committees with major jurisdiction, and 44.4 percent from the Ad Hoc Committee. On the second energy task force, 22 percent were from Ways and Means or Commerce and 26.8 percent from the ad hoc panel.

Table 4. Relationship Between Seniority and Frequency of Task Force Membership, 95th Congress

No. of Task Forces Member Has Served On	Senior Members	Junior Members*
0	70.5	49.7
1	16.5	22.1
2	5.8	11.4
3 or more	7.2	16.8
N =	139	149

Source: Compiled by the author from task force membership lists.

*First elected in 1972 or later.

members and, to some extent, from conservatives as well as liberals. From the leadership's point of view, broad inclusion is important because it contributes to the successful performance of both central leadership functions. A broadly representative task force is more likely to be successful in passing the legislation at issue because it includes members with ties to all segments of the party. Giving a large number and a wide variety of Democrats "a piece of the action" but under the aegis of leadership, it is hoped, will foster norms of intraparty cooperation.

The Task Force in Action

The broad strategy to be pursued on a bill often evolves out of what has happened during committee deliberations. On a bill important enough to warrant a task force effort, the party leadership will meet with key committee leaders to discuss strategy. According to a leadership aide, "The chairman of the task force is centrally involved."

Strategy, of course, varies. A participant explained that on the first budget resolution in 1979, "We had made up a careful package in committee. The strategy on the floor was to sell it as a carefully crafted package and beat back all [noncommittee sponsored] amendments." A package amendment adding funds for various social programs, as well as for two destroyers ordered and then canceled by Iran, was brought to the floor by the committee. According to a senior staff member, "Bringing the additions in the Simon amendment to the floor rather than adding them in committee was a conscious strategy. The purpose was to reinforce the notion of a package."

A similar strategy was used on the energy bill. It too was sold as a carefully crafted and balanced package. On Humphrey-Hawkins, it was decided that intense effort should go into defeating amendments because, in the words of a participant: "All you had to do was have one big bad amendment and the bill was finished. Because too many didn't want to vote for it anyway that, if you gave them a chance to say, well, it's got this bad amendment on it now—no bill. You had to win every one." The social security bill reported out of Ways and Means made participation by government workers mandatory, a provision the leadership considered politically untenable. Consequently, the leadership decided that an amendment deleting the provision would be offered on the floor.

Typically, the first task force meeting begins with a "pep talk" by the Speaker. The Speaker, the majority leader and the majority whip are ex officio members of every task force. Their frequency of attendance at meetings depends upon just how much trouble they believe the bill to be in, personal interest, and the absence of conflicting commitments. The Speaker was reported to have attended every meeting of the budget resolution task force in the spring of 1979. When the leaders cannot themselves attend, they send top aides in their stead.

Usually, the task force will begin with a whip count done by the whip system. Because, in the post-reform House, many amendments are offered, the counting process has become much more complicated. The whip system conducts polls on many bills and, consequently, the questions asked by the whips on a given bill must be restricted in number and fairly simple. According to a leadership aide:

> Task forces are especially helpful if the issues involved are very complicated substantively and you really need to run an information campaign to inform members. And where there are lots of possible permutations and you need to really get a feel for the members—that is, what they'll do under a variety of circumstances.

At the first meeting, strategy will be discussed and the task force members will be assigned a list of Democrats "they will be responsible for working [on] throughout" consideration of the bill. A task force chairman explained the process in a case in which no whip poll had been conducted:

> It was just a question of dividing up the states and assigning those members to some specific person. For instance, in the state of California, we had three people on the task force; from New York, I believe we had two. And from Texas, of course, we had Mr. Wright as well ______. Mr. ______ took Pennsylvania and also West Virginia because he's such a good friend of ______ and ______. So we were able to divide among the members of the task force all the Democrats and so then we had to decide what was the critical issue. We knew we were going to have a series of amendments, but I didn't want at the start to

> have to give 7 or 10 questions to a person to have to ask and so, in our discussion in the task force, we reduced it finally to two.

When the zone whips conduct a whip poll, they are simply counting. For task force members, counting and persuasion are always combined. Consequently, the process differs only slightly if the task force begins with a whip poll. According to a leadership figure:

> We'll kick around arguments—discuss which ones will be good with which members. Then we'll go through the whip poll and assign names. The list of names will be read, and a member will say, "I'll take that one." Or maybe no one will volunteer and someone will say, "______, why don't you take him?" and ______ will say, "I don't want to." But finally all the names will be assigned.

Or, as a senior staffer expressed it, "There will be candid discussion until they pin down where the trigger is."

Region and seniority are the primary bases for assigning names because, according to the participants questioned, they are the primary bases upon which friendships form. The strong belief that members will be more successful at persuading their contemporaries provides an additional reason for the heavily junior membership on the task forces. As in other vote-mobilization efforts, finding the "trigger" may mean going outside the House. According to a staffer, one of the ways of "motivating" members is via outside groups: "To get ______'s vote, you get Kenny Young [of the AFL-CIO] to call him."

The task force will, when possible, work with friendly interest groups. The chairman or the Steering and Policy Committee staffer working with the tassk force may meet with interest-group representatives to coordinate strategy. Such meetings on the Humphrey-Hawkins Bill involved about 40 interest groups. Information on voting intentions which such groups have gathered allows the task force to double-check its data on Democrats and to get some reading on Republican votes.

Although the results of whip polls are never given out, some trading of information with the Carter administration did occur and Bill Cable, White House liaison with the House, was sometimes invited to task force meetings. But, according to a leadership aide: "[The White House liaison people] will not usually attend the strategy meetings. They will talk to the Speaker separately. They are usually content to leave the details to the House leadership. They will lobby individual members themselves."

The arguments used by task force members as they attempt to persuade their colleagues vary. On the first budget resolution in 1979, the carefully balanced nature of the committee product and party pride were emphasized. A member explained: "Our strategy was that this was basically a majority party document. This was written by the budget committee majority, and we ought not let the minority party dictate what the majority party is going to pass." On debt-limit votes: "You ridicule the opposition arguments, minimize the electoral impact and appeal to their sense of responsibility."

Arguments about the substance of the matter at issue generally play a major role. A budget task force member commented: "There were some of these amendments that were very appealing considering the Prop. 13 mentality. So we had to really explain to them why we ought not to be succumbing to this kind of thing." The most persuasive argument varies from member to member. A participant remarked:

> It's very individualistic—you go by instinct. Members have different motivations. Some you have to puff up. ______: you tell him he'll get credit, that he's a leader. ______: you persuade him he's being patriotic. Others, you persuade them it's the devious thing to do politically. Some you have to convince it's the liberal thing to do; others, that it's conservative.

When a member cannot be persuaded to support the leadership position across the board, the task force will work for partial support. A participant describes this strategy as used on the first budget resolution in 1979:

> Now, part of that frankly was sold on the basis of, "If you're going to vote no on final passage of the budget resolution, help us out at this point by voting no on Holt-Regula or any other Republican substitute." And that argument seemed to hold with a lot of people, that they were willing to help us out by voting no on any Republican substitute, even if they were going to vote no on the resolution. The case of Texas, as an example. Everyone of them voted no on both Latta and Holt-Regula and that really helped us out immensely.

Several days after the first meeting, the task force will meet again and discuss the results. The names of Democrats now committed to voting with the leadership are marked off, and the remaining names may be reshuffled. The task force may go through this process several times. The top leaders always take some names. One of the purposes of setting up a task force, however, is to minimize the number of members whom the top leaders must persuade. They are busy and their personal appeals for votes are most likely to be effective if not made too frequently. Consequently, an attempt is made to save the top leadership, especially the Speaker, for "the tough ones."

The floor manager of the legislation is always invited to task force meetings. His attendance seems to depend primarily upon time pressures. Asked about the relationship among the party leadership, the floor manager and the task force, a top leadership figure said, "The leadership is the general staff, the floor manager, the battlefield general and the task force, the company commanders." The floor manager and the task force chairmen do seem to work together smoothly. Generally, floor managers seem to appreciate the help the task force provides, and the likely working relationship between the task force chairman and the floor manager is probably an important consideration in the choice of the task force chairman. A task force chairman, discussing his working relationship with the committee chairman, said:

> Well, of course, Chairman ______ sat in on every one of these sessions. So he saw whatever strategy we were developing within the task force and could say, "Hey, I don't think you ought to be doing that." Whenever I had a chance before going into the meetings, I'd try to check with him on certain things. I'd say, "______, this is what we're thinking about doing; what do you think?" He'd say yes or no.

Much of the task force's activity takes place during the period of floor consideration. Because numerous amendments are now proposed to most controversial bills, the floor stage has become much less predictable. It is not possible to count every amendment before the bill reaches the floor, both because of the numbers involved and because some will be offered without prior warning. Amendments can be extremely complicated and their actual effects obscure. Consequently, although vote-mobilization efforts in the days before a bill reaches the floor are essential, holding the coalition together during floor consideration and warding off amendments which could seriously damage the bill are equally crucial.

The task force chairman stays on the floor during the entire period of consideration. Members are, at minimum, expected to be on the floor during each roll call. Standing at the doors, task force members hand out information on the amendment pending. They are expected to keep track of the people assigned to them and work on them during a roll call if necessary. The task force chairman and some of the members "cluster around the monitor and look at the vote state by state. If they find someone has voted wrong, they will chase him down and work on him. Even if he comes to the floor, votes and then runs, he's got to be around somewhere and they will find him." Other task force members are charged with keeping pocket votes—those Democrats who have promised to vote with the leadership if they are needed—from leaving the floor. A female task force member was reported to have kept her assignee from leaving the floor by "practically sitting in his lap. He would have had to be a real boor to have gotten up."

The top leadership will also stay on the floor during important votes. This gives the leaders an opportunity to engage in some last minute persuasion and their presence signals the importance they place upon the vote. When the Speaker stands at the monitor examining how members voted, Democrats know "he's really watching this one. He wants it."

The unpredictability of events at the floor stage puts a premium on being able to respond quickly. When things go wrong, remedial action must often be taken immediately to prevent the supportive coalition from disintegrating. Early in the consideration of the budget resolution in May 1979 an amendment cutting $250 million of targeted fiscal assistance passed. If the decision had been allowed to stand, it would have split the coalition supporting the resolution and made the adoption of other major amendments more likely. A participant explained:

> We were using the argument, "Look, this is a very carefully written budget resolution to make sure we're satisfying the agriculturalists, the urban community, the rural, the East, the West, the whole thing, and don't monkey with this very tenuous balance we have." As soon as that $250 million was taken out, you could hear people saying, "Well, the hell with it; I'm not going to vote for this damn thing."

The committee chairman immediately met with a group of his members and with Budget Committee staff to draft a substitute which would restore $200 million. At this point an organized working group was crucial. The task force chair explained:

> Once we had [the Solarz amendment], we got to work. We recalled everybody and said "All right, here's what's happening, we need the votes on this one. Make sure the people you have as your responsibility vote yes on the Solarz amendment." So, within two days we came right back and got that thing reversed and got the $200 million added in.

Because of the number of amendments offered and the availability of recorded votes, an effective method of providing information to members has become necessary. Members simply will not blindly follow the leadership over and over again, especially when their votes are public. Thus the development of the "bullet"—a mimeographed, one-page summary of an amendment. Developed by the Steering and Policy Committee staffer working with the Humphrey-Hawkins task force, bullets have now become a regular part of the process. For anticipated amendments, the committee staff will prepare bullets prior to floor consideration. On amendments offered without warning, bullets will have to be written and duplicated on the spot. The committee and the Steering and Policy staff work together, often using an Appropriations Committee room close to the House floor.

Bullets are widely believed to be effective. A staffer said:

> On bad amendments, you need something to give the members. You've got to give them a rationale for voting against. Otherwise, you can have the "lurch" effect. If they have nothing and they go in, they'll look at the vote totals and may be stampeded.

He explained that many amendments are very complex:

> There may be only three or four or five people in the House—on our side of the House—who understand what some of these amendments really do, and if they don't have a way to reach the whole Democratic caucus, we're in trouble. On the ______ bill, every time we didn't have one we lost.

A task force chairman explained:

> You know these members are coming in through the door and they have essentially 7 to 2 minutes or even a minute to vote on an amendment. They're running in and asking, "What is it?" and you can't explain a four-page amendment, changing aggregates and everything else and subfunctions. So you say, "This is the Grassley of Iowa amendment, vote no," and hand them the sheet. It points out as succinctly as possible what's in it—that it adds $380 million to the deficit—that it cuts out all programs for babies.

If the bill is on the floor for more than one day—an increasingly frequent occurrence—the task force will meet between sessions. Committee staff will brief the members on amendments expected, arguments to be used will be discussed, voting records to that point will be analyzed. The task force may decide that on a given amendment "20 members are targets and everyone will work on them."

Sometimes the current situation will suggest new tactics. Republicans could not agree upon one substitute to the first budget resolution in 1979; consequently, two alternative plans were introduced and brought to a vote. This allowed the leadership and the task force to persuade those Democrats who believed they had to go on record as favoring budget cuts to vote for one or the other, but not both, and to make sure such members divided their votes between the two alternatives evenly so that both would fail. In fact, 33 Democrats did so vote and neither Republican alternative passed.

In addition to orchestrating persuasion efforts, the task force chairman sometimes provides advice on floor strategy. Whether to finish a bill even though it means staying very late or to pull a bill off the floor after one or two amendments have been lost can make a major difference. On important bills, the floor manager and the leadership make such decisions; but the task force chairman can provide another judgment. According to a staffer:

> On ______, we had a bad problem and the task force chairman insisted that we leave the floor and saved us. We went back and we had a day or a day and a half to regroup and, in this case, it was the presence of the task force that pushed us off the floor. The committee sort of wanted to stay. It was the task force chairman who made the difference in getting off the floor. He said, "Hey, look, I know you'd like to stay and finish the bill. Well, I happen to think that if we stay here another five minutes, we're not going to have a bill to manage any more." It wasn't quite that blunt, but it was along these lines, and that got us off the floor. . . . A task force chairman provides another judgment as well as that of the committee members who are often so close they don't see it too well, whereas the task force chairman, though he's pretty close himself, sometimes senses the mood a little better and can determine a little better whether to quit or stay.

By performing tasks the formal leadership group is too small to carry out alone, the task force does increase the probability of floor success. The task force can explain the provisions of a complicated bill on a member-to-member basis; it can get a detailed and precise count of Democrats' voting intentions and engage in face-to-face persuasion efforts with a large proportion of the Democratic membership. It has the organization and the numbers to respond quickly to the unexpected floor developments.

Evaluation of Task Force Success

One of the leadership's objectives in setting up a task force is passing the legislation in question. According to that criterion, all task force efforts have been successful. Because the bills on which task forces worked were highly controversial, winning was seldom easy and victory not always clear-cut. In several cases, success came after an initial defeat and, in some, compromise at the post-committee stage was necessary. In at least one case, the bill reported by the committee was not at all to the Speaker's liking. Nevertheless, all the legislation on which task forces worked was passed without the addition of crippling amendments at the floor stage. The leaders believe the task force device has proven itself as a coalition-building strategy well suited to the post-reform House.

From the beginning, the Speaker saw the task force strategy as having a benefit beyond that of passing a particular bill. "Give me a frustrated guy," the Speaker says, "and I'll give him a job to do and he becomes a 'Tip O'Neill man' " (*New York Times*, June 4, 1979). Rather than attemption to dampen the high rate of rank-and-file participation—an enterprise almost certainly doomed to failure—the leadership seeks to direct it into productive channels. In the process of participating in leadership-directed efforts, it is hoped that Democratic members, particularly the junior ones, will learn the need for "followership" and for some joint action under the aegis of the party.

Participants believe that task forces have been useful in socializing junior members. Working on a task force gives them an understanding of the problems of the leadership and, in some cases, results in an identification with the leadership. One participant explained:

> It's helped the leadership to get to know the new members and the new members to get to know the leadership. And it's certainly helped the new members who I've spoken to—my personal friends—to understand the need for leadership and followership. I think the guys who have served on task forces know a lot more about the need for a party structure with some loyalty than those who haven't.

According to another participant:

> The task force members; particularly the chairmen, got an awareness of the problems of leadership and a feeling of meaningful participation that paid dividends on later legislation, as they felt some obligation to cooperate with other task forces. Many of the task force members, like ______, became emotionally involved with the leadership.

As the socializing effect became clearer, the task force membership selection process changed. There is now a conscious effort to include Democrats specifically for the effect membership may have on their attitudes towards intraparty cooperation. As one observer put it:

> Initially—though not necessarily totally consciously—we chose those people who might be most amenable to working in the same philosophical mold as the man who appointed them, the chairman, and the Speaker, and the leadership as a whole. Though not totally—I mean that's not the only reason you choose somebody. But I think what happened was that we realized after a couple of task forces had gone by that we were doing that to an extent greater than we should, and have since then changed the nature of the invitation process a little bit to try and broaden it and bring in people who we're not that used to working with, so to speak. And there's been some discussion about that, and there is a conscious effort currently being made to broaden it, to reach into places we haven't reached into before. You can't always do it because the places you haven't reached into before may be philosophically totally opposed to your bill. But we try, and we're trying pretty hard.

Increased awareness of the socialization benefit of task force membership seems by the 96th Congress to have led to another form of reaching out in making up task force membership lists. Every one of the five new Democratic members of the Ways and Means Committee was included in the debt-limit task force in the spring of 1979. Of the eight new Budget Committee Democrats, six served on the budget-resolution task force; deep policy disagreements precluded the inclusion of the remaining two. This appears to represent a conscious attempt to draw new committee members into the leadership's orbit, to accustom them to working with the leadership on major bills within their committee's jurisdiction.

The transference of techniques learned working on a task force to other situations is still another benefit. A participant explained:

> Members who had previously served on task forces undertook to do on the part of their committee that which previously they had done on task forces. In other words, they got some new tools, they saw that they worked, and now they're carrying them back into their work for their committee. I think that's fantastic. You see members of the committee taking sheets of paper [explaining amendments] to the doors. It seems to be becoming more the way you handle things, a more routine kind of thing and I think that's just great.

Conclusion

At the core of successful leadership is satisfying the expectations of followers. Consequently, appropriate strategies change when members' expectations and other aspects of the environment which bear upon the satisfaction of those expectations change. In a House dominated by powerful committee chairmen, one in which junior members did not expect to play a significant legislative role and even senior members largely restricted their activity to their own committees' legislation, a strategy of inclusion was unnecessary and inappropriate. The post-reform House, in which rules allow and member expectations dictate wide participation, requires such a strategy. The use of task forces increases the probability of passing major legislation on the now highly unpredictable House floor. By satisfying members' expectations of significant participation in the process, it contributes to "keeping peace in the family." The leaders believe that service on task forces, in addition, teaches members the value of cooperative behavior.

As the leaders themselves recognize, their success depends not only upon what they do but also upon forces in the environment outside their control. Their ability to satisfy members' expectations about legislative results depends upon some core consensus within the Democratic party. No member expects the leadership to tailor its legislative priority list to his or her individual preferences; all realize the leadership must be responsive to the diversity of interests within the House Democratic membership and to significant actors outside the chamber. The legislative record as a whole must, however, be reasonably satisfactory to a substantial majority of the party membership. The failure to produce legislation that satisfies members' reelection needs and their policy goals cannot be fully compensated for by internal rewards. If, as now seems possible, the Democratic consensus on core elements of social welfare and economic policy totally disintegrates, the leaders will be caught in a conundrum. To the extent that they are successful at building winning coalitions in response to the demands of some set of members, they may so dissatisfy other members that their very success will be extremely expensive in terms of "keeping peace in the family." Each coalition-building effort may exacerbate intraparty conflicts, discourage cooperative behavior, and consequently, make future coalition-building success less likely. The continued adequacy of the strategy of inclusion and its success at fostering norms of intraparty cooperation, thus, depend upon the reestablishment of some consensus within the Democratic party.

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Institutional Context and Leadership Style: The House from Cannon to Rayburn

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This article deals with the transition in House leadership from Cannon to Rayburn. The transition involved moving from a hierarchical pattern of leadership to a bargaining pattern. In accounting for this transition, we argue that it is the institutional context of the House that determines leadership power and style. Moreover, we argue that there is no straightforward relationship between leadership style and effectiveness; rather, style and effectiveness are contingent or situational. We conclude that the impact of institutional context on leadership behavior is itself primarily determined by party strength. When party strength is high, power is concentrated and leaders are task- or goal-oriented, whereas when party strength is low, power is dispersed and leaders will be oriented to bargaining and maintaining relationships.

Leadership is an aspect of social life which has been extensively studied in a variety of institutional or organizational settings (Miner, 1980). Yet it remains a topic in which our intellectual grasp falls far short of our pragmatic sense of the impacts leaders have on organizational operations and performance.

This is as true, if not more true, of Congress than of other organizations. Here too analysts are perplexed by the difficulties of conceptualizing key variables, treating highly transient and idiosyncratic personal factors, and identifying relationships amidst a maze of interactive effects. Moreover, the task is rendered even more complex by the highly politicized character of the Congress as compared with most of the organizational contexts in which leadership has been studied.

This is not to say that knowledge and understanding of congressional leadership have remained static. Peabody (1976), Jones (1968), Ripley (1967), Polsby (1969), Manley (1969), Hinckley (1970), and Nelson (1977) have all done instructive and insightful work. Nonetheless, our grip on the topic is as yet not firm; we continue to lack a developed sense of what we should be looking at and how to proceed.

The purpose of this article is to aid in remedying this deficiency. It is premised on two key assumptions. First, the study of leadership requires comparative evidence regarding both behavior and contexts. Hence our use of history as a laboratory and our choice of Cannon and Rayburn as focal points of analysis. Second, the study of leadership requires abstract or analytical concepts to aid in formulating and testing important relationships. Hence our historical analysis relies on several broad concepts and relationships, drawn both from organization theory and from recent work on the operation of the Congress.

In sum, though this article deals with the transition from Cannon to Rayburn, its main objective is not to fill in the historical record. Its primary goals are rather to bring evidence and analysis to bear to improve our understanding of the key determinants and underlying dynamics of congressional leadership, and to suggest a set of propositions or hypotheses that can serve as a basis for more focused and elaborate forms of investigation and theory building.

The House Under Czar Rule

The legacy the House of the nineteenth century left to that of the twentieth was a set of rules which placed the majority firmly in control of the House and centralized power in the hands of the Speaker as the agent of this majority. It was this legacy the House rejected when it revolted against the Speaker in 1910. In so doing it not only stripped the Speaker of many of his important powers, but also paved the way for a metamorphosis in the nature of the House as a political institution.

The Speaker and the House. It was with good reason that Speakers of the House in the years between 1890 and 1910 were often referred to as czars (Galloway, 1961, pp. 134-36). The Speaker appointed the committees. He served as the chairman of and had unchallengeable control over the Rules Committee (Brown, 1922, pp. 87-90). He had great, though not unlimited, discretion over the recognition of members desiring to call business off the calendars, to make motions, or to address the House, and absolute discretion over the recognition of motions for unanimous consent and suspension of the rules (Chiu, 1928, pp. 175-97).

These prerogatives gave the Speaker great power to control outcomes in the House. At the committee stage, those who had received prized assignments from the Speaker naturally felt a sense of gratitude and obligation to him. Those who desired a change in assignment knew full well that their chances of advancement depended on the good graces of the Speaker. Conversely, since in this age seniority was far from as sacrosanct as it is today, members were also aware that to alienate the Speaker was to risk loss of a chairmanship, an assignment, or rank on a committee (Abram and Cooper, 1968; Polsby et al., 1969).

Nor was the appointment power the Speaker's only source of leverage in controlling outcomes at the committee stage. Members of any particular committee were also disposed to cooperate with the Speaker because of the vast array of rewards and sanctions his position in the House bestowed on him. For example, the Speaker could provide access to the floor by granting a rule or recognizing a motion to suspend the rules; he could lend invaluable assistance in getting a project included in a bill or in getting a bill out of committee. Moreover, if all the rewards and sanctions at the disposal of the Speaker still proved to be insufficient, there was yet another factor that discouraged opposition at the committee stage. The plain fact was that to oppose the Speaker would in all probability be fruitless. If a committee refused to report a bill the Speaker wanted reported, the Speaker could pry the bill out of committee either through use of the Rules Committee or suspension of the rules. Similarly, if a committee reported a bill the Speaker opposed, the bill had little chance of reaching the floor. The power of the Speaker was such that he could obstruct the consideration of any bill he did not want considered. Given the various and potent types of leverage the Speaker possessed, it is not surprising that in this period committee chairmen took their cue from the Speaker regarding which bills they would report and that Speakers referred to the committee chairmen as their "cabinet" (Busby, 1927, p. 219).

As for action on the floor, here too the Speaker's prerogatives under the rules gave him great power. A number of factors combined to give him control over the agenda of the House. Through use of the Rules Committee and other privileged committees, he could interrupt the regular order of business either to give priority to a bill he wanted considered or to block a bill he opposed.[1] He could use unanimous consent and suspension of the rules to give access to the floor to bills he favored and could deny the use of these procedures to bills he opposed. In addition, his discretion in the recognition of motions calling bills up for consideration was a source of leverage.

The Speaker's ability to control the agenda, however, stemmed not merely from his powers of repression, but also from the necessity of relying upon him if the House was to reach the bills it wanted to consider. The volume of legislation before the House made it exceedingly cumbersome to follow the involved order of business set forth in the rules. As a result, the House did not insist on following its regular order. Indeed, the points in the order where committee members could call business off the calendars were usually not reached. Instead, the House relied on the Speaker to bring bills up for its consideration and to determine the time of consideration through the use of privileged reports and special procedures, such as unanimous consent. In short, then, both because of the powers of the Speaker over the agenda and the unwieldiness of proceeding according to the regular order, the House gave the Speaker even more power over the agenda than his power under the rules bestowed on him.[2]

A second aspect of the Speaker's power over floor decisions concerned his ability to control considerations on the floor. Here again, his command of the Rules Committee and his power as presiding officer gave him considerable leverage over floor debate and dilatory tactics. In addition, many of the same rewards and sanctions at the Speaker's disposal for controlling committee decisions could also be used to control floor decisions. Especially for members of the majority party, to oppose the Speaker was to risk the loss of his assistance in matters of vital importance to one's constituency and therefore also to impair one's chances of reelection. Moreover, through bestowing favors over a number of years, the Speaker could build up a substantial fund of credits, credits which could then be expended as needed to secure the cooperation of members in his debt. Thus, the ability of the Speaker to control decisions on the floor and in committee stemmed not only from the immediate impacts the exercise of his formal powers involved, but also from their long-run dividends.

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[1] Aside from the Rules Committee, nine other committees were privileged to report at any time on certain matters. In addition, the eight committees charged with general appropriation bills also were privileged to report at any time on such bills. *Hinds' Precedents*, Vol. 4, Sec. 4621. It should also be remembered that the unanimous consent procedure at this time consisted simply of motions on the floor to that effect. The rule providing for a Consent Calendar was not adopted until 1909 (Chiu, 1928, pp. 179-88).

[2] The total control the Speaker exercised over the consideration of minor legislation thus resulted from the combined impact of the lack of a regular order of business and the need to secure recognition by the Speaker to use either unanimous consent or suspension to bring constituency bills to the floor (Chiu, 1928, pp. 228-30).

The Speaker as Party Chief. To complement his prerogatives under the rules, the Speaker possessed another source of power that was equally significant. In placing a potent array of rewards and sanctions at his disposal, the rules did of course provide him with considerable leverage. However, the Speaker's ability to command majority support in committee and on the floor was materially aided by another factor: party discipline.

In an age when party regularity is far from an overriding consideration, it is difficult to appreciate how important party was in the House at the turn of the century. In this period the great majority of members in both parties subscribed to the doctrines of party government. Representative government was seen to depend on the existence of a responsible majority which had the power to rule and which, as a result, could be held accountable for performance (Jones, 1968). Only under such conditions, it was believed, could the people effect their wishes. The individual representative was thought to be elected on the basis of a party's platform and was therefore regarded to have an obligation to support party positions, even against personal convictions or desires.

The fact that members in this age thought and spoke in terms of the doctrines of party government had more than rhetorical importance. Party government served as the main justification for vesting great power in the Speaker and permitting him to play the role of Czar. Though the Democrats never fully accepted the proposition and ended by rejecting it, a cardinal tenet of Republican faith was that rule by a responsible majority party required centralizing organizational power in the Speaker.

The Speaker's position as head of his party thus also provided him with an important source of leverage. It is true, of course, that even in this period many issues were not treated as party issues. Nonetheless, most important issues were regarded as matters on which the party as a whole should stand together. In such a context the Speaker derived considerable power from his position as party chief. Initiative in the definition of party policies belonged to him. Moreover, if he could not win the support of all elements in the party, he had at his disposal a powerful mechanism for enforcing adherence to his wishes—the caucus. Through a binding vote in the caucus, he could oblige the opposition to support his policy positions out of party loyalty (Brown, 1922, pp. 92-93, 100, 161-62; and Wilson, 1961, p. 96). In short, the Speaker could rule the House through the force of party discipline. As long as the bonds of party held taut, he had only to command the support of a majority of his party to command a majority in the House.

We may conclude, then, that the House of Reed and Cannon contained a highly centralized power structure with control resting essentially in the hands of the Speaker. The key to the Speaker's power lay not simply in his prerogatives under rules, nor in his position as party chief, but rather in the manner in which these two sources of leverage reinforced each other (Cooper, 1970). The existence of a stable party majority insured the Speaker's ability to implement his formal powers and gave him a degree of maneuverability and control that the rules alone could not give him. Similarly, the rewards and sanctions the rules placed in the Speaker's hands gave party regularity a degree of priority it would not have possessed if it had rested merely on the extent of agreement among party members or their devotion to the doctrines of party government. Speakers could therefore quite appropriately refer to committee chairmen as their "cabinet." During this period the committee and party systems were blended to an extremely high degree. The Speaker was both the party leader and the chairman of the Rules Committee. The majority leader was the chairman of the Ways and Means or Appropriations Committee and, with the start of the whip system in 1897, the whip was chairman of Judiciary or a top member of Ways and Means. Unlike the contemporary House where party leaders and committee chairs are separate, committee and party leaders were one and the same. Tensions between the two systems were accordingly greatly reduced. Whereas in the contemporary House committee chairs often have low party support scores, such was not the case at the turn of the century. It was thus not a mere figure of speech to refer to committee chairs as a "cabinet." Both structurally and behaviorally, committee and party leaders were a cabinet (Brady, 1973).

The Bases of Czar Rule. We have argued that the interaction of the Speaker's formal powers and the strength of party resulted in a centralized form of leadership—Czar rule. This can be shown empirically. Data exist which strongly buttress the

Table 1. Czar Rule and Levels of Party Voting in the House (1881-1921)

Congress	Year	Percentage Party Votes	Majority Party	Centralized Leadership
47	1881	16.6	Republican	No
48	1883	7.0	Democratic	No
49	1885	15.5	Democratic	No
50	1887	8.7	Democratic	No
51	1889	42.5	Republican	Yes
52	1891	4.2	Democratic	No
53	1893	6.1	Democratic	No
54	1895	24.8	Republican	Yes
55	1897	50.2	Republican	Yes
56	1899	49.8	Republican	Yes
57	1901	38.9	Republican	Yes
58	1903	64.4	Republican	Yes
59	1905	34.6	Republican	Yes
60	1907	26.3	Republican	Yes
61	1909	29.4	Republican	Yes/No
62	1911	23.0	Democratic	No
63	1913	19.9	Democratic	No
64	1915	21.7	Democratic	No
65	1917	9.4	Democratic	No
66	1919	14.9	Republican	No

Source: David Brady and Phillip Althoff, "Party Voting in the U.S. House of Representatives, 1890-1910: Elements of a Responsible Party System," *Journal of Politics* 36 (1974): 753-75.

argument we have made deductively on the basis of the historical record.

First, if party strength functioned as a key ingredient of Czar rule, then levels of party voting should be markedly higher in congresses with centralized leadership. Table 1 presents data on party votes (90 percent of one party versus 90 percent of the other) for congresses from 1881-1921. It thus includes "Czar rule" congresses in which the Speaker possessed the formal powers described above and a ready ability to use the caucus, and congresses in which one or more of these sources of leverage was absent.

Table 1 shows a strong connection between centralized leadership power and levels of party voting. In the period from 1881 to 1899 party voting scores attained levels of 25 percent or more only in the three congresses in which centralized leadership power existed. In the period from 1899 to 1921 party voting did not drop below 25 percent until after the 1910 revolt against the Speaker, and then fell to below 10 percent in 1917 for the first time in a quarter-century. To further substantiate our argument, we ran a point biserial analysis of the data. This statistic is used when the data are dichotomous and is appropriate for Table 1, given a distinction between centralized and non-centralized leadership power. The point bi-serial for this data set was a striking .89, demonstrating the degree to which levels of party voting can be seen as associated with concentrated leadership power.

Second, if an interactive relationship exists between concentrated formal power and party strength, then party strength must have its own sources of determination and impact. Indeed, in our view the causal impact of party strength on the distribution of power in the House is of primary importance. For the Speaker to have the power involved in Czar rule, a majority of the House members had to agree to bestow such power. Since the House is organized on the basis of party and since during this period the Republicans were usually in the majority, it was their potential for group cohesion and loyalty that established the conditions for centralized leadership. In short, the vehicle through which centralized leadership developed was the congressional Republican party (Brown, 1922, pp. 71-126). The rationale underlying this development was that without party government the industrial gains of the late nineteenth century would have been negated by congressional Democrats.

However, to sustain the role and significance we have accorded party strength, we must be able to identify and demonstrate independent sources of determination. In this respect it may be noted that the development of strong party systems in Europe and Britain is associated with the rise of leftist-socialist parties and that in the United States those states where the parties represent polarized constituencies have high levels of party voting. Our argument is therefore that the fundamental bases of party strength at the turn of the century, as in all periods of our history, are largely external, that party strength is rooted in polarized constituency configurations.

In order to ascertain the constituency bases of the congressional parties as well as the differences between them, we calculated the degree to which each congressional party represented agricultural as opposed to industrial districts. For example, in the 55th House (1897-1899), 69 percent of the Democrats and 26 percent of the Republicans represented agricultural districts, that is, districts where the ratio of farms to industrial workers was at least three to one. Thus the difference between the parties was 43 percentage points. This differential was computed for the 47th through the 66th Houses (1881-1921) and serves as a measure of electoral polarization. The specific hypothesis is that there should be a strong relationship between polarization and party voting. Table 2 confirms the hypothesis. When polarization was high, so too was party voting. Conversely, in Houses where the differential was less than 20, that is, where the parties were less polarized, the proportion of party votes did not rise above 20 percent and dropped to as low as 4.2 percent. However, perhaps the best overall statistic is Pearson's r, which is .81 for the two variables presented in Table 2.

Table 2. Polarization of Parties and Party Voting in the House (1887-1921)

Congress	Year	Differential	Party Votes
47	1881	36	16.6
48	1883	15	7.0
49	1885	25	15.5
50	1887	24	8.7
51	1889	41	42.5
52	1891	19	4.2
53	1893	22	6.1
54	1895	36	24.8
55	1897	43	50.2
56	1899	33	49.8
57	1901	35	38.9
58	1903	39	64.4
59	1905	41	34.6
60	1907	36	26.3
61	1909	31	29.4
62	1911	24	23.0
63	1913	12	19.9
64	1915	11	21.7
65	1917	14	9.4
66	1919	18	14.9

Source: David Brady and Phillip Althoff, "Party Voting in the U.S. House of Representatives, 1890-1910: Elements of a Responsible Party System," *Journal of Politics* 36 (1974): 753-75.

The data also show that during the period from the realignment of 1894-96 to approximately the election of Woodrow Wilson (the 54th through the 61st House), the parties remained polarized and levels of party voting remained high. On the other hand, during the "period of no decision" (47th through the 53rd Houses), the degree of polarization fluctuated, and levels of party voting varied accordingly. Similarly, after 1908 the congressional parties became less polarized as the Democratic party became more competitive in industrial districts, and party voting in the House again declined.

In sum, then, it is critical to note the correspondence between a polarized electoral system and a highly centralized leadership structure. Though the Speaker's formal powers reinforced party strength, the polarized electoral bases of the party system provided an indispensable platform for Czar rule. Thus, when electoral polarization began to decline, the centralized internal structure also began to come apart (Brady et al., 1979).

The House from Cannon to Rayburn

Despite its power, the system of Czar rule could not maintain itself. It proved to be too rigid a system to accommodate the factional tendencies in the party system. During the early years of the twentieth century, economic and social ferment in the Midwest and West brought to Congress a group of young Republicans passionately devoted to enacting a whole series of reform measures. Cannon used his power as Speaker and party chief to contain and frustrate the desires of these members. In so doing, he soon aroused their enmity not merely for his policies but also for the whole system of power then prevalent in the House (Jones, 1968).

The Revolt against the Speaker. Though the number of Insurgent Republicans in the House was never large, by 1909 their strength in combination with the Democrats was sufficient to bring the revolt to a successful conclusion. The first step came in 1909 with the establishment of a Consent Calendar and a call of the committees every Wednesday to take up business on the House or Union Calendars. At this time more sweeping change was prevented by the defection of a group of conservative Democrats (Hechler, 1940, pp. 42-63). The next year, however, the Insurgent Republican-Democratic coalition gained a decisive victory. On March 19, 1910, after a dramatic two-day fight, the House passed a resolution removing the Speaker from the Rules Committee, enlarging its membership, and providing for election of the committee by the House. This victory was followed two months later by the passage of a resolution which established a procedure through which individual members could initiate the discharge of bills from committees (Brown, 1922, pp. 143-88). Finally, in 1911 the last major objective of the opponents of Czar rule was achieved. The House, now under Democratic control, amended its rules to provide for the election of all standing committees and their chairmen (Hasbrouck, 1927, p. 11).

The immediate results of the revolt against the Speaker did not greatly impair the ability of the party leadership to lead the House on behalf of the party majority. In acting to weaken the Speaker, the Democrats had no intention of weakening the ability of the party majority to pass its program. Most Democrats believed as strongly in party government as most Republicans. Their objection was not to party government and party responsibility but to domination of the majority party and the House by the Speaker. Thus, when the Democrats gained control of the House in 1911, they set up an effective system of rule through the majority party. On the one hand, they made extensive use of the caucus and binding votes in caucus (Haines, 1915, pp. 53-110). On the other hand, they centralized power in the party by making the chairman of the Committee on Ways and Means, Oscar Underwood, both floor leader and chairman of the committee on committees.[3] Under Underwood's leadership, the Democrats controlled the House as tightly as the Republicans had under Cannon. Indeed, it is fair to say that the Insurgent Republicans were no happier in the new "reformed" Democratic House than they had been in the old "tyrannical" Republican one. They had no greater liking for "King Caucus" than for "Czar rule."

The long-run results of the revolt, however, were quite different. If Czar rule was unable to maintain itself in the face of centrifugal pressures in the party system, caucus rule was even less fitted to do so. In the absence of the buttress the formal powers of the Speaker provided for party cohesion, increases in factional discord within the party alignments easily asserted themselves and led both to a disintegration of party control mechanisms and to a dispersion of power within the House (Cooper, 1961, 1970).

The disintegration of party control mechanisms was gradual but extensive. The caucus was the first to go. Once the Democrats achieved the major items in their domestic program, the power of the caucus began to wane. From 1916 on, the divisions within the parties made it difficult to rely on the caucus and usage quickly declined (Luce, 1922, p. 513). This is consistent with the data presented in Table 1 that shows party voting at less than 15 percent in the 1917-1921 period—a 22-year low. There were small upsurges in activity in the early 1920s and early 1930s during the initial years of party turnover in the presidency. However, its use for policy purposes soon became rare in the 1920s and simply disappeared in the late 1930s. Thus by the end of the 1930s the caucus was virtually moribund as a mechanism for determining party policy (Kefauver, 1947, pp. 102-03).

When the Republicans regained control of the House in 1919, they set up a steering committee and began to rely on it rather than the caucus (Brown, 1922, pp. 195-224; Chiu,, 1928, pp. 329-34). Though this committee from the first was less of a control device than the caucus and more of a coordinating and planning mechanism, during the early 1920s it did serve to augment the leadership's power to direct its partisans. However, the same tendencies toward factionalism and bloc voting that reduced the caucus to marginal significance had a similar effect on the steering committee. By the late 1920s the party leadership had come to see the steering committee as a hindrance to their maneuverability and effectiveness. As a result, they abandoned the mechanism and began to rely instead on informal meetings among themselves, i.e., on an informal board of strategy composed of the Speaker, the floor leader, and a few trusted lieutenants (Chiu, 1928, pp. 334-36). The situation did not change when the Democrats took control of the House in 1931. Though they too established a steering committee, their leadership operated in much the same fashion as the Republican leadership had in the late 1920s (Galloway, 1961, p. 145). In short, then, by the late 1920s reliance on party control mechanisms to coordinate action and enforce cohesion had largely passed from the scene. Instead, the majority party was reduced to operating primarily through a small coterie of men, gathered around the Speaker, who met to plan strategy and whose power of direction was much less than that of the caucus or even the steering committee in their heyday.

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[3] In changing the rules to provide for election of the standing committees by the House, the Democrats took care to maintain centralized party control. Thus, they blocked the alternative preferred by the Insurgent Republicans, which would have provided for party committees on committees elected by geographic regions. Instead, the Democrats adopted a rule that only provided for election by the House and then made the Democratic members of Ways and Means, led by Underwood, their committee on committees (Brown, 1922, pp. 172-77, and 62 *Congressional Record* 1, pp. 63-64).

Nor were the caucus and the steering committee the only party control mechanisms to lose power and effectiveness in the period after 1916. The power of party mechanisms to control committee personnel also declined. Republican Speakers from 1890 to 1910 respected seniority, but they were quite prepared to violate it in the interests of party policy. The same is true of Underwood. By the 1920s the situation was substantially different. The decline of the caucus and, to a lesser extent, of the steering committee enhanced the power and independence of party factions. Their sheer willingness to stand together and cooperate with the leadership became more important than ever before. In addition, as the power and independence of party factions increased, the appointment mechanisms became more decentralized. Thus, by 1919 the Republicans had taken the power of appointment from the leader of the party and had vested it in a committee on committees, composed of nearly 40 members. Similarly, after 1923 the Democrats no longer combined the posts of floor leader and chairman of the committee on committees. In such a context seniority was transformed from an important consideration to a sovereign principle. It alone provided a standard in terms of which decentralized appointment mechanisms could distribute key committee positions among party factions without provoking disputes that would weaken the party. As a result, in contrast to earlier eras, departures from seniority were rare in the 1920s and even rarer thereafter (Abram and Cooper, 1968; Polsby et al., 1969).

From Hierarchy to Bargaining. Given the reductions in the formal powers of the Speaker between 1909 and 1911, the disintegration of party control mechanisms after 1916 produced a dispersion of power in the House. If in Cannon's day the Speaker's prerogatives as Speaker and as party chief combined to centralize power in the House, now the reduction in the formal powers of the Speaker and the disintegration of party control mechanisms combined to decentralize power in the House.

On the one hand, the rewards and sanctions which the rules placed in the hands of party leaders were reduced. The party leadership no longer had absolute control over committee appointment, the Rules Committee, or the consideration of minor business. On the other hand, the ability of party leaders to consolidate and maintain support in their own ranks was also reduced. If it is true that factionalism in the party system led to the decline of party control mechanisms, it is also true that the decline of these mechanisms had the further effect of allowing party factionalism greater expression. The result of these developments was to heighten the power and independence of the individual member and of key organizational units in the House. Denied the power they possessed over the individual member under Czar rule or caucus rule, party leaders began to function less as the commanders of a stable party majority and more as brokers trying to assemble particular majorities behind particular bills. Denied the power they possessed over the organizational structure under Czar rule or caucus rule, party leaders began to function less as directors of the organizational units and more as bargainers for their support.

These tendencies intensified as time passed. During the 1920s the breakdown of the steering committee and the rise of seniority to predominance cast party leaders more firmly in the roles of brokers and bargainers than had been the case at the start of the decade (Chiu, 1928, pp. 315-36 and Hasbrouck, 1927, pp. 48-50). Similarly, events during the 1930s confirmed and strengthened these roles. If the level of party cohesion during the 1920s was not high enough to permit reliance on party control mechanisms, it was still of such proportions that in general the holders of key organizational positions were loyal to the leadership and willing to cooperate with it. Nor, despite the increases in factionalism and bloc voting, did party leaders during the 1920s confront any stable and comprehensive basis of division among their fellow partisans, any extensive and consistent split across a whole range of issues. By the late 1930s, however, the situation had changed in both these regards.

After a brief increase in party voting during the initial years of the New Deal, party strength again began to decline in a steady and substantial fashion (Sinclair, 1978). Moreover, this decline gave birth to a new and distinctive feature, the Conservative Coalition (Brady and Bullock, 1980; Manley, 1977). Table 3 provides supportive data on both trends.

Thus, as the 1930s came to an end, party politics in the House began to display characteristics and configurations that were to become entrenched in the 1940s and to endure for several decades. These changes, however, made the task of the majority party leadership more, not less arduous. First, party divisions in the majority party now assumed a pronounced bifurcated form. In seeking to build majorities from issue to issue, the leadership accordingly was frequently threatened with the loss of support of a substantial portion of the southern wing of the party, a wing that from the late 1930s to late 1950s was roughly equal in size to the northern wing of the party (Cooper and Bombardier, 1968). Second, the divisions within the majority party now began to be translated into the organizational structure in a manner that far exceeded previous experience. The party leader-

Table 3. The Decline of Party Voting in the House and the Rise of the Conservative Coalition (1909-1953)

Congress	Year	Percent Party Votes	Percent Coalition Activity	Percent Coalition Victories
61	1909	29.4	–	–
62	1911	23.0	–	–
63	1913	19.9	–	–
64	1915	21.7	–	–
65	1917	9.4	–	–
66	1919	14.9	–	–
67	1921	35.2	–	–
68	1923	13.4	–	–
69	1925	5.3	3.5	63.5
70	1927	5.6	1.4	100.0
71	1929	13.6	5.8	80.0
72	1931	13.8	4.9	62.0
73	1933	18.9	2.1	48.0
74	1935	14.2	4.3	56.0
75	1937	11.8	7.6	67.0
76	1939	17.6	9.3	95.0
77	1941	10.5	12.5	92.0
78	1943	9.6	21.8	96.0
79	1945	12.1	22.1	88.0
80	1947	12.7	19.6	100.0
81	1949	6.5	16.4	83.0
82	1951	4.9	24.9	86.0

Sources: David Brady, Joseph Cooper and Patricia Hurley, "The Decline of Party Voting in the U.S. House of Representatives," *Legislative Studies Quarterly* 4 (1979): 381-407; David Brady and Charles Bullock, "Is There a Conservative Coalition in the House?" *Journal of Politics* 42 (1980): 549-59.

ship's ability to use the machinery of the House to suit its own purposes accordingly declined. It began to encounter difficulty securing the support of particular committees and committee chairmen much more frequently. This was especially true of the one committee in the House on which the leadership was most dependent and which historically had always been regarded as falling within the province of the leadership—the Rules Committee. For the first time in history the leadership found itself confronted with a Rules Committee that regarded itself and acted as an independent agent, rather than as an arm of the leadership (Galloway, 1961, pp. 145-48; Jones, 1968).

These developments further weakened the power and position of the leadership and in so doing further enhanced the independence of individual members and organizational units. Moreover, the impact was long-lasting, not transitory. A divided majority party was less amenable to leadership direction and control than an incohesive one. From the late 1930s on, the leadership was forced to place even more reliance on brokerage and bargaining than had been necessary in the early 1930s or 1920s (Herring, 1940, pp. 21-45).

The Rayburn House

The period from 1910 to 1940 may therefore be seen as a period of transition in the character of the House as a political institution. By 1940, the year Sam Rayburn assumed the Speakership, a new and distinctive type of House had emerged. It was a House that was destined to endure in most of its essential features until the reform of the Rules Committee in the early 1960s and in many of its essential features until the reemergence of the caucus in the late 1960s (Cooper, 1970; Brady et al., 1979).

The House under Decentralized Rule. The Rayburn House was a far different body from the House of Cannon or Reed. Centralization of power and hierarchical control had given way to a diffusion of power and bargaining.

On the one hand, the majority party leadership could no longer command the organizational units due to the breakdown of party control mechanisms and the elimination of the Speaker's prerogatives over appointment and the Rules Committee. Rather, it had to seek to win their support and do so in a context in which divisions in the majority party had become so pronounced that they had begun to appear at key vantage points in the organizational structure. On the other hand, the majority party leadership could no longer command overwhelming support from the ranks of its partisans on the floor due both to the decline in party strength and the decline in the fund of rewards and sanctions at its disposal. Rather, it had to seek to build majorities from issue to issue and do so in a context in which a deep split existed in the ranks of the majority party and distaste for party discipline was intense and pervasive. Political scientists writing about the House in the 1940s and 1950s accordingly emphasized themes quite different from those emphasized in the initial decades of this century: the primacy and amount of catering to constituency, not party loyalty or discipline; the dispersed and kaleidoscopic character of power in the House, not the authority and responsibilities of party leaders; the role of committee chairmen as autonomous and autocratic chieftains, not their operation as loyal party lieutenants (Young, 1943; Gross, 1953).

However, the fact that power became decentralized in the House does not mean that significant centers of power did not continue to exist. What occurred was a wider dispersal of power, not its fractionalization.

First, the party leadership retained substantial ability to influence and even control outcomes in the House. If party voting decreased, the party bond remained important both because of the degree of agreement still present and because of the interest most members had in establishing some kind of party record. Thus, though the leadership could no longer rule the House on the basis of votes drawn from its own party, it could still usually count on a large and stable reservoir of support from its fellow partisans (Mayhew, 1966). In addition, party leaders continued to derive leverage from other sources. The formal powers remaining to the Speaker aided their ability to control access to the floor and proceedings on the floor. The influence party leaders maintained over the party committee on committees enabled them to alter the political complexion of particular committees through the screening of new appointments. The power party leaders retained, due to their positions in the House and in the party, to dispense favors and build up credits augmented their capacity to secure the cooperation of ordinary members and holders of organizational positions (Ripley, 1967). Finally, the leadership could rely on the president's influence to win the support of reluctant partisans both in committee and on the floor.

Second, committees and committee chairmen emerged as rival power centers of great importance. In a context in which House rules gave the committees immense power over the handling of legislation within their jurisdictions and committee rules and practice gave their chairmen immense power within their committees, the decline in leadership authority and power redounded to the advantage of the committees and their chairmen. Typically, committee opposition to legislation sealed its fate, even when favored by the leadership. Conversely, committees that operated in a unified fashion were accorded great deference on the floor and had high levels of success (Fenno, 1962). Party leaders thus could not treat committees merely as instruments of their will nor chairmen simply as loyal lieutenants. Rather, they had to function largely as petitioners of committee support and floor managers of committee legislation.

In the Rayburn House the committees accordingly reemerged as the feudal baronies they had been in the decades immediately preceding Czar rule. And, indeed, to a significant degree the story of the Rayburn House is a story of conflict among northern majorities in the Democratic party, the majority party leadership, and southern-dominated committees in which northern pressure for action was continuing, leadership efforts sporadic, and committee obstruction very difficult to overcome. Ironically enough, then, the ultimate result of the revolt of 1910 was to redefine the problem of majority rule in the House, not to solve it. A new and equally serious difficulty, i.e., minority obstruction, simply replaced the difficulty that had aroused passions in the preceding era, i.e., autocratic leadership power.

Leadership Style in the Rayburn House. In sum, by 1940 the role and power of the party leadership in the House had been substantially altered. Though the leadership retained responsibility for and continued to provide overall guidance and direction in the conduct of the House's business, it now had to operate within a far harsher set of constraints than in 1910. At the floor stage, the leadership usually had no choice but to engage in the painful process of assembling shifting majorities behind particular bills through bargaining and maneuver. At the committee stage, the leadership was often forced to engage in intricate and prolonged negotiation with committees and committee chairmen. Indeed, the leadership was now placed in a position where inability to accommodate an organizational unit would mean failure to pass party legislation, unless it was able to organize a majority of such strength and intensity that it could force a vote on the floor through the pressure of opinion in the House or the use of a mechanism such as discharge. The result was that by

1940 the personal, political skills of the leadership, rather than its sources of institutional power, had become the critical determinant of the fate of party programs.

All this, in turn, led to the emergence of a leadership style that contrasted markedly with that of Cannon and Reed. The components of this new style emerged gradually in the 1920s and 1930s as power in the House decentralized. It crystallized under Rayburn and was fully applied by him. It represented his experienced and finely tuned sense of what made for effective leadership in a House in which the Speaker lacked the formal powers of a Czar, had to mobilize a majority party fairly evenly balanced between discordant northern and southern elements, confronted a set of committees and committee chairmen with great power and autonomy, and had to deal with individual members who rejected party discipline and prized their independence.

The main facets of the Rayburn style can be analyzed in terms of the following categories: personal friendship and loyalty, permissiveness, restrained partisanship and conflict reduction, informality, and risk avoidance.

Whereas Cannon and Reed relied on their authority and power as Speakers and party chiefs, Rayburn relied on personal friendship and loyalty. If the Speaker could no longer command the House, his vantage points in the formal and party systems as well as his personal prestige provided a variety of opportunities to do favors for members. Rayburn exploited these opportunities in a skillful and imaginative manner. He sought continually to bind members to him as a person on the basis of favors rendered to them as persons, favors which eased their lives in Washington, enhanced their sense of personal worth, and/or advanced their political careers. In contrast to Cannon and Reed, who emphasized policy goals over personal relationships, Rayburn sought to attain policy goals through personal relationships, through nurturing friendships and creating obligations (Bolling, 1965, pp. 65-68; Daniels, 1946, pp. 56-58).

Whereas Cannon and Reed were quite intolerant of party defection and quite amenable to employing punishments as well as rewards as means of inducement, Rayburn was very permissive. He explicitly legitimized party irregularity on the basis of policy disagreement or constituency pressure and was reluctant ever to punish or coerce a member. To be sure, he did withhold rewards or favors from those he felt failed to cooperate with him for light or insubstantial reasons. Nonetheless, his prevailing inclination was not to alienate members whose vote or help he might need on future occasions (Steinberg, p. 178).

Whereas Cannon and Reed were highly partisan and accepted both intraparty and interparty conflict as necessary aspects of majority party leadership, Rayburn sought to temper partisanship in personal relationships and to restrain conflict generally. He saw party mechanisms, such as the caucus and steering committee, as mechanisms for exacerbating party divisions and studiously ignored them. He established friendly relations with minority party leaders receptive to his overtures and extended advice and favors to rank-and-file minority members. He emphasized reciprocity and compromise as the prime behavioral rules for all members. Thus the guiding motif of his regime was not "serve party policy goals," but rather "to get along, go along," i.e., trade favors (MacNeil, 1963, pp. 84-85).

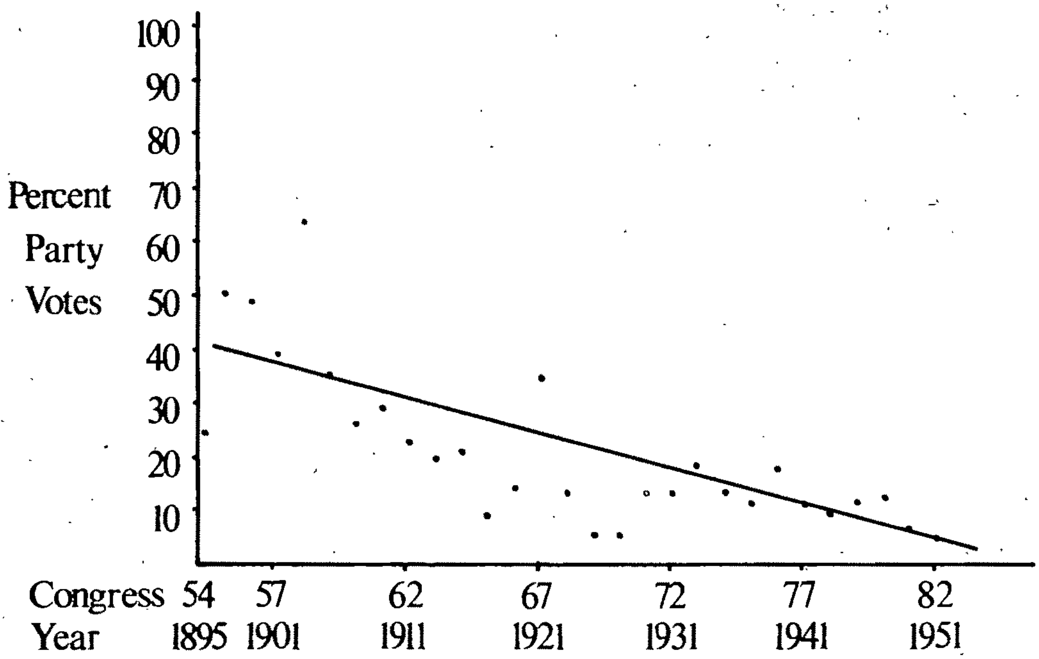
Whereas Cannon and Reed sought to achieve party programs by mobilizing partisan majorities and working through a stable set of partisan lieutenants, Rayburn's approach was more informal and ad hoc. Bargaining needs and opportunities determined his legislative strategies and personal contact served as his main means of implementing these strategies. Thus, on the whole, he worked through varying sets of trusted friends who were loyal Democrats and whom he had placed in key positions in the committee system. However, he was not averse when pressed at the committee stage to appealing to powerful opponents, who were nonetheless close friends, for help, men such as the southern Democratic stalwart, Gene Cox, or the Republican leader, Joe Martin. Similarly, at the floor stage he customarily asked varying sets of members, who were close friends and/or owed him favors, to insure his majority by standing ready to vote for him if needed, even against their policy preferences and/or constituency interests (Clapp, 1963, pp. 286-87).

Finally, whereas Cannon and Reed were aggressive in the pursuit of party policy goals, Rayburn was cautious. His inclination was to avoid battles when the outcome was uncertain. To be sure, in instances when a Democratic president and/or large number of his fellow partisans pressed him, he would usually wage some sort of fight. But both because he felt that defeat undermined his influence and because he did not like to expend his credits in losing causes, his clear and decided preference was to refuse battle, to wait until prospects for victory were favorable. Similarly, he shied away from challenging any of the key facets of decentralized power in the House, despite their restrictive impact on his ability to lead. His inclination was to work with what existed and endure, rather than to seek basic change. Only when extremely provoked did he contest the power of senior chairmen or the prerogatives of the Rules Committee and even then only indirectly. Thus he did not discipline Graham Barden but rather took over the Education and Labor Committee by filling vacancies with liberal Democrats. Thus he did not discipline Howard Smith or Bill Colmer or limit the power of the Rules Committee. He rather chose to expand its membership. In short, then, Rayburn was far more inclined to accept the defeat of party programs than to risk his influence and prestige in battles to attain them (Clapp, 1963, pp. 66-69; Wicker, 1968, pp. 43-54).

The Bases of Personalized Leadership. Earlier we argued that Czar rule derived from the interaction of the Speaker's formal powers and his leverage as party chief. We further argued that party strength was the determining factor in this interaction and that it was rooted primarily in the polarized constituency bases of the two parties. The emergence by 1940 of a new type of House and a new leadership style, both of which we may identify with Sam Rayburn, can be explained in terms of the altered character and impact of these same variables.

Confining ourselves simply to events in the House, the interaction between formal power and party strength again played a critical, though quite different, role. As we have already suggested, the interaction of these variables now worked to reduce leadership power. Party strength could no longer support or justify high concentrations of formal power in the leadership. Limited formal power, however, allowed party divisions fuller expression and increases in these divisions undermined party control mechanisms. The atrophy of these mechanisms, in turn, augmented the power and independence of party factions and transformed the leadership into bargainers and brokers, into middlemen rather than commanders (Truman, 1959, pp. 202-27).

Evidence of the continuing decline in party strength, which we interpret as both cause and effect of the decentralization of power in the House, has already been presented in Table 3. To reinforce our tabular evidence we regressed party voting against time for the whole period from 1894-1952. The results are presented in Figure 1. The slope of the line is negative (B = -1.3) and the correlation between time and party voting -.74. Clearly, changes in party strength and changes in



Intercept = 40.8
B = −1.3
r = − .74

Source: David Brady, Joseph Cooper, and Patricia Hurley, "The Decline of Party in the U.S. House of Representatives, 1887-1968," *Legislative Studies Quarterly* 4 (1979): 384-85.

Figure 1. Party Voting in the House of Representatives (1895-1953)

insitutional structure covary in a manner that is consistent with our argument.

Nonetheless, if we again would acknowledge the impact of the internal, interactive effect between formal power and party strength and accord party strength the determining role in this interaction, we again would also argue that levels of party strength are subject primarily to external determination. In short, though restricted formal power provided a context in which party divisions could be expressed and extended, the primary engine of increased divisiveness was increassed disharmony in the constituency bases underlying the majority party coalition. Thus, as in the case of leadership power and style during the period of Czar rule, the key to the Rayburn House and the Rayburn style lies in electoral alignment patterns.

Table 2 shows that from 1881 to 1921 party strength was high when the constituency bases of the parties were highly polarized and that it declined when these bases became less polarized. We have argued that increased factionalization in the party system was the primary source of the increased divisiveness that undermined the use of party control mechanisms in the 1920s. In order to show how constituency alignments are related to the further decline of party and the emergence of the Rayburn House, it is necessary to analyze the New Deal realignment and its aftermath.

The political revolution known as the New Deal was the product of the Great Depression. The voters providing the Democrats their majority came primarily from those groups most affected by the depression, farmers and low-income city dwellers, including blue-collar workers, ethnic groups, and blacks. Thus, the New Deal resulted in an increase in Democratic party allegiance across all constituent characteristics. Rather than recreating polarized congressional parties as in the period of Czar rule, the New Deal created a monolithic majority party which encompassed all types of constituencies. To the Democratic party's traditional base of support, the rural South, it added the urban Northeast and the urban and rural Midwest (Sundquist, 1973, pp. 183-218; Ladd and Hadley, 1975, pp. 31-88).

Table 4 illustrates and supports this point. It includes the following data, collected from the 1930 census and mapped onto congressional districts: the number of blue-collar workers, value added by manufactures, and population density. Constituencies are ranked as high or low in relation to these characteristics in terms of the national mean and the percentage increase as well as the absolute ratio increase calculated for Democratic congressmen.

The monolithic majority party coalition created by the New Deal was formed around the basic issue of government aid to combat the effects of the Depression. Hoover and the Republican party favored voluntarism and nonintervention, whereas the Democrats favored active government involvement. As long as the issue of the role of government in combating the Depression remained the central and defining one, congresional Democrats had a broad basis for unity, despite their increased disparateness. And, indeed, in the 1930s there was a break in the long-term trend toward declining party voting (see Figure 1). However, as is now evident, the Roosevelt coalition could not maintain its cohesion across changing issue dimensions (Sinclair, 1978). As a monolithic rather

Table 4. Increases in Congressional Majority Party Composition during 1932 Realignment

District Characteristic	Percent Democratic Congressmen 70th House (1927-29)	 73rd House (1933-35)	 Percentage Increase	Absolute Ratio Increase
Blue-Collar				
Low (Farm)	57	82	25	1.37
High (Labor)	32	64	32	1.97
Value Added				
Low (Non-Industrial)	54	81	27	1.40
High (Industrial)	34	67	33	1.89
Density				
Low (Rural)	53	77	24	1.38
High (Urban)	30	67	37	1.96

Source: Compiled from U.S. Bureau of Census, *Fifteenth Census of the United States* (1930): *Agriculture,* Vol. 2, parts 1, 2 and 3; *Manufactures,* Vol. 3; *Population,* Vol. 3, parts 1 and 2 (Washington, D.C.: U.S. Government Printing Office).

than polarized coalition, it was particularly vulnerable to the emergence of issues that would divide its various components rather than unite them as the Depression had done.

In the late 1930s and early 1940s two factors combined to redefine the political climate and render it far less hospitable to majority party unity. The first was the alteration in the character and thrust of the New Deal, which focused attention and controversy on the federal government's general role as an agent of social welfare rather than its narrower role as an agent of economic recovery. The divisive potential of this development was signaled by the battle over New Deal legislation in the 75th Congress (1937-1939), a battle that in the eyes of many analysts marks the true emergence of the Conservative Coalition (Brady and Bullock, 1980). The second was the worsening international situation, which finally led to the Second World War. This development focused attention and controversy not only on defense and foreign policy, but on the management of a war economy as well. In so doing, it also by-passed or submerged old bases of Democratic unity and reinforced divisions along liberal-conservative lines (Young, 1956).

The emergence and growth of the Conservative Coalition in the late 1930s and early 1940s, documented in Table 3, testifies to the impact of these factors in producing a new and enduring split in the congressional Democratic party that was rooted in differences between rural conservative southern constituencies and urban liberal northern ones. Nor is it surprising that as the dimensions of the split increased and finally stabilized, party voting declined. A comparison of the data in Table 3 broadly demonstrates the point; but to pin it down we calculated the correlation (Pearson's r) between Conservative Coalition activity and party voting for the period 1931-1953. The result is an impressive -.67.

In short, then, external factors are of primary importance in accounting for personalized rule as well as Czar rule. In a context in which the interaction of restricted formal power and declining party strength had already combined to disperse power, the emergence of a basic split in the majority party, rooted in constituency differences, further substantially undermined leadership power. Rayburn's highly personalized style was thus a reaction to his party situation, to the corps of independent and divided partisans he had to work with and lead. Indeed, his style is not only distinguishable in kind from that of Cannon and Reed, but even in degree from that of preceding Democratic Speakers, such as Garner and Rainey, who did not have to worry continually about southern support on key committees and the floor (MacNeil, 1963, p. 34).

Conclusion

Our historical analysis of the transition from Cannon to Rayburn suggests several broad propositions or hypotheses that explain House leadership roles and behavior and have general import or significance for the analysis of legislative leadership. They are as follows.

First, institutional context rather than personal skill is the primary determinant of leadership power in the House. To be sure, leadership power, like other forms of power, is a combination of the fund of inducements available and the skill with which they are used. Nonetheless, skill cannot fully compensate for deficiencies in the quality or quantity of inducements. Indeed, the very skills required of leaders themselves vary in terms of the parameters and needs imposed by the character of the House as a political institution at particular points in its history. Thus, Rayburn was not and could not be as powerful a Speaker as Cannon or Reed. His sources of leverage in the formal and party systems were simply not comparable. Nor did Reed or Cannon require the same level of skill in building credits or bargaining as Rayburn to maximize their power. Similarly, it is doubtful that O'Neill can be as strong a Speaker as Rayburn whatever the level of skill he possesses, given the increased fractionalization in both the formal and party systems that has occurred in the past two decades.

Second, institutional context rather than personal traits primarily determines leadership style in the House. To be sure, style is affected by personal traits. Nonetheless, style is and must be responsive to and congruent with both the inducements available to leaders and member expectations regarding proper behavior. Indeed, the personal traits of leaders are themselves shaped by the character of the House as a political institution at particular points in time through the impact of socialization and selection processes that enforce prevailing norms. Thus, if Rayburn was a more permissive and consensual leader than Cannon or Reed, this is not because he was inherently a less tough or more affective person, but rather because of his weaker sources of leverage and the heightened individualism of members. If O'Neill's leadership style is far closer to that of Rayburn, McCormack, and Albert than to Cannon or Reed, this is not attributable to basic personality similarities and differences.[4] It is rather attributable to the fact that the House he leads is far more like theirs than the House during the days of Czar rule. Similarly, though leaders remain distinct personalities (note, for example, O'Neill and Albert), range of tolerance for personal traits or predispositions that conflict with prevailing norms is restricted. O'Neill therefore must and does curb the exercise of new sources of leverage gained since the revival of the majority caucus in 1969 to avoid even appearing like a Czar.[5] In contrast, members who cannot eliminate or temper traits that run counter to prevailing norms are disadvantaged in the pursuit of leadership office. Witness Dick Bolling who would have been far more at home in the House of the 1890s.[6] In a basic sense, then, the impact of context on leadership style has something of the character of a self-fulfilling hypothesis.

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[4] O'Neill's style is more partisan in personal relationships and more consultative or open than Rayburn's. But it remains highly personal, ad hoc, informal, and permissive. See *National Journal* (1977), pp. 940-46; (1978), pp. 4-9, 1384-88; (1979), pp. 1326-31.

Third, there is no direct relationship between leadership style and effectiveness in the House. This is true whether effectiveness is interpreted relatively or absolutely. Interpreted relatively, effectiveness is a matter of the skill with which resources are used, not actual results. However, whereas style is primarily determined by the parameters and needs imposed by the political character of the House as an institution at certain points in his history, particular styles can be applied with varying degrees of skill. Note, for example, the differences between Rayburn, McCormack, and Albert. Similarly, if effectiveness is interpreted absolutely, i.e., in terms of actual results of achievements, there is still no direct or simple relationship between style and effectiveness. On the one hand, there is no one best "style"; the relationship between style and effectiveness is rather a highly contingent or situational variable. On the other hand, even when contexts dictate roughly similar styles, they do not necessarily accord them roughly equal chances of success. Thus, given his sources of leverage in the formal and party systems, O'Neill has little choice but to adopt a leadership style that in many key respects is similar to Rayburn's. Yet, the greater degree of fractionalization in both systems in the 1970s, as opposed to the 1940s, reduces his overall prospects for success in passing party programs. O'Neill therefore is likely to have far less success overall than Rayburn, even though he too leads in a highly personal, informal, permissive, and ad hoc manner.

Fourth, and last, the impact of institutional context on leadership power and style is determined primarily by party strength. To be sure, the degree of organizational elaboration substantially affects the intensity of the demands imposed on integrative capacity and is largely a product of factors other than party strength, e.g., size and workload. Nonetheless, integrative capacity derives or flows primarily from party strength (Cooper, 1975). The higher the degree of party unity or cohesion the more power in both the formal and party systems can be concentrated in the hands of party leaders and the more leadership style will be oriented to command and task or goal attainment. The lower the degree of party unity or cohesion the more power in both the formal and party system will be dispersed and the more leadership style will be oriented to bargaining and the maintenance of good relations. The infrequency of eras of centralized power in the House is thus explicable in terms of the very high levels of party strength required to support it, requirements which have increased as the organization itself has become more elaborate. Similarly, the degree of power dispersion now present is explicable in terms of present weaknesses in party unity or coherence both in absolute terms and relative to an organizational structure that has grown far more complex in the past two decades. Given the dependence of internal party strength on appropriate constituency alignments, all this, in turn, means that leadership power and style are ultimately tied to the state of the party system in the electorate, that external or environmental factors have a decisive bearing on the parameters and needs that institutional context imposes on leadership power and style. In short, then, to be understood Cannon and Reed must be seen in the context of an entire party system, and the same applies to Rayburn in the 1940s and 1950s and to O'Neill today.

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[5] Since the mid-1970s the Speaker has had the power to nominate Democratic members of the Rules Committee to the Democratic Caucus and the power to multiply or sequentially refer legislation to committees, to establish time limits for their reports (in effect, Speaker discharge), and to establish special ad hoc committees to receive such reports and report to the House. However, the Speaker has used ad hoc committees and referral with a time limit cautiously. In addition, he has not as yet refused to recommend the reappointment of any sitting Rules Committee member and threatened to do so only rarely and even then only by implication. See *National Journal* (1977), pp. 940-46.

[6] Bolling is perhaps more reminiscent of Reed than any recent House leader. Compare Polsby (1969, pp. 343-45), Bolling (1975, pp. 236-44), and *National Journal* (1976), pp. 170-71, with Brown (1922, pp. 84-97) and Robinson (1930, pp. 377-99).

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The End Game in Presidential Nominations

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This study shows that a simple, empirically derived rule accounts well for the outcome of multi-ballot, majority-rule conventions in the period 1848-1948 and that the same rule would have enabled one to predict correctly the outcome of all contested presidential nominations since 1952 before any candidate had achieved majority support in the Associated Press' polls of delegates. The rule's success suggests two conclusions about the behavior of delegates: (1) that their guesses about outcomes are based on objective cues, similarly interpreted, and (2) that the trend in voting for candidates, in addition to the level of support they attract, is an important element of such cues. The study's findings also suggest that delegates in recent years do not *contrast sharply with their predecessors in their predilection for bandwagon voting.*

By a usage long established in both American and British politics, the calculated shifting of one's support from a loser to a winner is known as "jumping on the bandwagon."[1] Delegates to U.S. national party conventions often try their skill at this athletic feat, most observers agree, and most also agree on the reason: many delegates seek spoils. Successful presidential candidates, who control a vast treasury of spoils, reward friends and punish enemies. After the fashion of the leprechaun in "Finian's Rainbow," delegates who cannot win with the candidate they love are prepared to love the candidate who wins, and their voting traces patterns of support like those in Figure 1. That this view of bandwagon voting in conventions should be widely accepted is not surprising. The great shifts in support often observed in conventions argue strongly that something other than assessments of candidates' merits guides the voting of many delegates. Conceivably, sudden fits of enthusiasm might set off the stampedes toward the winner that characteristically end conventions, but such an explanation does not square well with what is known about delegates and their leaders.

Note, however, that the common view of convention bandwagons does not fully explain them. It gives the motive for bandwagon voting but not

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Without trying to claim their blessing for any errors herein, we express, for the advice we have received, our wholehearted thanks to Douglas Arnold, James DeNardo, Fred Greenstein, and Walter F. Murphy, all of Princeton University; to Glenn Shafer of the University of Kansas; to Michael Dempster, currently on leave from Balliol College, Oxford; to John Enos of Magdalen College, Oxford, Anthony King of Essex University, and Nick Stern of Warwick University; and to David Price of Duke University. Sheryl Brown, June Traube, and Patricia Jenkins also have our gratitude for their assistance. Donald S. Collat wishes to thank the former Secretary of the Rhodes Trust, Sir Edgar T. Williams and the other Trustees for their support of this research. Stanley Kelley, Jr. is grateful to the Center of Advanced Study in the Behavioral Sciences for the opportunity that his fellowship gave him to pursue this and other research. Ronald Rogowski expresses his thanks to the Duke University Research Council and to Duke University for leave time and other research assistance.

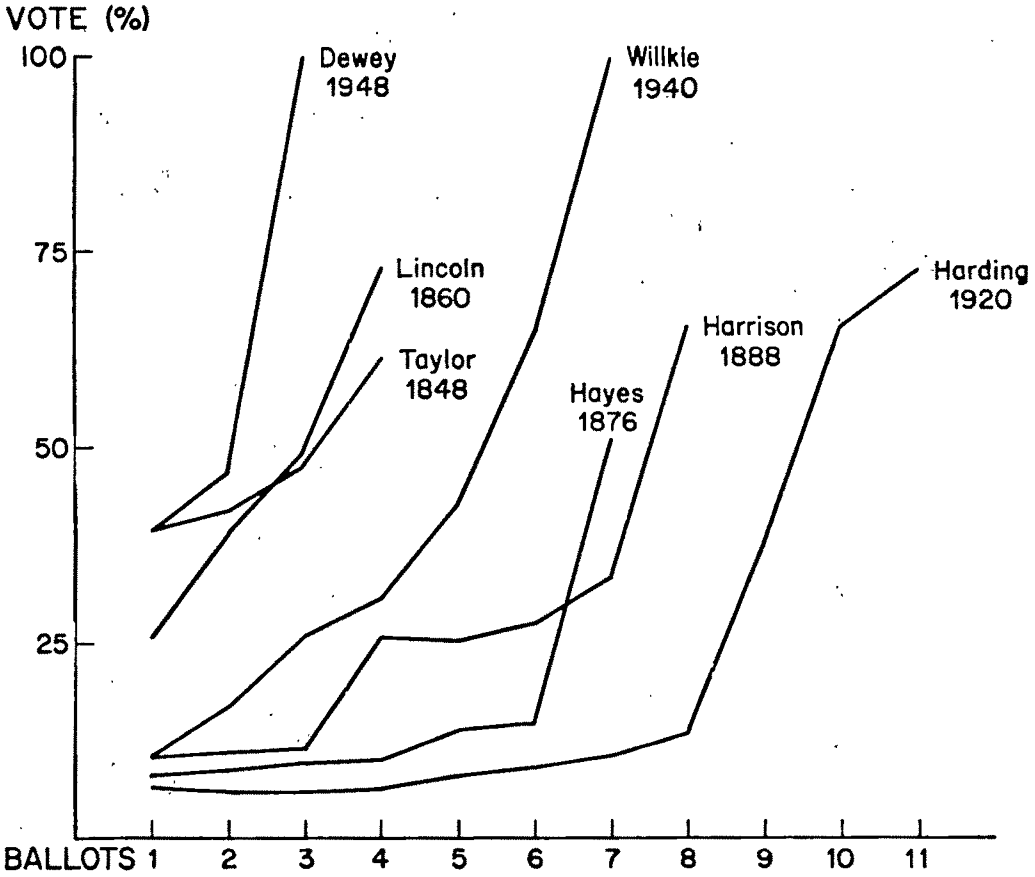
[1] The original meaning of "bandwagon" was literal: "a large wagon, capable of carrying the band in (and typically at the head of) a procession." By the early 1900s in the United States, "keeping abreast of the bandwagon" meant, roughly, being in the van, e.g., of fashion or of opinion, and "to climb, hop, jump, etc., on the bandwagon" came to mean to put oneself at the head of a crowd or, more broadly, "to join in what seems likely to be a successful enterprise, to strive to join the winning side." The first recorded use of the term in its political sense is attributed to Theodore Roosevelt, who, as governor of New York, nominated to the State Lunacy Commission in 1899 one William Church Osborn—"a Princeton man, well up in charitable work," as Roosevelt described him. A nominal Democrat, Osborn was strongly opposed by organization Republicans in the state senate; but, on April 28, Roosevelt was able to write him: "Well, old man, we got you through! At first it looked very doubtful, but when I once became sure of one majority [sic] they tumbled over each other to get aboard the band wagon." See Burchfield (1972, vol. 1, p. 197) and Morison (1951-54, Vol. 2, pp. 985, 999).

the means—an important omission, because profitable bandwagon-jumping requires skill as well as motivation. Candidates are most grateful for support that helps them win; to jump when a candidate has already been nominated is to jump too late for anything but cursory consideration. A delegate can also jump too soon, off a bandwagon only temporarily stalled, or onto one that later stops dead in its tracks, for an overall effect that is graceless. Although timing is everything, when to jump is not obvious, especially if there are many would-be bandwagon riders. Predicting the outcome of a race in which loyalties or ideology tightly constrain choices can be relatively easy, once those constraints are known. Predicting the votes of bandwagon chasers, however, requires a guess about others' guesses, which, as many followers of the stock market can sadly testify, is a perilous undertaking.

Yet the data suggest that delegates' predictions are good ones and that they very largely coincide. Why? How does one guess well in such a case? Without an answer to these questions, the coincidence of judgments in bandwagon voting remains unexplained. We shall suggest a possible answer by showing that anyone could have predicted accurately the outcome of all contested nominations from 1952 onward, using information easily available to any delegate, and could in every case have done so *before* the final rush of support that decided the nomination.

A Predictive Rule

Imagine a delegate, typical of some sizeable group of delegates, who is wholly indifferent to the merits of the candidates and wants only to support the winner before the decisive ballot.



Source: Constructed from data compiled in Richard C. Bain and Judith H. Parris, *Convention Decisions and Voting Records*, 2nd ed. (Washington, D.C.: Brookings, 1973).

Figure 1. Patterns of Support for Winning Candidates in Multi-Ballot, Majority Rule Conventions, 1848-1948

What information might he or she use to guess the eventual winner? In the rumor-ridden environment of a national convention, surely the most reliable indicators of candidates' support are early ballots (McGregor, 1974, p. 470). Our delegate should examine this record closely for some show of strength impressive enough to persuade delegates that a candidate's victory is highly likely—so likely, in fact, that they will jump on that candidate's bandwagon, bringing victory with them.

Three factors might affect delegates' estimates of a candidate's chances:

1. S_{t-1}, the candidate's support in the most recent test of it (on a convention ballot or in a survey of delegates' commitments);
2. $(S_{t-1} - S_{t-2})$, the change in the candidate's support from the next-to-last test to the last one; and
3. Q, the minimum proportion of the vote sufficient to nominate.

The ratio,

$$\frac{S_{t-1} - S_{t-2}}{Q - S_{t-1}},$$

which combines these terms, seems a reasonable measure of how impressively a candidate has performed on any given test of support (Collat, Kelley, and Rogowski, 1976). The ratio takes account of where the candidate is, how close victory is, and how fast the candidate is progressing towards it. A negative value for $(S_{t-1} - S_{t-2})$ means that a candidate has lost support, always a bad omen.[2] If a candidate makes a small gain when close to victory, or a large gain when not,

$$\frac{S_{t-1} - S_{t-2}}{Q - S_{t-1}}$$

(which we shall henceforth call the gain-deficit ratio), will be large and positive. Let us say, then, that a candidate's showing is the more impressive the larger the gain-deficit ratio, and impressive enough to predict victory when that ratio reaches some critical value.

A procedure for our hypothetical delegate to use in forecasting the outcome of a contested convention now suggests itself: compare the amount of support that each candidate still needs to win after the second test of support (the candidate's "deficit") with the change in that candidate's support between the first and the second test. Predict to win any candidate whose gain-deficit ratio attains the critical value. If there is none such, calculate the gains and deficits of the candidates anew after the third test of support. Again, predict to win any candidate whose gain-deficit ratio attains the critical value, and, if there is none, recalculate after the next test. And so on.

There is one ambiguity in this procedure: the gain-deficit ratios of more than one candidate could reach the critical value simultaneously. The natural solution is to predict victory for that candidate with the highest gain-deficit ratio.

Now, our delegate needs one more piece of information to apply the procedure just described: the critical value of the gain-deficit ratio.

We looked for a critical value of the ratio in the voting of one set of conventions—the eleven majority-rule conventions of three or more ballots held before 1952—and then used that value to predict the outcomes of another set—contested conventions from 1952 onward.[3] The critical value that maximized the accuracy of predictions for the conventions before 1952 turned out to be 0.360.[4]

Table 1 reports the results of our procedure when applied to contests in majority-rule conventions before 1952, using 0.360 as the critical value of the gain-deficit ratio. In two of the eleven conventions we were unable to make a prediction; in those no candidate's gain-deficit ratio reached 0.360. In the remaining nine conventions, we predicted all winners correctly. This result was unexpected; before embarking on our search for a critical value, we expected to make at least some incorrect predictions, whatever specific value we chose.

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[2] In majority-rule conventions no winning candidate has sustained successive losses of as much as 1.5 percent of the convention's vote. The highest successive losses for winning candidates who suffered any losses at all were those of Winfield Scott in 1852, 1.4 percent; James A. Garfield in 1880, .3 percent; Benjamin Harrison in 1888, .5 percent, and Warren G. Harding in 1920, .8 percent. Further, no candidate who led on the first ballot of a majority-rule convention ever lost votes thereafter—*any* number of votes on *any* ballot—and then won nomination.

[3] In the period 1848-1948 there were 19 majority-rule conventions of one ballot, none of two ballots, and (as noted) eleven of three or more ballots. In that period Whigs and Republicans required a majority to nominate; the Democrats required a two-thirds majority until after 1936.

[4] There was actually a range of critical values that maximized the number of correct predictions for the eleven conventions before 1952. In choosing a predictive rule to be applied to conventions since then, our main aim was to minimize the potential for error. *Ceteris paribus*, the higher the critical value specified in the rule, the greater the chance that a candidate attaining it would turn out to be the winner. Thus, we chose the highest value of the range.

Table 1. Predicting the Outcome of Majority-Rule Conventions of Three or More Ballots, 1848-1948
(Critical value of gain-deficit ratio = 0.360)

Convention[a]	Prediction Possible?	Predicted Winner	Actual Winner	Actual Value of Gain-Deficit Ratio	Ballot of Prediction	Total No. of Ballots
1848 W	Yes	Taylor	Taylor	2.144	3	4
1852 W	Yes	Scott	Scott	0.429	50	53
1860 R	Yes	Lincoln	Lincoln	1.491	2	4[b]
1876 R	No	–	Hayes	–	–	7
1880 R	No	–	Garfield	–	–	36
1884 R	Yes	Blaine	Blaine	0.722	3	4
1888 R	Yes	Harrison	Harrison	0.607	4	8
1916 R	Yes	Hughes	Hughes	0.453	2	3
1920 R	Yes	Harding	Harding	2.038	9	10
1940 R	Yes	Willkie	Willkie	0.363	3	6
1948 R	Yes	Dewey	Dewey	2.455	2	3

Sources: Richard C. Bain and Judith H. Parris, *Convention Decisions and Voting Records,* 2nd ed. (Washington, D.C.: Brookings, 1973); *New York Times,* 22 June 1852; and *Official Proceedings of the Republican Convention of 1880.*

[a] W = Whig, R = Republican.

[b] The third ballot, in which there was a shift in votes after the roll call, has been counted as two ballots, since no candidate had a majority after the initial roll call.

Table 2 shows the application of our procedure in two cases. On the second ballot of the Republican convention of 1860, Abraham Lincoln's gain was 1.491 times his deficit, and the gain-deficit ratio of every other candidate remained below 0.360. Lincoln should be predicted the winner. On the second ballot of the Republican convention of 1940 no prediction could be made, but on the third ballot Wendell Willkie's (and only Willkie's) gain was more than 0.360 times his deficit. A prediction is again in order. Notice that in both these cases the predictions are made when the outcome is still far from obvious and when a losing candidate still holds the lead.

Predicting the Outcomes of Recent Conventions

In making predictions for recent contests, we treated press polls of delegates' preferences as equivalent to convention ballots. Obviously, the two are not the same thing: the latter decide the nomination; the former do not. Still, our best information about the delegate-strength of candidates before conventions comes from such polls, and there is ample reason to think that delegates take them seriously.

We found seven or more surveys of delegates' preferences for every recent race whose outcome was in doubt after the last primary: the Democratic contests of 1952, 1956, 1960, 1968, and 1972 and the Republican ones of 1952, 1964, 1968, and 1976. All the surveys that underlie Table 3 and Figure 2 were conducted by the Associated Press, except for one conducted in 1956 by the staff of the *New York Times;* and all were taken when delegates representing at least seven-eighths of a convention's votes had been chosen. Our decision to start counting at this precise point was more or less arbitrary. Early in a race for nomination, many increases in candidates' support reflect the decisions of the primary electorates or party agencies that mandate delegates, not those of the delegates themselves. This fact suggested limiting our attention to surveys taken after all delegates had been chosen, but we also wanted to see how early we might be able to predict outcomes. In the end we found that no predictions would change, whether based on surveys taken when as few as 80 percent of the delegates had been chosen or when all had been.

Table 3 reports our results for recent races, using 0.360 as the critical value of the gain-deficit ratio. This time, predictions were possible in every case, and all were correct.[5] Four of the predictions

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[5] We also applied our rule to data from surveys by the *New York Times* in the 1972 Democratic race and by the *New York Times* and the *Washington Post* in the 1976 Republican race. These data also yielded the predictions reported in Table 3, though the gain-deficit ratios and

Table 2. Two Predictions of the Onset of Bandwagon Voting

Republican Convention of 1860 (Needed to Win: 234)

Candidate[a]	Ballot Number and Vote 1	2	3	4
Seward	173.5	184.5 (0.222)[b]	180	121.5
Lincoln	102	181 W[c] (1.491)	231.5	340
Cameron	50.5	2 (−0.209)	0	0
Bates	48	35 (−0.065)	22	0
Chase	49	42.5 (−0.034)	24.5	2

Republican Convention of 1940 (Needed to Win: 501)

Candidate[a]	Ballot Number and Vote 1	2	3	4	5	6
Dewey	360	338 (−0.135)	315 (−0.124)	250	57	11
Taft	189	203 (0.047)	212 (0.031)	254	377	318
Willkie	105	171 (0.200)	259 W[c] (0.364)	306	429	655
Vandenberg	76	73 (−0.007)	72 (−0.002)	61	42	0

Source: Richard C. Bain and Judith H. Parris, *Convention Decisions and Voting Records,* 2nd ed. (Washington, D.C.: Brookings, 1973).

[a]The table omits data on minor candidates.

[b]Gain-deficit ratios appear in parentheses.

[c]W is attached to the tally on which a victory would have been predicted.

—for the Democratic conventions of 1960 and 1972 and the Republican conventions of 1964 and 1976—were based on information available some three to six weeks before these conventions opened.

Figure 2 reports our results in another way. The vertical axis represents votes gained ($S_{t-1} - S_{t-2}$), the horizontal axis the deficit in votes remaining ($Q - S_{t-1}$). For each predicted winner, we have plotted the ordered pair (gain, deficit) associated with that test of support on which he was predicted to win. The other points represent the highest value of the gain-deficit ratio attained by other strong contenders in tests before or on the one on which the winner was predicted, and the diagonal line marks the critical value. The figure makes clear the predictive efficiency of our procedure. Points above the line should be, and are, those of winning candidates; points below the line should be, and are, those of losers. Using a critical value derived from other data and in the face of much happenstance, we were able to make some very close calls.

This last fact, while certainly counting for our procedure, also suggests caution for any inclined to gamble. Credentials contests were crucial to the outcome of the Republican race in 1952 and were arguably so for that of the 1972 Democratic race. Had these contests come out differently, our predictions would probably have been wrong.[4] In a

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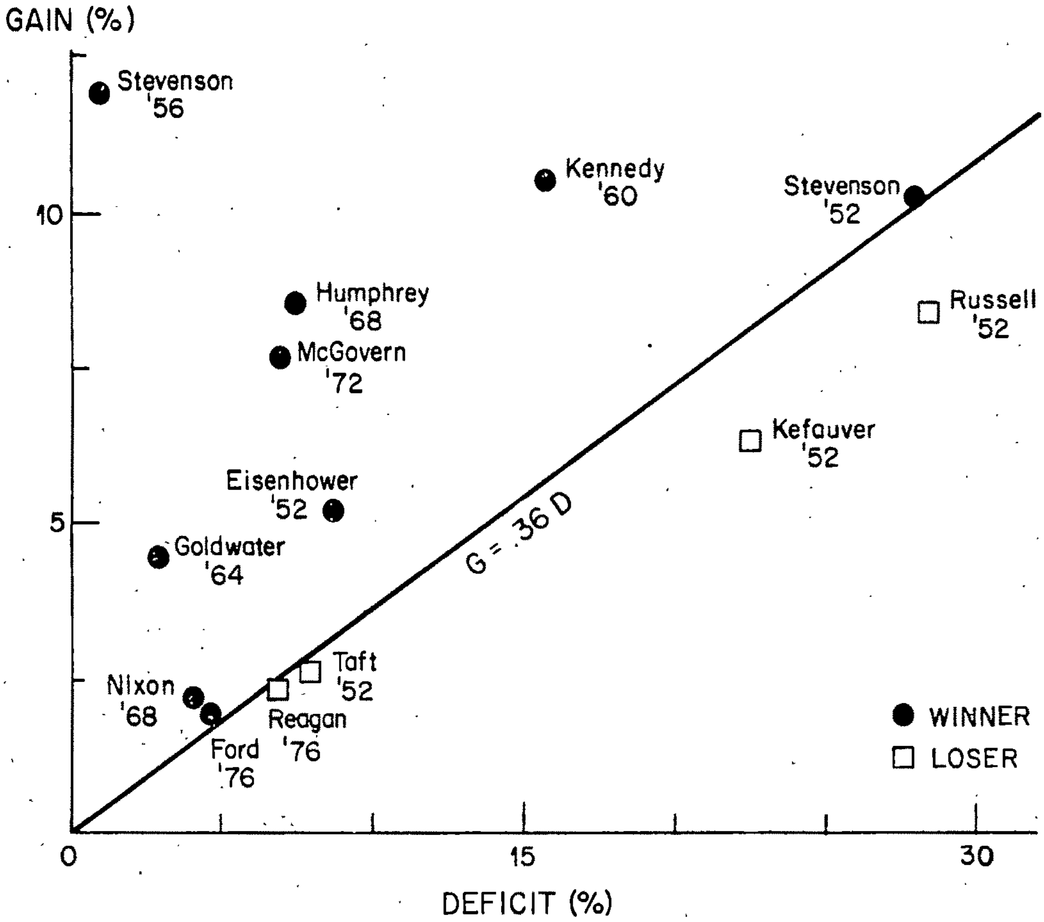
the precise points at which the predictions were made changed somewhat.

[4]Interestingly, in both the *New York Times* and Associated Press surveys McGovern's gain-deficit ratio exceeded 0.360 before the credentials committee (on June 29) had ruled against him on the issue of his entitlement to all the California delegates and again after a court of appeals had ruled in his favor on that issue on July 4.



Sources: Richard C. Bain and Judith H. Parris, *Convention Decisions and Voting Records,* 2nd ed. (Washington, D.C.: Brookings, 1973); Associated Press surveys of delegates reported in the *New York Times* and *Washington Post,* supplemented by Associated Press releases; and a report of a *New York Times* survey, published 29 July 1956.

Figure 2. Winners and Leading Losers, 1952-1976

race as close as the one between Gerald Ford and Ronald Reagan in 1976, small differences in the timing and procedures of surveys could change a prediction without necessarily changing the actual result. In late June of 1976, for instance, the *New York Times* (unlike the *Washington Post* and the Associated Press) began to assign to Reagan the votes of 25 Mississippi delegates. If the *Times* had failed to re-survey between June 27 and June 30, the *Times* data would have forecast a Reagan victory. Further, in early July of 1976 the *Times, Post,* and Associated Press surveys all showed heavy gains by Ford. Aware that time was running out, Reagan announced his selection of a running mate on July 26 and pressed President Ford to do likewise. This maneuver succeeded in unsettling some delegates' preferences, as it was intended to do. While one could argue that the maneuver was doomed to failure because the bandwagon had already begun to roll, slightly different circumstances might have given Reagan the Republican nomination four years before he actually secured it.

As a complication for our procedure, we observed above that the gain-deficit ratios of more than one candidate might reach the critical value simultaneously. In the 20 presidential races now examined, there is no such case. That fact is surely not accidental, since a big gain for one candidate depletes the pool of votes from which big gains for another must come.

In races for the vice-presidential nomination,

Table 3. Predicting the Outcomes of Contested Presidential Nominations, 1952 to 1976 (Critical value of gain-deficit ratio = 0.360)

Convention[a]	Winner Predicted	Gain-Deficit Ratio	No. of Tests of Support	Test No. at Prediction	Prediction Was Possible:
1952 R	Eisenhower	0.602	16[b]	13	One day before balloting
1952 D	Stevenson	0.364	11	9	First ballot (of 3)
1956 D	Stevenson	12.654	8	7	Just before balloting
1960 D	Kennedy	0.672	8	2	2½ weeks before convention
1964 R	Goldwater	1.568	11	2	4½ weeks before convention
1968 R	Nixon	0.519	12[c]	11	Day before balloting
1968 D	Humphrey	1.169	15	13	Day before balloting
1972 D	McGovern	1.113	9	2	3 weeks before convention
1976 R	Ford	0.431	18	4	5¾ weeks before convention

Sources: Richard C. Bain and Judith H. Parris, *Convention Decisions and Voting Records,* 2nd ed. (Washington, D.C.: Brookings, 1973); Associated Press surveys of delegates reported in the *New York Times* and *Washington Post,* supplemented by Associated Press releases; and a report of a *New York Times* survey, published 29 July 1956.

[a] R = Republican, D = Democrat.

[b] The first ballot in which there was shift in votes after the roll call has been counted as two ballots, since Eisenhower fell short of a majority before the shift.

[c] A shift after the roll call has not been counted as a separate ballot, since a majority was attained before the shift.

however, there have been three cases of this sort.[7] In each the second ballot saw two contenders attain gain-deficit ratios above the critical value, as follows:

1924 Republican convention—Lowden, 1.345; Burton, 0.558
1948 Democratic convention—Truman, 1.417; Wallace, 0.375
1956 Democratic convention—Kennedy, 4.688; Kefauver, 0.627.

Our rule for this eventuality—predict to win the candidate with the largest gain-deficit ratio—thus produces two correct predictions and one that is wrong.[8]

Before evaluating our results further, we note a rule of thumb that our hypothetical delegate might use. The number 0.360, which we have used as the critical value of the gain-deficit ratio, is teasingly close to the number one-third. What would happen if one-third were substituted for 0.360?

There would be little change in the accuracy of our procedure. As before, one could predict correctly the winners in all but two of the pre-1952, majority-rule conventions of three or more ballots. One could also make predictions for all the recent conventions that figure in Table 3, and in only one—the Republican convention of 1952—would the prediction be in error.

Some Other Procedures for Prediction

Before adopting our procedure, our hypothetical delegate might compare its results with those of other systems of prediction. Four alternatives are general enough in application to make comparison fruitful.[9] Respectively, they would predict that candidate to win:

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[7] Note that races for second place on a party's ticket are not strictly comparable to those for presidential nomination. A vice-president is not a major dispenser of spoils, and close contests are likely to develop only when the successful presidential candidate, who *is* looked to for spoils, indicates no firm preference for a particular running mate.

[8] The circumstances of the 1956 Democratic race are worth recalling. When the roll call ended on the second ballot, Kefauver's vote stood at 551½ and Kennedy's at 618, with 687 needed to nominate. Senator Albert Gore, Kefauver's junior colleague from Tennessee and the candidate who had run third, then asked his supporters (who included the Tennessee delegation) to support Kefauver, who then prevailed. Analyzing this case, Philip D. Straffin, Jr., shows how a Kefauver victory follows from the assumptions of his model and from the data, once one rules out the possibility of a Gore-Kennedy coalition (Straffin, 1977).

[9] A number of scholars have exploited coalition theory to identify takeoff points for bandwagon voting. See, for instance, Brams and Riker (1972), Brams and

1. who first wins the support of a majority in press polls of delegates;
2. who first receives the support of delegates with 41 percent of a convention's votes;[10]
3. whose gain in votes from the first to the second test of support is greatest;[11] or
4. who has a plurality on the first test of support.

We will call the first of these procedures *the polling-majority rule,* the second *the threshold rule,* the third *the rule of second-ballot gains,* and the last *the front-runner rule.*

Table 4 shows the results of these procedures when applied to contests in which a majority was required to nominate. The first, the polling-majority rule, has an obvious flaw—it yields predictions for only four of the nine recent races. For conventions in the century from 1848 to 1948, the threshold rule has the same shortcoming, though to a lesser extent: it yields seven predictions for eleven cases. One of these, however, is non-unique (Winfield Scott and Millard Fillmore each had more than 41 percent of the vote on the first ballot of the Whig convention of 1852), and another is wrong. The threshold rule holds relatively well for recent conventions: it fails to predict in only one case and yields only one incorrect prediction (for the Taft-Eisenhower race of 1952).[12] Eugene McGregor's "rule of second-ballot gains" permits a prediction in all cases, but five of its eighteen predictions are wrong, and one (that for the Republican convention of 1880) is non-unique. Moreover, in recent races the success of this rule is affected considerably by which surveys one chooses to regard as the first and second "ballots."[13] The front-runner rule is quite successful for recent conventions—predictions in all cases, errors in only two—but very poor for earlier ones—seven errors in eleven cases. Our procedure is less risky than these alternatives.[14]

Having glanced briefly above at the results of contested vice-presidential nominations, we will note here that in all only five such races have gone to more than two ballots in majority-rule conventions: the Whig contest of 1844, the Republican ones of 1868 and 1924, and the Democratic ones of 1944 and 1956. In predicting the outcomes of these, our procedure is correct in four cases, wrong in one. The rule of second-ballot gains does equally well, while the front-runner rule gives three correct and two incorrect predictions, and the threshold rule yields only three predictions altogether, two correct and one incorrect. The polling-majority rule is of course inapplicable to these data.

Concluding Observations

We have shown that an empirically derived rule accounts well for the outcome of multi-ballot,

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Heilman (1974), Brams and Garriga-Pico (1975), Brams (1978), and Straffin (1977). The models developed in these works are of considerable interest, and Straffin's analysis of the 1976 Republican race is particularly suggestive. The assumptions of these models are too restrictive, however, to permit their application to the full set of conventions that we have been analyzing.

[10] This rule adopts and adapts one suggested by Gamson (1962, pp. 65-66), who argued: "What percentage of votes is necessary at a political convention to 'stampede' the delegates for a particular candidate? . . . In majority conventions, the opposition has held firm when a candidate has had as much as 40 percent of the vote but never with more than that in the conventions studied. . . . A candidate with 41 percent or more of the votes when no other candidate has a total close to this generally will have demonstrated to the satisfaction of most of these uncommitted delegates that it is now only a matter of time until he secures the additional votes formally necessary for his nomination." Except for its escape clause concerning other candidates, this procedure amounts in our terms to predicting to win any candidate whose deficit is nine percent or less.

[11] Eugene B. McGregor, Jr. (1974, p. 470) has offered this rule, stated as follows: "The candidate who gains the largest share of the votes from the first to the second ballot will be the eventual winner no matter how many ballots are needed for nomination."

[12] Note that the threshold rule gives an erroneous prediction in the 1952 Republican race and a correct prediction in the 1976 Republican race *as we have stated it.* As Gamson stated the rule (see n. 10), it would yield non-unique predictions for both cases.

[13] We have applied all these alternative rules to exactly the same data to which we applied our own, which means that the first data point was from a survey taken when the roster of delegates was nearly complete, but not entirely so. As noted above, our precise starting point was not important for our results, nor was it in the application of the frontrunner, threshold, and polling-majority rules. Two of the successes and one of the failures of the McGregor rule, however, did depend on the precise starting point that we chose.

[14] One might claim that the success of our procedure in predicting winning candidates is not equivalent to predicting bandwagon voting in those cases in which the initial front-runner did not win. Such a claim would be consistent with a common definition of the bandwagon effect. We do not think this definition a useful one, and it should be distinguished from the one on which we rely: A *bandwagon effect* may be said to exist if some decision maker supports, not the contender he actually prefers, but one who seems more likely to win, and does so *because* that other contender seems more likely to win. This definition is as consistent with the usage described in n. 1 as the more limited sense of the term just noted.

Table 4. Five Procedures for Predicting the Outcomes of Majority-Rule Conventions

C = Correct prediction
E = Incorrect prediction
NP = No prediction possible
N-UP = Non-unique prediction
NA = Data for prediction not available

D = Democratic
R = Republican
W = Whig

												1848W	
												1852W	
												1860R	
												1884R	
						1848W						1888R	
						1852W						1916R	
		1848W				1860R						1920R	
1848W		1860R				1884R					1848W	1940R	
1884R		1884R				1916R					1852W	1948R	
1916R	1852W	1940R				1940R					1960R	1952R	
1948R	1860R	1948R				1948R					1876R	1952D	
1956D	1876R	1956D				1952D					1880R	1956D	
1960D	1880R	1960D				1956D					1884R	1960D	
1964R	1888R	1964R			1876R	1960D	1876R			1952R	1888R	1964R	
1968R	1920R	1968R			1888R	1964R	1888R		1964R	1952D	1916R	1968R	
1968D	1940R	1968D			1916R	1968R	1920R		1968R	1956D	1920R	1968D	
1972D	1952R	1972R	1880R		1920R	1968D	1952R		1968D	1960D	1940R	1972D	1876R
1976R	1952D	1976R	1952R	1852W	1952D	1972D	1976R	1880R	1972D	1976R	1948R	1976R	1880R
C	E	C	E	N-UP	NP	C	E	N-UP	C	NP	NA	C	NP
Front-Runner Rule		Threshold Rule[a]				Rule of Second-Ballot Gains			Poll Majority			Gain-Deficit Rule[b]	

Sources: Richard C. Bain and Judith H. Parris, *Convention Decisions and Voting Records,* 2nd ed. (Washington, D.C.: Brookings, 1973); *New York Times,* 22 June 1852; *Official Proceedings of the Republican Convention of 1880;* Associated Press surveys of delegates reported in the *New York Times* and *Washington Post,* supplemented by Associated Press releases; and a report of a *New York Times* survey, published 29 July 1956.

[a] Threshold = 41.0%

[b] Critical value = 0.360

majority-rule conventions in the period 1848-1948 and that it also accurately predicts the actions of recent conventions. This rule, simple in form, relies not at all on data regarding the ideological affiliations or factional loyalties of delegates; instead, its key element is an event best interpreted as a signal that a particular candidate is likely to win.

Of what relevance is this finding to what real delegates do in real national conventions? Since all delegates do not jump on the bandwagon simultaneously, it is evident that some either do not follow precisely our procedure for predicting the final rush of bandwagon voting or do not vote in accordance with their predictions. It is reasonable to suppose, however, that some delegates intuitively follow a procedure similar to ours in assessing the odds. If bandwagon-jumpers guessed the outcome of contests idiosyncratically, it is doubtful that any procedure could predict their behavior. Our success in predicting actual results, therefore, suggests two conclusions about competition in the convention setting: (1) that delegates' guesses are often based on objective cues, similarly interpreted, and (2) that the *trend* in voting for candidates, in addition to the level of support they attract, is an important element of such cues.

Interpreted with common sense, our findings are consistent with the general conception of delegates' behavior advanced by James P. Zais and John H. Kessel (1973, p. 124). As they see it, a successful candidacy first attracts its ideologically "natural friends," then, as the coalition supporting it grows, depends more and more on size to attract adherents. In the early phases of a race, changing odds will favor shifts of support within groups of like-minded delegates. At the end, delegates, motivated both by sour grapes and by the sweet smell of success, will move toward the eventual winner, led by those with the weakest preferences, the strongest ambitions, and (perhaps) the keenest desire for party unity. If one is to account for the smoothly increasing support received by almost all nominees, however, delegates must have some shared sense of what impends, some roughly equivalent way of estimating the odds.

Our findings also suggest that the collective behavior of delegates in recent years does not contrast sharply with that of their predecessors in the end game of convention politics. Before and after 1952 the same kind of event forecasts the collapse of opposition to the eventual winner. While the number of "purists" among the delegates may have increased, and while multi-ballot conventions have disappeared since 1952, neither phenomenon is inconsistent with a decisive role for bandwagon riders. Writing in 1960, Paul T. David and his associates observed (1960, pp. 319-20):

> Advance polls of delegates (like those conducted by the Associated Press) . . . may themselves influence the outcome. When they reveal that sentiment is turning strongly toward a single outstanding candidate, they accelerate a development that would otherwise take place more slowly. . . . When one candidate is showing strength in delegate polls, other candidates are under pressure to make a similar showing by urging supporters to become publicly committed. . . . Open contests thus tend to develop more openness of information and the stage is set for bandwagon movements even before the actual voting in the convention.

Our results for recent conventions, reached by treating polls of delegates as the equivalent of early ballots, add considerable force to this observation of 20 years ago. Helped along by the press, as is so much else in contemporary politics, the bandwagon still rolls.

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Economic Expectations and Voting Behavior in United States House and Senate Elections

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Past individual-level studies of economic voting (1) have incorrectly operationalized the model they employ by using past-oriented rather than future-oriented questions and (2) have failed to examine the level of economic voting in United States Senate elections. Using the 1978 National Election Study, we show that economic voting exists in Senate but not House elections, presumably due to the differences in electoral context. Even when economic voting occurs, however, there is no guarantee that the public will influence the direction of macroeconomic policy.

Social scientists have given much attention, particularly during the past five years, to the relationship between American voters' personal economic circumstances and their political expression of them at the ballot box. Early efforts to ascertain the extent of voting based on economic concerns looked at the relationships between aggregate economic indicators and election returns over time. Although far from unanimous, the studies on the whole showed that an economic upturn helped congressional candidates of the incumbent party, while economic decline benefited the opposition (cf. Kramer, 1971; Tufte, 1975).[1] Aggregate electoral outcomes do not uniquely specify or describe the behavior of individual voters (Tufte, 1975, p. 826), however, and so more recent research has turned to the national surveys of the Center for Political Studies (Fiorina, 1978; Kinder and Kiewiet, 1979; Weatherford, 1978). Contrary to the findings of the aggregate studies, investigations based on individual-level data have uncovered only scant evidence that voters' assessments of their personal economic situations influence their choices of congressional candidates. Midterm elections, in particular, appear to fall short of being referenda on the incumbent party's economic performance. Thus Kinder and Kiewiet conclude (1979, p. 523): "Private economic experience is important, but not for politics. Economic discontents and political judgments inhabit separate domains."

The undeniable thrust of the individual-level studies notwithstanding, researchers persist in their efforts to identify economic determinants of the congressional vote. This persistence stems in part from a belief by scholars that whom the electorate puts in office can make a difference. Political choices presumably condition macroeconomic outcomes. Another and probably more telling explanation of researchers' pertinacity is that while individual-level data are better suited than aggregate data to uncovering a relationship between economic conditions and citizens' choices of candidates, the results to date run counter to conventional political wisdom. Members of Congress, especially those of the administration's party, view a deteriorating economy much as a parent might view an impending flu epidemic—with fear and trepidation.

So who is right—social scientists armed with survey data or the politicians who must place themselves directly before the voters? Believing as Kernell did in his earlier work (1977) that conventional wisdom should not be taken lightly, we argue below that conclusions about the absence of a linkage between personal economic circumstances and congressional vote may be premature.

While the study of economic voting is important in its own right, it is only part of a more general concern, namely, whether members of the public exert at least some control over the direction of economic policy. Understandably, efforts

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This article was originally prepared for presentation at the Conference on Congressional Elections, Houston, Texas, 1980. We extend our deepest appreciation to David Brady and Joseph Cooper, convenors of the conference. We also owe a special debt to David Knoke of the Department of Sociology at Indiana University, whose insightful comments are reflected throughout the article. Peter Burke of the same department freely gave his time and valuable advice. Others making helpful suggestions include Edward Carmines, Lawrence Dodd, Heinz Eulau, John McIver, Elinor Ostrom, David Webber, and Gerald Wright. The Workshop in Political Theory and Policy Analysis and the Political Science Data Laboratory provided valuable staff support. Finally, Kuklinski would like to thank the Department of Political Science at Indiana University for supporting his attendance at a Workshop on Log-Linear Analysis, Cambridge, Mass., in 1979. Data were made available by the Inter-University Consortium for Political and Social Research.

[1] A complete review of the literature on this topic is beyond our scope here, but is aptly summarized in most work published within the last two years.

to answer this more general question should begin with the study of economic voting; after all, nothing is more fundamental to popular control than the idea that citizens hold officials accountable for their collective actions. But voting on economic grounds is not tantamount to popular control of economic policy. Even when citizens carry out their democratic responsibility, other constraints may limit if not preclude their abilities to affect the macroeconomic performance of government. While it is easy to overlook these constraints when a researcher uses individual-level data, failing at least to take cognizance of them can lead to inappropriate—in particular, overly optimistic—conclusions about how much influence citizens do in fact exert. Accordingly, when we consider the implications of our analysis, we will try to consider some of the constraints on voters' efforts to bring about economic change at the ballot box.

Two Unsettled Matters

Economic Voting as a Prospective Act. The so-called economic voting model is based on the (sometimes implicit) assumption that voters act rationally, which means that they seek to maximize their expected utility (Downs, 1957). Of direct interest here is the term *expected* utility: it emphasizes that as purposive actors, voters are future-oriented in their political decision making, i.e., they base their election decisions on what lies ahead. Specifically regarding economic voting, the congressional vote should be a function of whether voters expect their personal economic situations to improve or deteriorate in the future.

The idea that economic voting as a rational act looks toward the future is clearly evident in the aggregate studies. Arcelus and Meltzer (1975, p. 1233), for example, explicitly use expected utility as an organizing device. Stigler (1973, p. 165), although highly critical of Kramer's pathbreaking study (1971), nonetheless pointedly agrees with him that economic voting is essentially a "forecasting" process. These studies' reliance on indicators of *past* economic conditions—inflation, unemployment, or change in real income, all measured over the past year or two—simply reflects the fact that investigators who use aggregate data are not able to measure voters' expectations directly. They are required to make the assumption (Kramer, 1971, p. 134) that "expectations about year *t* are formed on the basis of experience during the previous year, *t*—1."[2]

It does not necessarily follow, however, that citizens' assessments of past economic circumstances and their expectations about the future will be identical, or even nearly so. To take one real-world example, predictions of a severe recession have increased in number and intensity at the time of this writing. These prognostications undoubtedly have led numerous members of the mass public to conclude that even if conditions have been acceptable up to now, they are about to worsen. In this instance, the researcher's choice of time frame—past or future—could obviously affect conclusions about the impact of economic grievances on congressional voting. More generally, Fair's finding (1978) that the discount rate of voters is high suggests that attempts to incorporate previous periods into the voting model may have limited utility for estimating voter expectations.

The strong possibility of divergence takes on added significance when we turn to the individual-level studies. Although analysts using individual-level data do not lack information on citizen expectations, they have followed in the path of the earlier aggregate studies and also used evaluations of past economic conditions to predict the congressional vote. Typically, they employ this or a similar question: "During the last few years [or in the past year], has your financial situation been getting better, worse, or stayed about the same?"

Because appropriate questions are available, researchers using individual-level data should turn what has been a perhaps unjustified working assumption into an empirical question: *are* voters' evaluations of past economic well-being strongly related to their expectations about future conditions or not? If the two are strongly related, as the aggregate studies *must* assume and the individual-level studies *have* assumed, then the choice of questions should make little difference in terms of the final results. But if judgments of past and future conditions diverge, then researchers should use future-oriented questions that validly operationalize the voting model they employ.[3]

Economic Voting in Electoral Context. A second concern is the failure of researchers to examine the level of economic voting in Senate elections;

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[2] Because of this necessary assumption, researchers using aggregate data have become embroiled in something of a controversy over whether voters look more at the *level* of economic activity or its *change*. This controversy reflects the inevitable frustration involved in attempting to measure expectations indirectly with aggregate data.

[3] Our purpose is not to reject the concept of retrospective voting. The point is that the model of concern is a forecasting model. Fiorina's impressive expansion of his original analysis of economic voting (1980) is an effort to incorporate both types of judgments.

virtually all voting research has focused on the House. So singular has been this focus that the terms "House election" and "congressional election" are now used almost interchangably. Yet there are indications that House and Senate elections differ considerably; these differences support the proposition that economic voting should be greater in one electoral context than the other.

The most widely studied phenomenon in recent years is the so-called incumbency effect (Erikson, 1971). Studies of the electoral advantage of incumbency show a consistently more clear-cut advantage for House than for Senate incumbents. In recent years, the gap has widened (*Congressional Quarterly*, 1979b, p. 1351). This difference in incumbency success raises the very real possibility that House members have more successfully than their Senate counterparts isolated themselves from citizen reaction to in-party economic performance, principally by establishing their roles as ombudsmen and providers of individualized services.

The recent work of Parker and Davidson (1979) captures well the essence of the electoral relationship between House incumbents and their constituents. In an attempt to resolve Fenno's discovery (1975) that voters love their individual members of Congress but hate Congress, they show that the electorate judges Congress in terms of its ability to solve national problems, while it simultaneously evaluates individual members of the House in terms of their reputed ability to provide individualized services. This, they argue, has come about because incumbent House members are able to focus constituents' attention on activities and personal attributes that will enhance their popularity. In other words, they successfully "control" the information—district-oriented and nonpartisan in nature—that voters use to judge them.

If one side of the coin is that the incumbency phenomenon may *discourage* economic voting in House elections, then the other side is that the nature of Senate campaigns may *encourage* it. Because our understanding of Senate campaigns and elections is still in its infancy, the evidence is indirect at best. Nonetheless, Hinckley (1979) has demonstrated that House challengers are considerably less visible to the electorate than Senate challengers, which helps to explain, in turn, why Senate elections are more competitive. Similarly, citizens are more likely to express negative feelings toward Senate than House incumbents, a fact apparently due to the more controversial nature of Senate campaigns (Abramowitz, 1979). Taken in conjunction, these research findings leave the impression that discussions of salient problems and solutions to them are an integral part of most Senate campaigns. When issues, and—in light of our specific concern—economic issues in particular, are forcefully debated, voters should at least become more sensitized to the connection between their own economic situations and the candidate of the in-party.[4]

The observations of practitioners strike the same theme. Democratic pollster Michael Barone has argued, for example, that senators "are just high enough on the political ladder for voters to throw them out of office as an expression of political anger or frustration" (*Congressional Quarterly*, 1980, p. 906). And William Hathaway, former senator from Maine, has noted that "Senators are perceived [by voters] as being more responsible [than House members] for what is wrong" (*Congressional Quarterly*, 1980, p. 906). Again, the implication is that the tie between important issues and in-party candidates is strong in Senate elections, presumably due to the nature of Senate campaigns.

Our intention is neither to elaborate in detail the differences between House and Senate elections nor to explain them. We *are* suggesting that any conclusion about the presence or absence of economic voting in congressional elections must be based on an analysis of both House and Senate voting patterns. For there is ample reason to believe that the nexus between personal economic situation and political expression will be greater in one electoral context than the other.

The 1978 CPS Election Study

To address the concerns posed above, we use data collected in the 1978 National Election Study. Designed explicitly as a study of congressional voting, the 1978 survey serves our purpose in several ways. In light of Fiorina's conclusion (1978) that voters' personal economic situations and their congressional votes are consistently unrelated in off-year elections, the survey's midterm timing provides a stern test of the economic voting thesis. For the first time in some years, moreover, economic issues were the singularly important concern of the mass public. With Watergate and Vietnam behind us and race relations at least temporarily put aside, the state of the economy became the focal point of political attention in 1978. As a statistical matter, the absence of noneconomic factors renders specification error a less worrisome problem.

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[4] It might be argued that this statement runs counter to the notion of economic voting, which, as originally construed (Kramer, 1971), requires little information on the part of the voter. Given the consistent presence of economic voting in presidential elections, it is reasonable to assume that competitive and visible campaigns can at least provoke voters to think in terms of blame.

Another reason to use the 1978 survey is that party control of government was total at the time the data were collected. Most previous investigations—the longitudinal studies in particular—cover periods in which there has been divided control of Congress and the presidency. Researchers have assumed that under such circumstances voters equate the incumbent party with the incumbent president's party, presumably because they see the chief executive as responsible for the economic health of the nation. In light of evidence that voters do not agree on who controls economic policy—the president, Congress, or both—the safest research strategy is to avoid *any* assumption about the most likely target of citizen wrath, which can be done only when the same party controls both institutions.[5]

The data have some limitations, however. One disturbing feature is the rather dramatic discrepancy between the vote reported in the National Election study and the actual vote. Fifty-four percent of the sample indicate they had voted in 1978, while the estimate of the actual vote is 35.1 percent (*Congressional Quarterly*, 1979a, p. 574). This considerable difference between reported and actual turnout, which is characteristic of all SRC-CPS surveys, reflects both an unwillingness by some respondents to admit their absence from the polling booth and a reluctance to participate in an election study among citizens with little interest in politics (Traugott and Katush, 1979). Unfortunately, the discrepancy is a form of systematic error, and there is no foolproof way to estimate its effect on the coefficients we present below.

To complicate matters further, Jacobson (1980) has convincingly argued that the House elections in the sample overestimate the strength of incumbents, while Mann and Wolfinger (1979) have shown that the CPS respondents who voted in Senate elections are concentrated in three states. Because we wish only to examine the level of economic voting within two different electoral contexts, these sampling problems are not prohibitive. From our theoretical perspective, the subsamples of House and Senate voters function essentially as a dichotomous variable, in accord with the chamber differences that we noted in the previous section. On the other hand, the National Election Study probably is not a good source of information from which to generalize about all 1978 House and Senate races. Accordingly, we refrain from making grand statements about the 1978 congressional elections.

Personal Financial Condition and the Midterm Vote

The economic voting model presumes a relationship between expected financial well-being and vote. Because previous research has relied on assessments of respondents' past financial condition, the first question we must consider is whether retrospective and prospective judgments are sufficiently related such that one can justifiably be substituted for the other.

Two CPS items allow us to address this concern; they are as follows:

- We are interested in how people are getting along financially these days. Would you say that you are better off or worse off financially than you were a year ago?
- Now looking ahead—Do you think that a year from now you will be better off financially, or worse off, or just about the same as now?

Both questions obviously deal with personal financial conditions; the first measures perceptions of immediately past trends, the second expectations about the upcoming year.[6] Table 1 reports the relationship between answers to these two questions among individuals reporting to have voted in 1978. Since Senate elections were not held in all House districts for which there are respondents, we have presented the results for each of the two subsamples of voters.

Voters' estimates of past and future financial well-being fall far short of being identical. In terms of those voting in House elections, 51 respondents or 5.2 percent of the total subsample who feel their financial circumstances are better than they were a year ago also predict that they will be worse in the future. At the other extreme, 4 percent of the respondents believe that even though conditions have gotten worse, they are likely to improve in the future. More generally, nearly 50 percent of the respondents do not fall on the diagonal, i.e., their perceptions of past and future financial well-being diverge. Similar patterns hold among the Senate subsample.

Although it does not necessarily follow that the use of retrospective and prospective judgments will lead to different conclusions about the presence or absence of economic voting, the possibility clearly suggests itself. One particularly valuable technique for gauging the impact of each type of judgment on citizens' political decisions is log-linear analysis. Increasingly used by sociologists, log-linear analysis was developed specifically to provide concise measures of the relationships among categorical variables such as those comprising the economic voting model (Goodman, 1972, 1973; see also Knoke and Burke, 1980).

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[5] As part of the 1978 CPS survey, respondents were asked to indicate who, the president or Congress, has more say in handling the two major economic problems facing the country—inflation and unemployment. In both areas, only about 15 percent agree with the traditional assumption that the president has more say; another one-third express just the opposite view; and the large bulk believe that responsibility is equally divided.

[6] To assure ourselves that these two measures do not simply reflect socioeconomic status, we examined the relationships between them and selected characteristics of the respondents. Associations tended to be weak.

Log-linear analysis consists of two logically and practically distinct steps. In the first step, the researcher uses odds and conditional odds ratios to select the most parsimonious and theoretically interesting model from a set of hierarchical models.[7] A well-fitted model is one in which the predicted and actual cell frequencies are similar. The second step involves estimating the strength and direction of the relationships among the variables comprising the chosen model. Either multiplicative or additive parameters can be used to estimate the relationships, although the tendency is toward the latter.[8] When one of the variables is construed as a dependent variable, as is the congressional vote, the log-linear model is conceptually equivalent to a logit model and the appropriate additive parameters are betas (ßs). The ßs are not as readily interpretable as those in regression analysis, but their magnitudes can be compared to ascertain the relative strengths of the relationships.

Table 2 lists, for contested House and Senate elections, a number of log-linear models in which vote is the dependent variable and region and partisan identification, along with past financial condition, are independent variables. Two brief comments about them are in order. First, even though we will focus our attention only on those parameters involving the dependent variable, each of the models includes a term (RPF) that incorporates all possible interactions among the independent variables. This procedure is analogous to regression analysis, which takes the correlations among the independent variables into account even though these relationships are not explicitly included in the equation (Knoke and Burke, 1980, pp. 24-25). Second, out of the complete set of hierarchical models, we have limited analysis to the models that are directly relevant to our theoretical concern of whether personal economic

Table 1. Relationship Between Past and Expected Financial Well-Being Among Voters in House and Senate Elections

Voters' Past Financial Well-Being	Voters' Expected Financial Well-Being					
	House Elections			Senate Elections		
	Better	Same	Worse	Better	Same	Worse
Better	130 (13.3)	20 (2.0)	51 (5.2)	70 (12.8)	85 (15.7)	15 (2.8)
Same	173 (17.7)	199 (20.3)	106 (10.8)	10 (1.8)	134 (24.7)	41 (7.6)
Worse	39 (4.0)	59 (6.1)	202 (20.6)	27 (5.0)	60 (11.1)	100 (18.5)
		Gamma = .56 Tau$_b$ = .39			Gamma = .57 Tau$_b$ = .38	

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

Note: Figures represent absolute frequencies; percentage of total subsample is given in parentheses.

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[7] A hierarchical model obeys the rule that if a parameter involving a set of parameters S is included in the model, then the model must also include all the parameters involving any subset of S. Consider for example, the three-variable model

$$F_{ijk} = \eta\tau_i^A\tau_j^B\tau_k^C\tau_{ij}^{AB}\tau_{ik}^{AC}\tau_{jk}^{BC}\tau_{ijk}^{ABC},$$

where F_{ijk} is the expected cell frequency in cell $_{ijk}$ and the parameters τ_i^A, τ_j^B, τ_k^C represent the main effects or marginal distributions of each variable and τ_{ij}^{AB}, τ_{ik}^{AC}, τ_{jk}^{BC}, τ_{ijk}^{ABC} represent two- and three-way interactions. This model, called the saturated model because it includes all possible interactions and thus must perfectly fit the data, must also include the model

$$F_{ijk} = \eta\tau_i^A\tau_j^B\tau_k^C\tau_{ij}^{AB}\tau_{ik}^{AC}\tau_{jk}^{BC}\tau_{ijk}^{ABC}.$$

[8] The multiplicative model is transformed into an additive one by taking natural logarithms.

Table 2. Log-Linear Models for Vote, Region, Partisan Identification, and Past Financial Well-Being Among Voters in House and Senate Elections

	Likelihood-Ratio Chi-Square		P-Value		
Model	House Elections	Senate Elections	House Elections	Senate Elections	Degrees of Freedom
(VRPF)	0.00	0.00	–	–	0
(RPF) (VRP) (VPF)	2.96	2.83	>.5	>.5	6
(RPF) (VRP) (VF)	9.56	8.05	.480	>.5	10
(RPF) (VPF) (VR)	4.47	9.70	>.5	.286	8
(RPF) (VPF)	20.39	10.04	.016	.347	9
(RPF) (VR) (VP) (VF)	11.21	14.36	>.5	.278	12
(RPF) (VR) (VP)	18.63	17.82	.180	.215	14
(RPF) (VP) (VF)	26.52	14.64	.014	.330	13
(RPF) (VP)	34.15	18.03	.003	.261	15
(RPF) (V)	264.30	156.09	.000	.000	17

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

Key:
V = Vote
R = Region
P = Partisan Identification
F = Past Financial Well-Being

conditions independently affect House and Senate votes. In essence, we have let theory dictate the models to test rather than use every model and simply let the data choose the best-fitting one.

The simplest model is (V). Fitting only the marginals of the dependent variable, it hypothesizes no significant relationship between any of the independent variables and House or Senate vote. At the other extreme, the saturated model (VRPF) includes all four variables and all possible interactions among them. Identifying the most parsimonious model that also fits the data begins with a comparison of (V) and (VRPF) to determine whether vote is unrelated to all three independent variables—region, partisan affiliation, and past financial situation. The test involves subtracting the maximum-likelihood chi-square values (L^2s) and comparing the difference to the difference in degrees of freedom. Looking at the subsample of those voting in House elections, Table 2 shows that the difference in L^2 (264.30 — 0) is significant at the .001 level with 17 degrees of freedom (17 — 0), which means at least one of the independent variables must be included in the final model. We thus proceed to (VR)(VP)(VF), which includes the relationships between each independent variable and vote, controlling for the other independent variables. The question is whether (VR)(VP)(VF) is a better fit to the data than the simpler model (VR)(VP), which does not include past financial condition as an independent variable. It hypothesizes, in other words, that region and partisan identification independently affect the House vote, while individuals' evaluations of changes in their economic situations do not. Comparing (VR)(VP)(VF) to (VR)(VP) indicates that the addition of (VF) results in a substantial reduction in L^2 at the cost of only two extra degrees of freedom; past financial condition cannot be dropped from the model. Similarly, although of secondary importance, region must be included in the final model.

At this point, our model is (VR)(VP)(VF). The final question is whether a model incorporating interaction effects among the variables fits the data significantly better than (VR)(VP)(VF). Accordingly, we compare the independent effects model to the more complex models. Substantively, for example, (VPF) posits that the level of economic voting varies by partisan identification. In each instance, the small reduction in L^2 relative to the cost in degrees of freedom, combined with the *p*-value of (VR)(VP)(VF), leads to acceptance of the simpler model.[9] Applying the same procedure to the subsample of Senate voters, we choose

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[9] An investigator normally looks for large chi-squares and correspondingly low *p*-values. Log-linear analysis reverses this logic. The chi-square difference and the *p*-value tests can occasionally lead to different conclusions about the most appropriate model, in which case theoretical considerations must dictate the final choice.

We might also note here that the L^2-difference test involving the final and the no-effects models is analogous to R^2 in regression. The L^2 of (V), for example, is 264.30, while the L^2 of (VR)(VP)(VF), the final model, is 11.21. Taking the ratio

$$\frac{264.30 - 11.21}{264.30},$$

(VP) as the most parsimonious model that closely fits the data.

The appropriate βs are in Table 3. As expected, there is a strong relationship between voters' partisan affiliations and their votes in both House and Senate elections. In addition, region is statistically significant among the House subsample: southerners are more likely than non-southerners to vote Democratic.[10] More important, in neither electoral context do voters' assessments of changes in their past financial well-being condition their choices of candidates. Among those voting for candidates for House seats, neither those who believe that their personal financial situations have improved nor those who think that conditions have worsened give any indication that their political choices are a function of their economic evaluations. And, of course, the economic variable is not even included in the final model for voters in Senate elections. Our first cut of the data leads to a conclusion similar to that drawn in earlier studies: economic experience and political decision making are unrelated among the mass public. This conclusion applies to House and Senate elections alike.

Table 3. Beta Effects for Best-Fitting Log-Linear Model Involving Past Financial Well-Being Among Voters in House and Senate Elections

	β Parameters in Model	
Independent Variable	House Elections	Senate Elections
Region		
South	.412*	
Partisan Identification		
Democrat	1.452*	1.476*
Independent	−.118	−.186
Republican	−1.334*	−1.290*
Past Financial Well-Being		
Better	.160	
Same	−.330*	
Worse	.170	

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

*$p < .05$

Our initial discovery does not rule out the existence of economic voting, for as we noted earlier, economic voting implies a relationship between voters' *expected* financial condition and their vote. Table 4 lists the set of hierarchical models in which expected financial well-being has been substituted for respondents' perceived change in their financial situations during the past year. In terms of the L^2-differences and the *p*-values, the simplest acceptable model among voters in House elections is (VR)(VP). Expected financial well-being is not included in the model. Among those voting for senator, on the other hand, an accurate representation of the relationships in the data is given by the model that posits independent effects of partisan identification and expected financial situation on political choice.

Table 5 reports the appropriate coefficients showing the strength and direction of the relationships for each subsample of voters. Partisan identification and region once again are important and separate determinants of the vote among individuals voting in House elections. While party affiliation also continues to condition the vote among the Senate subsample, more noteworthy is the fact that expectations about financial well-being during the upcoming year are significantly and strongly related to support for Senate candidates. Individuals who feel that their personal financial situations are likely to deteriorate are considerably more inclined to vote Republican than is expected under conditions of independence. Similarly, support for Democratic candidates is strong among Senate voters who anticipate an upward swing in their personal financial situations. Using individuals' *expectations* about their financial conditions, as the economic voting model requires, we find evidence that voters draw upon their personal economic circumstances to make midterm judgments.[11] But such judgments are

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we can interpret the final model as explaining approximately 95 percent of the "original unexplained variance." It is important to remember that this ratio and R^2 are not equivalent.

[10] In the ECTA program, tests of statistical significance make use of the standardized parameters, which, strictly speaking, apply only to the saturated model. These parameters define the lower bounds on the estimated effects of unsaturated models and thus are conservative measures of significance of association.

[11] In addition to region and party, we controlled for the effects of incumbency and evaluations of presidential performance. The incumbency control is particularly important given the Senate sampling problems. Having found that region has no independent effect on the Senate vote, we substituted each of these two controls for it and thus avoided the problem of numerous empty cells. In both instances, our conclusions remain intact, although the addition of incumbency reduces the relationship between expected financial well-being and Senate vote slightly.

Another potential control variable is candidate preference, as measured by the relative rankings of the incumbent and the challenger on the feeling thermometer. Mann and Wolfinger (1979, p. 22) have noted, how-

Table 4. Log-Linear Models for Vote, Region, Partisan Identification, and Expected Financial Well-Being Among Voters in House and Senate Elections

Model	Likelihood-Ratio Chi-Square House Elections	Likelihood-Ratio Chi-Square Senate Elections	P-Value House Elections	P-Value Senate Elections	Degrees of Freedom
(VRPF')	0.00	0.00	–	–	0
(RPF') (VRP) (VPF')	8.79	4.73	.186	>.5	6
(RPF') (VRP) (VF')	11.54	5.68	.317	>.5	10
(RPF') (VPF') (VR)	9.28	9.63	.319	.292	8
(RPF') (VPF')	26.00	9.63	.002	.381	9
(RPF') (VR) (VP) (VF')	12.12	10.58	.437	>.5	12
(RPF') (VR) (VP)	14.68	17.43	.401	.234	14
(RPF') (VP) (VF')	28.60	10.58	.008	>.5	13
(RPF') (VP)	31.17	17.45	.008	.293	15
(RPF') (V)	251.34	145.57	.000	.000	17

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

Key:
V = Vote
R = Region
P = Partisan Identification
F' = Expected Financial Well-Being

selective: they occur in Senate but not House elections. The context within which voting takes place apparently is an important determinant of whether the most fundamental of political acts functions as an expression of economic concern.

Employment Status and Choices of Candidates in House and Senate Elections

One of the central characteristics of the economic voting model is its short-term perspective. It predicts that voters' short-term expectations about their own economic circumstances will influence their judgments about congresssional candidates. We identified such behavior with regard to citizens' anticipated financial well-being, but again, only within the context of Senate elections.

Although it has generally gone unnoticed, there is one economic problem on which the utility of the model is inherently limited—unemployment among the electorate. While the short-term perspective of the model makes sense with regard to very general economic concerns, such as those we have been considering, the distinct and consistent positions of the two parties over time on this most

Table 5. Beta Effects for Best-Fitting Log-Linear Model Involving Expected Financial Well-Being among Voters in House and Senate Elections

Independent Variable	β Parameters in Model House Elections	β Parameters in Model Senate Elections
Region		
South	.436*	
Partisan Identification		
Democrat	1.446*	1.470*
Independent	–.090	–.198
Republican	–1.356*	–1.272*
Expected Financial Well-Being		
Better		.426*
Same		–.044
Worse		–.382*

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

*$p < .05$

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ever, that "94 percent of all respondents who prefer the [House] incumbent vote for the incumbent; 88 percent of all respondents who prefer the challenger vote for the challenger." A similar relationship obtains among Senate candidates. These extremely strong relationships indicate, in accord with extant psychological research, that candidate preference and vote are, for all practical purposes, one and the same. To control for the former is to control for the latter, which is what we are trying to explain. Moreover, Hinckley (1979) has suggested that the thermometer ratings are best understood as summary measures that themselves are affected by various influences. Since our purpose is to estimate the *independent* effects of theoretically relevant influences, inclusion of candidate preference would only serve to contaminate our analysis.

debilitating of all economic problems portends a long-term perspective. Historically, the Democratic party has taken stronger positions on the unemployment problem than the Republican; the unemployed should be especially sensitive to these party differences when the president and Congress are faced with redressing *both* severe unemployment and severe inflation, as was true in 1978. Such sensitivity is reflected in the data. Among the currently unemployed who voted in 1978 (at least as ascertained by the CPS survey), 24.5 percent believe the Democratic party is more competent on unemployment than the Republican; only 9.1 percent feel the Republican party is more capable.[12] In essence, despite their current status and contrary to the prediction of the economic voting model, the unemployed should, on the whole, be inclined to vote Democratic because they anticipate that a Democratic administration will be more likely than a Republican administration to improve their personal situations in the future.

The 1978 CPS survey includes a large number of questions about the respondent's employment status. In the name of parsimony, we use a dichotomous variable that categorizes the respondent as currently employed or unemployed.

The data in Tables 6 and 7 bear out the prediction that the unemployed will vote disproportionately Democratic, *but once again the nexus between personal economics and political choice is limited to the context of Senate elections.* On the one hand, as is consistent with our earlier findings, voting in House elections is a function solely of partisan identification and region; employment status has no independent effect of its own. Within the context of Senate elections, on the other hand, the unemployed are considerably more likely to vote Democratic than is predicted on the basis of chance alone. The historical records of the two parties on unemployment seemingly are not lost on those most directly affected.

Like expected financial condition, then, employment status conditions the vote for candidates to the upper but not the lower chamber of Congress. While one set of economic judgments is based on a short-term perspective, the other is based on the accumulation of experience over time.

Economic Voting and Popular Control of Economic Policy

Sampling problems associated with the 1978 national survey recommend a cautious and restrained concluding statement. Nonetheless, there was, in 1978, a significant relationship between citizens' expected financial well-being and their votes in at least some (and we suspect most) Senate races. We also found a strong association between employment status and the Senate vote. For reasons not totally known but presumably having much to do with the incumbency phenomenon, on the other hand, there was no evidence of a link

Table 6. Log-Linear Models for Vote, Region, Partisan Identification, and Employment Status Among Voters in House and Senate Elections

	Likelihood-Ratio Chi-Square		P-Value		
Model	House Elections	Senate Elections	House Elections	Senate Elections	Degrees of Freedom
(VRPE)	0.00	0.00	–	–	0
(RPE) (VRP) (VPE)	2.45	0.84	.484	>.5	3
(RPE) (VRP) (VE)	4.33	1.04	>.5	>.5	5
(RPE) (VPE) (VR)	3.32	5.89	>.5	.317	5
(RPE) (VPE)	18.68	5.91	.005	.433	6
(RPE) (VR) (VP) (VE)	5.29	6.14	>.5	>.5	7
(RPE) (VR) (VP)	7.46	23.55	.488	.003	8
(RPE) (VP) (VE)	21.09	6.16	.007	>.5	8
(RPE) (VP)	23.09	23.57	.006	.005	9
(RPE) (V)	255.86	157.58	.000	.000	11

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

Key: V = Vote
R = Region
P = Partisan Identification
E = Employment Status

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[12] As expected, similar perceptions prevail among the employed.

Table 7. Beta Effects for Best-Fitting Log-Linear Model Involving Employment Status Among Voters in House and Senate Elections

	β Parameters in Model	
Independent Variable	House Elections	Senate Elections
Region		
South	.404*	
Partisan Identification		
Democrat	1.428*	1.452*
Independent	–.116	–.204
Republican	–1.312*	–1.248*
Employment Status		
Unemployed		.740*

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

*$p < .05$

between personal economic condition and political decision in House elections. We conclude that the electoral context makes a difference with regard to the presence or absence of economic voting. Indeed, only when the nature of the context is explicitly taken into account do the reported relationships take on full meaning.

As we noted earlier, the significance of our analysis extends beyond the conclusion that context matters to a fundamental democratic concern: citizen control and direction of national policy. From the perspective of popular control, voting on the basis of economic considerations is not only desirable but essential, for otherwise public sentiments for change are unlikely to be translated into public policy (cf. Asher and Weisberg, 1978). It might be argued, of course, that popular control of economic policy itself is not desirable. Keech (1980, p. 366), for example, on the basis of his innovative work, concludes that "the [macroeconomic] policy results of a fair and open electoral process are not necessarily optimal."

Assuming, nonetheless, that popular control of public policy remains a central tenet of contemporary democratic theory, what are the implications of our results? The temptation is to suggest that popular control exists, but only within the context of Senate elections. We refrain from this conclusion, however, out of a recognition that economic voting is only a necessary condition of citizen influence on government performance. Because of formidable constraints inherent in the political system, there is no guarantee that voters will bring about economic change even when they act on their grievances.

Consider, as a starting point, the obvious fact that individual voters can control policy only through collective action. But if *individuals* are *collectively* to control economic performance, there must be widespread consensus among them. Such consensus is precisely what Key (1964, pp. 562-68) had in mind when he discussed popular control in terms of "broad sweeps of sentiment." At least as was ascertained by the 1978 CPS survey, this condition did not obtain in that year, for citizens were nearly evenly split in their assessments of future financial conditions. Votes cast on economic grounds undoubtedly neutralized each other in many instances.

Let us assume, nonetheless, that a very clear consensus existed in 1978. Even then the electorate was able to vote for only one-third of the total Senate membership. Because of the institutional arrangements established by the Founding Fathers, voters cannot simultaneously make judgments on all the men and women who determine the direction of economic policy. Although they admittedly can use their votes to send a "message," election returns are not readily interpretable as policy directives.

A more illuminating if not more significant constraint has to do with the options available to the voters. The unemployed represent a case in point. Unemployment was high in 1978 and at the time of the election there was no reason to believe that it would lessen. Yet the party widely perceived as having the best track record on unemployment was in office and had been there for some time. The unemployed, in 1978, could vote for the party that historically had been sensitive to their needs but was now floundering badly; they could opt for a change and vote contrary to past experience; or they could abstain. The options available to the unemployed to improve their lot were, needless to say, limited. Although they remained overwhelmingly loyal to the Democratic party, they presumably did so without great enthusiasm.[13]

More generally, the evidence of the 1970s points to the national government's growing inability to carry out effective macroeconomic policy. Traditionally the conflict has been over the relative priority of inflation and unemployment. One could be reduced, even though at the expense of the other. But the last two administrations—one

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[13] Even if a viable and meaningful option had been available, the unemployed are, as a group, sufficiently small and dispersed among individual congressional districts and states to preclude them from directly determining who will and will not hold office. The unemployed are largely dependent on the employed to express a concern for the unemployment problem.

Republican, the other Democratic—have been unable to control either. It is beyond the scope of this article to try to explain these trends, although factors outside the United States political system, such as Arab oil prices, and the apparent deterioration of the party system in Congress suggest themselves as likely candidates. The point is that when the options available to government are reduced, so are the opportunities of the electorate to effect meaningful economic change.

Given these constraints, it is not too surprising that individual voters support House incumbents who have already shown that they can effectively deliver the goods upon individual request. Indeed, it may be the *most* rational choice for the individual voter who has benefited from personalized constituent services in the past—and expects to benefit in the future. The question may be less why citizens do *not* relate their economic concerns to their votes in House elections than why they *do* in Senate elections. If Senators should follow the path of House members and become increasingly effective in activities that directly benefit individual constituents, even this question may become moot. The paradox, of course, is that as individual constituents increasingly hold individual representatives responsible for provision of particularized services, collective influence on the direction of government policy making diminishes.

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Policy-Oriented Voting in Response to Economic Issues

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This study explores the hypothesis that voting in response to economic problems is policy-oriented: voters concerned about unemployment are predicted to give greater support to Democratic candidates, while those concerned about inflation are predicted to vote more Republican. In light of evidence from previous research, this study investigates the electoral effects of inflation and unemployment as (1) problems directly experienced by the individual, and (2) problems deemed serious for the nation as a whole. Support is strongest for the unemployment side of the hypothesis. Voters personally affected by unemployment gave a modest boost to Democratic candidates in virtually every election. And in years of high unemployment the large percentage of voters who felt it was a serious national problem voted heavily Democratic as well. This study concludes by discussing the important implications these findings have for our understanding of how economic conditions influence voting behavior in American national elections.

Recently several studies have appeared concerning the impact of economic concerns upon voting. Largely motivated by a desire to find patterns of individual behavior consistent with the strong aggregate relationships which Kramer (1971) and others have uncovered, these studies focused on an *incumbency-oriented* hypothesis, i.e., the theory that voters support or oppose the incumbent president and candidates of his party in response to fluctuations in economic conditions.

Most of these survey analyses have found clear evidence of incumbency-oriented voting. Interestingly, the major source of support for the hypothesis appears not to be individuals' assessments of their own personal financial situation. Such perceptions exerted only weak, scattered effects upon voting in congressional, senatorial, and gubernatorial elections, and more consistent but still quite modest effects in presidential races (Ben-Gera Logan, 1977; Fiorina, 1978; Klorman, 1978). Rather, it is assessments of the *nation's* economy which appear to have the greater impact. Kinder and Kiewiet (1979) found that voters who felt national business conditions had recently worsened were consistently more likely to vote against congressional candidates of the president's party than voters who believed conditions had improved. The evidence was similarly supportive of this proposition in senatorial and presidential races (Ben-Gera Logan, 1977; Fiorina, 1979).

The concentration of work in this field upon the single hypothesis of incumbency-oriented voting, however, implies some serious limitations in our knowledge of how economic concerns influence voting decisions. This study addresses at least one major deficiency in this literature by investigating the hypothesis, suggested by Stigler (1973) and others, that voting in response to economic problems is *policy-oriented,* especially with regard to inflation and unemployment. Given the different policy priorities of the two major parties (Sundquist, 1968; Okun, 1973; Hibbs, 1977; Tufte, 1978), a policy-oriented hypothesis predicts that, everything else being equal, voters concerned about unemployment will tend to support Democratic candidates, while voters more concerned about inflation will opt for the Republicans.

To characterize such voting as "policy-oriented" is not to imply that voters need a sophisticated understanding of fiscal or monetary policy instruments. Neither must they believe anything in particular about the empirical relationship between inflation and unemployment, nor recognize that full employment and price stability pose conflicting goals. Rather, it requires only that they (1) see either inflation or unemployment as a serious problem and want to see it alleviated, and (2) perceive differences between the parties in the amount of effort and/or skill they apply in combating that problem.

One other point deserves comment before we proceed. This hypothesis seems to imply, paradoxically, that when in office the Republicans could bolster their reelection prospects by running up a high inflation rate, while the Democrats have the same perverse incentives with regard to unemployment. Such a "strategy," however, would almost certainly be counterproductive. A complete flip-flop on macroeconomic policy priorities would betray the party's key constituent groups and alienate some of its most dependable supporters. The resulting uncertainty as to what the party could be expected to do in office would also scare off many others (Downs, 1957). And as Okun (1973) points out, a party's long-term favorable image vis-à-vis unemployment or inflation would surely be eroded by such policy failures.[1]

The following analyses thus examine the hypothesis that voting in response to inflation and unemployment is policy-oriented. In light of the evidence on incumbency-oriented voting, this study will investigate the impact of (1) inflation and unemployment as personally experienced economic problems, and (2) perceptions of inflation and unemployment as serious national problems. The survey data used come from the biennial CPS American National Election Studies.[2]

The Effects of Concern over Inflation and Unemployment upon Voting in National Elections

Given their focus on incumbency-oriented voting, studies in this area have relied mainly upon the same measures of perceived trends in one's own financial situation. A couple of previous analyses, however, did look at personal difficulties due to unemployment (Kinder and Kiewiet, 1979; Fiorina, 1980). Kinder's and Kiewiet's analysis used a dummy variable which registered whether or not the respondent's head of household had been out of work at some time in the last year or two. This measure did no better than the personal finances item in predicting votes for or against the incumbent party's candidates. When their results are examined from the perspective of policy-oriented voting, however, a striking pattern emerges. After making a few desirable changes in their estimation procedures, I present these results in Table 1. The top numbers in each entry are the probit estimates, the numbers in parentheses beneath them are standard errors. As in their original analysis, the personal (family) finances item was also specified (the "better off," "same," and "worse off" categories taking on values of 1, 3, and 5, respectively) as were party identification terms.[3] Estimates of the latter, of course, were always quite large and statistically significant. This has no direct bearing on the analysis here, however, and so for the sake of conciseness I do not report these estimates. Reported votes for Democratic candidates are represented by 0, and reported Republican votes by 1, so positive signs are in a pro-Republican direction.

As previous studies had shown, effects associated with personal financial assessments were generally in keeping with incumbency-oriented voting. Also as before, effects were weak and erratic in congressional elections, but somewhat stronger in presidential elections. Signs of the estimates were in the correct (incumbency-oriented) direction in all six presidential elections, and four of the six were statistically significant. Only eight of the 12 estimates for congressional races were consistent with incumbency-oriented voting, and only three were statistically significant (the significant 1966 estimate was in the wrong direction).

None of this is very new or exciting. What is striking about Table 1 is that the sign of the head of household's unemployment term is in a negative and thus pro-Democratic direction in 18 of the 19 elections studied. The only exception is the 1968 presidential race, an election in which George Wallace made substantial inroads into the Democratic party's traditional blue-collar constituency. Whether or not a Democrat sat in the White House, respondents from households whose head had been out of work consistently gave more support to candidates of the Democratic party.

As with the personal finances measure, estimates for the unemployment term were stronger in presidential elections than in congressional ones. Weighting each estimate by the inverse of its standard error, the mean estimates were —.383 and —.235, respectively. This makes sense, given the lower salience on congressional races (Stokes and Miller, 1962), and that the president is probably held more directly accountable for economic conditions than his party's House candidates. But the probit estimates varied widely in size; in six elections they exceed .5, but in four others they are less than .1. And only five of the nineteen attain convention levels of statistical significance. True, in 1956 and 1966 the unemployment measure reflects current status only, and the low N (in 1956 only 15 respondents reported that their head of household was currently unemployed) make statistical significance difficult to achieve. But low N is usually not the problem; an average of 40 voters reported their head of household had been out of work in the previous six months, and when the previous year is considered the average is 108. So while voters from families which have been affected by unemployment appear to give consistently greater support to Democratic candidates, the magnitude of this support is only occasionally impressive.

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[1] To be sure, the previous Democratic administration would seem to have adopted this strategy. President Carter fully supported the Federal Reserve's credit-tightening moves which helped trigger the 1980 recession. The argument here, though, is not that the Democrats care only about unemployment and pay no heed to inflation, while the Republicans are prepared to accept any rate of unemployment for the sake of price stability. It is, rather, that the parties' *relative* priorities differ, and this point seems secure. Even though Carter may have opted for typically Republican policies, it is hard to believe Reagan will be less zealous than Carter in the battle against inflation, or more troubled by unemployment.

[2] These data were made available by the Inter-University Consortium for Political and Social Research. Neither the original collectors of the data nor the Consortium bear any responsibility for the analysis or interpretations presented here.

[3] In the years in which panel data were available (1958, 1960, 1974, and 1976) dummy variables registering the respondents' vote in the previous election were used. In other years partisanship was specified by a pair of nominal party identification dummies. Results were virtually identical, however, when the nominal party identification dummies were substituted for the previous vote terms in the four panel years.

There are obvious shortcomings in this analysis. Only the unemployment side of the hypothesis has been addressed, and only at the personal level. These deficiencies will be remedied by (1) analyzing direct indicators of personal difficulties with inflation as well as unemployment, and by (2) investigating the effects of perceptions of unemployment or inflation as serious national problems.

Table 1. The Effect of Head of Household's Unemployment Experience and Trend in Family Finances on Voting in Congressional and Presidential Elections, 1956-1978

	Presidential Elections		Congressional Elections	
Year	Head's Unemployment Experience[a]	Personal Finances	Head's Unemployment Experience[a]	Personal Finances
1956	–.097 (.513)	–.134** (.031)	–.659 (.597)	–.087** (.034)
1958	–	–	–.413* (.243)	–.103** (.036)
1960	–.659** (.241)	–.046 (.030)	–.335 (.274)	–.019 (.033)
1962	–	–	–.244 (.230)	.037 (.042)
1964	–.579 (.355)	.040 (.033)	–.148 (.314)	–.014 (.035)
1966	–	–	–.630 (.560)	–.077* (.036)
1968[b]	.130 (.305)	.134** (.038)	–.411 (.308)	.037 (.048)
1970	–	–	–.211 (.308)	–.008 (.048)
1972	–.523** (.150)	–.078* (.037)	–.395* (.179)	–.022 (.041)
1974	–	–	–.041 (.155)	.032 (.030)
1976	–.210 (.131)	–.081** (.029)	–.039 (.157)	–.131** (.034)
1978	–	–	–.093 (.132)	–.007 (.026)

Source: American Panel Study, 1956, 1958, 1960; American National Election Studies, 1962-1970; American National Election Series, 1972, 1974, 1976; American National Election Study, 1978; Center for Political Studies, University of Michigan.

[a] In all elections between 1958 and 1964, as well as in 1968 and 1970, this variable indicated whether or not the respondent's head of household had been unemployed in the previous six months. From 1972 on the time frame was the previous year, and in 1956 and 1966 it could only be ascertained whether or not the head was currently out of work.

[b] Wallace voters were excluded from the presidential election analysis in 1968.

*$p < .05$
**$p < .01$

Personal Economic Problems. What is needed in order to conduct a more compelling test of the policy-oriented hypothesis at the personal level is a means of isolating those voters who find inflation or unemployment salient and troublesome. In three of the CPS election studies—1972, 1974, and 1976—this could be done on the basis of answers to the following questions:

> Let's change the subject for a moment. We like to have people tell us what sorts of problems they have to deal with in their daily lives. Can you tell me what some of the problems are that you face these days in your life. . . . Anything else?

Although these questions did not specifically refer to economic problems, well over half the problems mentioned were economic in nature.

Unfortunately, the coding categories developed by the CPS for these items were, for present purposes, not appropriate. It was therefore necessary to recode responses as they appeared on the original protocols.[4] References to unemployment and inflation were naturally sorted out, but other categories were derived from the verbatim interview data as well. The coding scheme which was developed, along with the marginal frequencies from all three years, is presented in Table 2. What are reported are the respondents' most important personal *economic* problems. If a respondent mentioned some non-economic problem as most important, e.g., poor health, but also mentioned an economic problem, the economic problem was the one which was coded here.

As Table 2 shows, respondents cited *inflation* more frequently than any other economic problem. This category includes all references to high or rising prices, either in general or for specific commodities. Many economists, of course, express puzzlement that anyone should be concerned about inflation in and of itself. What should worry people is the failure of pay increases to match price increases—a declining real income—and not the nominal price level. But as Table 2 indicates, between 5 and 7 percent of the respondents did refer explicitly to a *declining real income*. Responses subsumed under this category included failure of wages to keep up with price increases, living on a fixed income, declining purchasing power, failing to maintain the standard of living one is accustomed to, or having great difficulty in doing so.

The *unemployment* category is broadly defined. It contains all respondents who felt their worst economic problem was that they (or people close to them) were laid off, unemployed, worried by the threat of unemployment, or underemployed. Despite the breadth of this definition, only between 3.4 percent and 4.4 percent fell into this category. Perhaps, as some economists and politicians claim, unemployment compensation and other benefits have substantially eased the burden of joblessness for many people. Or it might be argued that many respondents felt that references to unemployment would reflect badly on themselves, and thus chose to name some other problem. This seems very unlikely, however, given that they showed no reluctance to admit that they (or family members) were out of work in response to other questions.

Table 2. Respondents' Most Important Personal Economic Problems (Percent)

	1972	1974	1976
Inflation	14.4	30.2	22.2
Declining Real Income	4.8	7.1	5.9
Unemployment	3.4	4.4	4.0
Taxes	5.5	1.3	4.1
General or Miscellaneous Economic Problems	18.6	19.2	19.8
Non-Economic Problems	32.5	24.4	26.8
No Problem Mentioned	20.8	13.4	17.2
N[a]	1109	2523	2415

Source: American National Election Series, 1972, 1974, 1976. Center for Political Studies, University of Michigan.

[a] Based on the 1972, 1974, and 1976 cross-sectional weighted samples of all respondents. No important turnout effects were present, however, so the percentages reported here are virtually identical when voters only are considered. Weights are *not* used in any of the probit analysis.

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[4] One of the major problems with the 1976 code was that complaints about unemployment were lumped together with all other job-related references; only about half of the references in the employment-related category concerned unemployment. Many of the 1972-74 codes contained a wide variety of responses. One category, for example, contained references to inflation, taxes, governmental waste, and the cost of college. These responses obviously need to be disaggregated.

Similarly, few respondents referred to *taxes* as their worst personal economic problem. A residual category, *general or miscellaneous economic problems,* was largely composed of vague references to such things as "bills," "finances," or "money problems." As Table 2 shows, the percentage of respondents falling into this category was remarkably stable—around 20 percent each year. In fact, the size of most categories remained quite stable from year to year—only the percentage citing inflation fluctuated much. Changes in the size of that category, though, clearly reflected the actual rates of inflation which obtained in 1972, 1974, and 1976.

At this point, it should be noted that personal problems due to inflation and unemployment differ in two fundamental ways. First, the serious objective costs of unemployment are reflected in the fact that respondents who cited it were far more likely than average to report that their financial situation had worsened. Respondents in the inflation category, on the other hand,, were not more likely than average to report a worsened financial situation. This is quite in keeping with economic studies reviewed by Hibbs (1979) which indicate that, at least in the short run, the objective income, wealth, and distributional effects of inflation are small. Hibbs is almost certainly correct in concluding that "less tangible subjective and psychological factors are probably more important than objective costs" in explaining public aversion to inflation (p. 712).

Secondly, these data reflect the fact that unemployment is concentrated disproportionately among lower-income and working class groups, while the less tangible costs of inflation are spread pretty evenly across society. In 1976 black respondents were more than twice as likely as whites to cite unemployment as their most serious economic problem. Among occupational categories, the percentages ranged from only 1.6 percent of managerial-administrative personnel to 14.0 percent of non-farm laborers. The propensity of respondents to cite inflation as their most serious economic problem, however, was unrelated to a host of background socioeconomic characteristics, including income, age, sex, race, and union membership. In short, there are major differences between inflation and unemployment in the nature of the costs they entail and how these costs are distributed.[5]

Perceived National Economic Problems. Any comparison between the personal-level and national-level versions of the policy-oriented hypothesis should be a fair one. A desideratum for the following analysis, therefore, is to develop measures of concern with national economic problems which are as similar as possible to the measures of personal economic problems. Fortunately, almost all CPS Election Studies since 1958 have contained questions suitable for present purposes. Like the personal problems questions, they were all open-ended, imposed a minimum of constraint on responses, and did not refer to any parties or candidates.

The various questions used were as follows:

- What do you think are the most important problems facing this *country*? (1972, 1974, 1976, 1978).
- What do you personally feel are the most important problems which the government in Washington should try to take care of? (1966, 1968, 1970).
- What would you personally feel are the most important problems the government should try to take care of when the new President and Congress take office in January? (1960, 1964).
- Now, how about problems here at home inside the United States in the past year or so. . . . Would you say that things in general have been going along better than they were a year ago, not as well as before, or have they stayed about the same? How is that? (1958).

The coding scheme derived from the available CPS categories and marginal frequencies for each year are presented in Table 3. As before, it is the most important *economic* problems respondents cited which are reported; if a non-economic problem (e.g., Vietnam) was deemed most important, but an economic problem was also mentioned, the economic problem was the one which was coded.[6]

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[5] Parenthetically, these data indicate that it was useful to differentiate respondents who cited a declining real income from those who simply cited inflation. The former were considerably more likely to report being worse off financially, and such references came considerably more frequently from low-income groups and retired persons.

[6] In all studies except 1958, respondents were subsequently asked which of the problems they had mentioned they considered most important. Problem reports could thus be ordered according to perceived importance, and the highest ranking economic problem was included in Table 3. The procedure followed in the 1958 study was more complicated. After indicating whether they believed "problems here at home in the United States" had got better in the past year, got worse, or stayed the same, respondents were asked to report the particular problems they had in mind. Only the reported problems of those who believed things had stayed the same or had worsened were coded, but about 85 percent of the sample fell into these two categories. As Table 3 indicates, inflation and unemployment were the only two economic problems which could be extracted from the CPS codes for that year. The "no problem men-

Most categories require little explanation. The *inflation* category includes all those who named high or rising prices as the nation's most serious economic problem. Included under *unemployment* are respondents who felt the government should create more jobs, provide job retraining, or grant aid to depressed areas; most, though, simply cited unemployment. As Table 3 clearly shows, the number of respondents who named either of these two economic problems as most important closely tracked the actual inflation and unemployment rates in these years. The unemployment figures peaked in 1958 and 1976, inflation in 1974 and 1978. It should be noted that in the three years in which comparisons are possible (1972, 1974, 1976), the number of respondents who cited inflation or unemployment at the national level was always larger than the number who cited them as personally troublesome. The potential impact of voters' perceptions of national-level issues upon elections is thus greater, especially with regard to unemployment. Only a small percentage ever cited unemployment as a personal problem, but in 1958 and 1976 it was cited more frequently as a problem affecting the nation as a whole than any other problem.

As at the personal level, few respondents felt *taxes* were the most important national economic problem. Under *more government programs* were respondents who believed that the federal government should do more to alleviate social ills. Most of them advocated new or enlarged federal programs in particular areas, e.g., housing, transportation, health care, poverty programs, or social security. The category also included those who felt the federal government should increase its spending to stimulate the economy, or exert more control over private business. Individuals in the next category, however, believed that a large and growing public sector was precisely what was wrong with this country. A variety of responses fell under the rubric of *less government spending* —the federal government's budget deficits, waste and inefficiency, creeping socialism, or too much government interference into private enterprise. As Table 3 shows, the percentage of respondents

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tioned" category is unusually large in 1958 because responses relating to defense and foreign affairs were inappropriate, and because it includes the 15 percent who felt the U.S. domestic situation had been improving.

Table 3. Respondents' Perceptions of the Nation's Most Important Economic Problems, 1958-1978 (Percent)

	1958	1960	1964	1966	1968
Inflation	6.4	3.7	1.2	15.9	2.6
Unemployment	33.9	11.6	6.9	2.6	2.3
Taxes	–	4.7	4.3	6.4	3.7
More Government Programs	–	20.3	27.3	19.8	16.1
Less Government Spending	–	3.5	3.6	7.2	2.5
General Economic Problems	–	2.9	0.6	1.9	0.8
Non-Economic Problem	18.2	42.1	37.9	38.5	69.4
No Problem Mentioned	41.5[a]	11.2	18.2	7.7	2.6
N	1514	1514	1571	1291	1557

	1970	1972	1974	1976	1978
Inflation	14.2	20.0	50.2	30.3	52.2
Unemployment	7.9	6.3	6.6	32.1	4.6
Taxes	6.1	5.5	0.3	1.6	2.5
More Government Programs	26.1	10.9	1.7	4.2	3.3
Less Government Spending	1.9	3.3	1.0	2.3	2.6
General Economic Problems	4.9	8.0	26.5	14.1	6.3
Non-Economic Problem	34.6	42.7	10.8	9.3	23.8
No Problem Mentioned	4.3	3.3	2.9	6.1	4.7
N	1580	1109	1624	1217	2304

Source: American Panel Study, 1956, 1958, 1960; American National Election Studies, 1964-1970; American National Election Series, 1972, 1974, 1976; American National Election Study, 1978; Center for Political Studies, University of Michigan.

[a]The "no problem mentioned" category in 1958 is not comparable to the same category in the other years. See footnote 6 for details. Column totals may not add exactly to 100 percent because of rounding error.

in the more government programs and less government spending categories dropped off considerably after 1970. But given the traditional differences between the two parties on the proper size and scope of the public sector (Tufte, 1978), it is probably important for the following analysis that these categorical variables be specified.

Finally, most respondents included in the next category, *general economic problems,* referred simply to "the economy," although a few cited such problems as interest rates or a bearish stock market. This category was quite small during the prosperous sixties, but by 1974 over a quarter of the respondents were included in it. As was also the case at the personal level, no obvious policy-oriented hypotheses can be made about respondents who cited general or miscellaneous economic problems. It seems reasonable to posit, though, that such voters might tend to vote against the incumbent president and his party's candidates.

Non-economic problems, of course, include everything else: the Vietnam War, national defense, crime, and a host of other things. Not surprisingly, this catch-all category was largest in the tumultuous, war-torn, but prosperous year of 1968, smallest in the peaceful but economically troubled period beginning in 1974.

The Nature of National Economic Problem Perceptions. An intriguing finding in Kinder's and Kiewiet's study (1979) was that individuals' assessments of their personal financial situation were only weakly related to their perceptions of national business conditions. However, probit analyses of references to unemployment and inflation at the national level indicated that individuals' personal economic problems clearly influenced these perceptions. (For the sake of brevity only a summary of these analyses can be reported—a report on estimation procedures and results is available from the author upon request.) Respondents who named unemployment as a problem at the personal level were much more likely to cite it as a problem at the national level. The same was true for inflation.

When group differences and party identification were considered, though, the same pattern present at the personal level reappeared. Blacks, union members, and low-income respondents were more likely than average to name unemployment the nation's most serious economic problem. Democrats were also more likely, but differences between partisan groups were generally smaller than they were between social, economic, and racial groups. In contrast, the percentage of respondents naming inflation as the most serious national economic problem was nearly constant across all major socioeconomic and partisan groups.

To be sure, the strength of the personal-national associations should not be overstated. Despite the greater propensity of respondents who referred to inflation and unemployment as personal problems also to cite them at the national level, the large majority of those who named them at the national level had not referred to them as personal problems. Furthermore, the impact of party identification upon people's perceptions of national economic problems was quite small in comparison to the overall variation in perceptions of inflation and unemployment from year to year. In short, perceptions of national economic problems, though affected to some degree by the respondents' personal economic experiences, socioeconomic background, and party affiliations, closely reflect actual conditions in the nation's economy.

A couple of other matters must be considered before the electoral effects of concern over inflation and unemployment can be estimated. First, although perceptions of national problems are evidently not very susceptible to partisan coloration, such perceptions might well produce changes in partisanship. If so, a cross-sectional analysis which specified partisanship might underestimate the effects of perceived national problems upon voting. Fortunately, this threat proved not to be serious. Cross-tabular analyses revealed that change in respondents' party identification from 1974 to 1976 was only weakly related to perceptions of national economic problems. Furthermore, consistent with Brody's findings (1977), what little systematic change there was occurred in the strength components—respondents perceiving inflation to be the nation's worse problem tended to become more strongly identified with the Republicans, while unemployment tended to strengthen the identification of those who had already been Democrats in 1974. In specifying partisan predispositions, therefore, we will use these procedures in the following analyses: when panel data are available, the respondent's vote in the previous election will be used ("previous election" means the previous congressional election when congressional elections are analyzed, but the previous presidential election when presidential elections were analyzed—in 1976, for example, the previous election considered in the congressional equation was 1974, but for the 1976 presidential race the respondents' reported vote for president in 1972 was specified). When only cross-sectional data are available, the partisanship indicators will incorporate the directional component only.

Finally, both the personal and national level indicators of concern over inflation and unemployment reflect only the presence of these problems, and not their absence. Now it may well be that the parties benefit electorally from dealing successfully with these problems. Success of this nature, however, would be indicated by the percentage of respondents citing inflation or unemployment approaching zero, and any electoral effects could not be detected by an analysis based upon these measures. Estimation will thus be confined to those years in which inflation or unemployment was bad enough to prompt a significant number of respondents (over 10 percent) to cite one of these problems at either the personal or national level. These years are 1958, 1960, and 1970 through 1978.

Estimation. The following analysis is a simple one. The probit model to be estimated is of the form:

$$V^{P,C} = f(\beta_0 + \beta_1 R + \beta_2 D + \beta_j PE_j + \beta_k NE_k + u)$$

where $V^{P,C}$ = respondent's reported vote for president or congressman, taking on the value of 0 if Democratic, 1 if Republican.

β_0 = a constant term.

R = a dummy variable which, when only cross-sectional data are available (1966, 1970, 1972, 1978), takes on the value of 1 if the respondent identifies with Republican party (as either a strong or weak identifier, or as an Independent leaning Republican), 0 otherwise. When panel data are available (1958, 1960, 1974, and 1976), the respondent's vote in the previous election is used. This variable then takes on the value of 1 if the vote was Republican, 0 otherwise.

D = the same as above, except it registers Democratic identifiers or respondents who voted Democratic in the previous elections. A reference group is thus composed of respondents who, in years in which cross-sectional data are used, are Independents, or, when panel data are used, had not voted in the previous election.

PE_j = in 1972, 1974, and 1976 a battery of dummy variables, one for each of the categories of personal economic problems which were coded. The reference groups are composed of those respondents who either reported no economic problems or no problems whatsoever. In other years the same personal finances and head of household's unemployment terms used previously are included.

NE_k = a battery of dummy variables, one for each of the categories of perceived national economic problems which were coded. As above, the reference groups are composed of respondents who did not cite an economic problem.

u = a randomly distributed error term.

Results are presented in Tables 4 and 5. As before, the top numbers in each entry are probit estimates, the numbers in parentheses below them are standard errors. Democratic votes were coded 0 and Republican votes 1, so positive signs are pro-Republican.

At the personal level, results were much the same as in the previous analysis. Coefficients of the personal finances and head of household unemployment terms were virtually unchanged; thus the head of household unemployment variable continued to have a consistent but usually small pro-Democratic impact upon voting. Substituting the personal economic problem items for these measures provides some additional support for the policy-oriented hypothesis. As before, the most impressive figures are for the unemployment variable—signs of all five estimates are pro-Democratic in direction, and most estimates are fairly large. True, none is statistically significant, but the small size of the categories would make conventional significance levels difficult to achieve.

The inflation side of the hypothesis, however, fares poorly at the personal level. True, every estimate of the inflation term is more positive and thus more pro-Republican than the corresponding unemployment term. But this is largely due to the consistent, pro-Democratic direction of the unemployment terms; most estimates for the inflation terms did not differ noticeably from those of the neutral, nondescript reference groups. The one bright spot is the 1972 presidential election, where the sign was in the correct direction and statistically significant. Apparently the only Democratic candidate punished by voters who felt personally injured by inflation was the unfortunate George McGovern. There is little to report concerning the other personal economic problems included in the analysis. In general, estimates for the effect of a declining real income fell in between the inflation and unemployment terms, with respondents voting more Democratic than the former but more Republican than the latter. Estimates of the taxes term were erratic in direction, while those for the general economic problems category hovered around zero.

Turning to perceptions of *national* economic problems, the unemployment side of the policy-oriented hypothesis again receives strong empirical backing. Voters who named unemployment the most serious national economic problem gave considerably more support to Democratic presidential candidates in 1960 and 1976 and to that party's congressional candidates in 1958. To be sure, the sizes of the probit estimates in these three elections were not overwhelming, and were generally smaller than those for the personal unemployment terms. This means that for any one individual, personally experienced unemployment

Table 4. The Effect of Personal and Perceived National Economic Problems on Voting in Presidential Elections, 1960-1976

	Presidential Elections		
	1960	1972	1976
Intercept	.207	.567	–.131
Democrat[a]	–1.08** (.112)	–.826** (.179)	–.715** (.149)
Republican[a]	.577** (.092)	.840** (.198)	.731** (.128)
Personal Economic Problems			
Family Finances	–.049* (.030)	–	–
Head of Household's Unemployment	–.638** (.244)	–	–
Inflation	–	.273* (.166)	–.037 (.116)
Declining Real Income	–	–.087 (.233)	–.152 (.208)
Unemployment	–	–.346 (.296)	–.338 (.258)
Taxes	–	.070 (.234)	–.047 (.233)
General Economic Problems	–	.067 (.145)	–.088 (.123)
National Economic Problems			
Inflation	.395* (.231)	.040 (.143)	.104 (.148)
Unemployment	–.257* (.138)	.022 (.220)	–.361** (.148)
Taxes	.010 (.204)	.360 (.267)	***
More Government Programs	–.065 (.103)	–.072 (.176)	.096 (.265)
Less Government Spending	–.160 (.206)	.198 (.308)	.269 (.329)
General Economic Problems	.010 (.222)	–.417* (.198)	.029 (.171)
N	1149	760	923

Source: American Panel Study, 1956, 1958, 1960; American National Election Studies, 1966, 1970; American National Election Series, 1972, 1974, 1976; American National Election Study, 1978; Center for Political Studies, University of Michigan.

[a] These terms are nominal party identification in 1966, 1970, 1972, and 1978. In other years they indicate the respondents' vote in the previous election.

*$p < .05$

**$p < .01$

***Insufficient response

probably exerts a slightly larger influence upon voting than does the perception of unemployment as a serious national problem. But as I have pointed out earlier, the number of voters who perceived unemployment to be nationally troublesome in these years was a large multiple of the number for whom it was personally troublesome. The national-level measure included over eight times as many voters as the personal level measure in 1976, over three times as many in 1960, and

Table 5. The Effect of Personal and Perceived National Economic Problems on Voting in Congressional Elections, 1958-1978

	Congressional Elections							
	1958	1960	1966	1970	1972	1974	1976	1978
Intercept	.052	–.149	.053	–.077	1.02	–.498	–.327	–.159
Democrat[a]	–.964** (.133)	–.759** (.105)	–.892** (.182)	–.831** (.181)	–1.00** (.188)	–.573** (.129)	–.293** (.125)	–.604** (.140)
Republican[a]	.998** (.119)	1.27** (.120)	.986** (.188)	1.09** (.188)	.824** (.189)	1.12** (.129)	1.17** (.139)	.682** (.144)
Personal Economic Problems								
Family Finances	–.105** (.036)	–.021 (.033)	–.077** (.037)	–.008 (.049)	–	–	–	–.008 (.026)
Head of Household's Unemployment	–.387 (.245)	–.344 (.275)	–.720 (.555)	–.193 (.222)	–	–	–	–.094 (.133)
Inflation	–	–	–	–	–.024 (.175)	–.037 (.116)	–.244* (.136)	–
Declining Real Income	–	–	–	–	–.187 (.296)	–.234 (.176)	–.137 (.229	–
Unemployment	–	–	–	–	–.537 (.412)	–.214 (.287)	–.409 (.304)	–
Taxes	–	–	–	–	–.389 (.250)	***	.235 (.256)	–
General Economic Problems	–	–	–	–	–.094 (.159)	–.104 (.142)	–.010 (.147)	–
National Economic Problems								
Inflation	–.035 (.211)	.176 (.269)	.325* (.165)	.216 (.171)	–.277 (.163)	.037 (.167)	.103 (.175)	.011 (.108)
Unemployment	–.289** (.110)	.015 (.151)	.694* (.317)	–.162 (.222)	.074 (.246)	.044 (.237)	–.218 (.169)	–.106 (.251)
Taxes	–	.101 (.241)	.053 (.242)	–.090 (.272)	–.123 (.263)	***	***	.179 (.260)
More Government Programs	–	.194* (.117)	–.032 (.151)	.031 (.144)	–.135 (.180)	***	.943** (.307)	.279 (.276)
Less Government Spending	–	–.057 (.227)	–.160 (.198)	.425 (.377)	–.141 (.321)	***	–.115 (.354)	.110 (.233)
General Economic	–	.173 (.236)	***	–.061 (.227)	–.217 (.217)	.162 (.178)	.146 (.196)	.047 (.167)
N	825	987	677	683	692	863	689	1009

Source: American Panel Study, 1956, 1958, 1960; American National Election Studies, 1966, 1970; American National Election Series, 1972, 1974, 1976; American National Election Study, 1978; Center for Political Studies, University of Michigan.

[a] These terms are nominal party identification in 1966, 1970, 1972, and 1978. In other years they indicate the respondents' vote in the previous election.

*$p < .05$

**$p < .01$

***Insufficient response

over six times as many in 1958. Taking this into account, it is clear that in elections which follow severe recessions (1958 and 1976) or periods of slow growth (1960), the electoral impact of unemployment derives mainly from voters who see it as a pressing national problem, and not from those who have been personally affected.

As to the personal level, though, the inflation side of the hypothesis does not fare as well. The estimate in the 1960 presidential election was in the correct direction and statistically significant. And support for the hypothesis was quite strong in 1966, when the large percentage (16 percent) of voters who cited inflation were considerably more likely to vote Republican. It seems hard to believe that the 2.8 percent rise in consumer prices that year triggered such a reaction, but this was a substantial increase over the average 1.3 percent of the previous five years. In the elections from 1970 on—all years in which large percentages of voters named inflation the nation's most serious problem—the greater support they were predicted to give to the Republicans was not forthcoming. These figures suggest that through the 1960s the Republicans may have held an edge in being perceived as better able to handle inflation. This, of course, was a period when inflation was so mild few people were much concerned about it. But in keeping with Okun's reasoning (1973), any favorable image voters may have had of the Republicans in this regard eroded in the face of the large acceleration in the inflation rate during the Nixon and Ford presidencies.

Similarly—at least in the elections analyzed in Tables 4 and 5—respondents who believed more government programs were needed to alleviate social ills did not appear to differ in their voting decisions from those who believed too much government spending or taxation was to blame for the nation's difficulties.[7] It should be noted, though, that the former category was very small from 1972 on, and that the latter categories never contained more than a small percentage of respondents. The final categories were composed of those respondents who mentioned some general or miscellaneous economic problem. For the most part, such concerns had no effect upon voting. The only glimmer of an impact occurred in 1972, when voters in this category gave greater support to the Democratic presidential nominee.

One last point deserves attention, and that is that the policy-oriented hypotheses receive more support in the earlier years of the congressional elections series than in the later years. In the first three or four equations the hypotheses do not fare badly. As mentioned earlier, the Republicans paid dearly for the 1958 recession, as did the Democrats for inflation in 1966. And although not statistically significant, the signs of the national inflation and unemployment terms were in the predicted direction as late as 1970. To be sure, this first set of elections contain some anomalous results as well, e.g., the effect of the commitment to more government programs in 1960 and in the 1966 concern over national unemployment. On the whole, though, results of the first half of congressional elections equations compose a somewhat attenuated and more erratic version of the pattern of findings obtained in the analysis of presidential elections.

This was not the case in the second half of the congressional series. In the five election equations beginning with 1970, only one national problem—more government spending in 1976—is statistically significant, and its sign is pro-Republican in direction. As the virtually random patterns of the signs of the various terms indicate, after 1970 the particular economic problems cited by respondents did not appear to affect their congressional voting decisions in any consistent fashion. Admittedly, the break between the first set of elections and the second set has probably been exaggerated. As one looks across the entire series in Tables 4 and 5, however, it seems clear that voters' perceptions of national economic problems have come to have virtually no impact upon their voting decision in congressional elections.

These findings are consistent with the evidence showing a "denationalization" of congressional elections over the past few decades (Burnham, 1974). Most researchers in this area attribute this denationalization to a substantial increase in the electoral advantage enjoyed by incumbents (Mayhew, 1974). Although this advantage is usually seen as producing comfortable winning margins, the greater importance of incumbency would also appear to make congressional elections less sensitive to cross-district national forces—or more precisely, to voters' perceptions of particular national economic problems.

Summary and Conclusions

The main theme running through the analyses undertaken by this study is that they repeatedly found important differences between the issues of inflation and unemployment. There were large differences between racial and occupational groups in respondents' propensity to name unemployment their most pressing personal problem. There were no such differences regarding inflation. These same group differences were also present, to a lesser degree, in the propensity to name unemployment the nation's most serious problem. Again, there were no such differences on inflation. And there are differences between inflation and unemployment in the very nature of the costs they impose on people. The survey data in this study were fully consistent with the findings of the economics literature: unemployment entails serious objective economic costs; but the major costs of inflation, at least in the short run, are the more intangible ones associated with living in a more uncertain and anxiety-provoking world.

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[7] Findings concerning the effects of these public sector issues were markedly different in the 1964 and 1968 elections, however. In these years there were large percentages of respondents in these categories, while the percentages in the inflation and unemployment categories were very small. Furthermore, estimates for the more government programs terms were strongly pro-Democratic in direction, while voters in the too much government spending and taxes categories in 1964 and 1968 voted strongly Republican.

Finally, inflation and unemployment had different effects upon voting. The inflation side of the policy-oriented hypothesis garnered only weak, scattered support. There were a few instances at both the personal and national level of inflation-sensitive voters supporting the Republicans, but that is all. In contrast, there was a mass of evidence showing voters reacting to their concern over unemployment by voting more Democratic. Voters who had personally experienced unemployment gave a modest but consistent boost to Democratic candidates in virtually every election. This effect was heavily supplemented in years of high unemployment by the large percentage of voters who felt unemployment was a serious national problem and consequently voted heavily Democratic as well. Thus in terms of the nature of its costs, its distribution, and its ramifications for party support and voting behavior, unemployment coincides neatly with the lines of cleavage drawn at the time of the New Deal. Inflation does not.

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COMMUNICATIONS

ON ARTICLES

Comment on Bensel
(Vol. 74, September 1980, pp. 734-44)

TO THE EDITOR:

The article by Richard F. Bensel, "Creating the Statutory State: The Implications of a Rule of Law Standard in American Politics," was interesting to me because I have always been fascinated by both Frederick Hayek and Theodore Lowi. But I question two points in the article.

The first point is a nit I would like to pick about federalism. Bensel makes a good case for the incompatibility of the rule of law with cooperative federalism as practiced in the U.S. today (p. 737). However, his assertion that the rule of law "favors a unitary national state over a mixed federal-state alternative" (p. 737) does not make an important distinction. Dual or layer-cake federalism in which certain reserved powers are delegated exclusively to state governments and other responsibilities are handled by the national government acting alone would be, in theory, completely in harmony with the rule of law. In this kind of federalism there would be no mixing of federal and state standards, a mixing which is incompatible with the rule of law.

Whenever there are more government levels involved, the possibility of ambiguity as to which has control over what activity is increased. Therefore, I would argue that unitary system is marginally more compatible with the rule of law than dual federalism, but marble-cake or picket-fence, cooperative federalism is clearly incompatible with this standard.

The second point concerns the idea of a responsible two-party system. The last paragraph of the article asserts that "supporters of the rule of law position seem to be at odds with those who advocate a strong, national two-party system" (p. 743). This contention does not follow from the empirical data presented in the article. These data show that party leaders, chairs of congressional committees, and presidents recently have not favored strengthening the rule of law standard as defined by Bensel. This suggests that the existing leadership in our weak, non-responsible party system does not support a return to this standard in America; but perhaps they would if held responsible to any standard—e.g., party responsibility—other than the whims of vocal constituent groups and the perquisites gained from close and enduring alliances with powerful forces in the career bureaucracy. There are no incentives in our current system to encourage adherence to the rule of law. Quite the reverse: party leaders, especially presidents, are encouraged to keep options open for future negotiation among interests. In a responsible party system, the major goal of party leaders is to carry out the party platform, thus assuring their own renomination by the party organization. Leaders will find the rule of law standard a handy, if not necessary, tool with which to accomplish this goal.

The article really does not address the question of conflict between rule of law and responsible parties. I believe that a direct comparison of the important features of a responsible party system and the rule of law will show that these two are compatible and even reinforce one another.

The rule of law statutory criteria listed by Bensel are:

1. The general rule contained in a statute must be prospective, not retrospective.
2. Statutory language must be clear, specific and publicly advertised. The statute must minimize administrative discretion.
3. The general rule contained in a statute must treat individuals equally in classification and enforcement. The objects of state policy should be classes and categories of citizens or behavior rather than specific individuals or isolated situations.
4. Statutes must be drafted by a government body (usually the legislature) not involved in direct enforcement or implementation.
5. All administrative decisions must be subject to judicial review for compatibility with statutory specifications (pp. 736-37).

The criteria for a responsible party system are as follows:

1. Parties must be committed to a range of government policies.
2. Party platforms must clearly state these policy preferences in advance of elections.
3. The party with the most votes must get complete control of the government.
4. The elected public officials of the party must carry out the party platform.

The requirement to write out laws to specify what government policy should be, rather than mushing one's way through with administrative compromises, would seem to be a necessary means of implementing point 4 of the responsible party criteria. Even point 5 of the rule of law statutory criteria helps responsible parties. If the point of judicial review is to assure that the bureaucracy is carrying out the law as written by the legislature, then rule of law point 5 has the effect of using the courts to make the bureaucrats carry out the party platform. Thus a rule of law standard brings judges and civil servants into line with the party. How else could this be done without compromising merit selection of public employees and an independent judiciary?

It also seems clear that a responsible party model *is* incompatible with the interest group liberalism decried by Lowi. How can a party define its policies clearly in a platform and then carry them out in the context of the legislative and bureaucratic negotiation among interest groups which characterizes our system in the absence of the rule of law? Interest group liberalism flourishes within a loose, weak, non-responsible, faction-ridden party system. It cannot survive alongside strong parties. Or perhaps it should be stated the other way around: parties cannot be strengthened or at least made responsible without the rule of law standard.

THEODORE ARRINGTON
University of North Carolina, Charlotte

Reply

TO THE EDITOR:

Theodore Arrington's communication raises two questions: the relationship of the rule of law standard to federalism and the implications of the empirical findings I reported for responsible party government. Concerning federalism, we are in agreement that the rule of law standard prefers a unitary, nationalized policy to a cooperative federal-state alternative, due to the discretion allowed local administrators under the latter arrangement. Arrington questions the relationship of the standard to "dual sovereignty": a federal system in which the state and national governments exercise absolute control over mutually exclusive legislative domains. I don't think we disagree on this point, but I would resolve the question in a slightly different way.

In the article, my goal was to develop an empirical standard which would measure the degree to which competing legislative alternatives conformed to a rule of law. For this reason, I considered only those policy alternatives which commonly arise as choices on the floor of the House of Representatives. Congress can choose between nationalized and mixed, federal-state alternatives by substituting one administrative structure for another. In the normal consideration of legislation, a "dual-sovereignty" alternative cannot arise because it is a systemic principle embodied in a constitution and not an attribute of an individual policy; only a constitutional doctrine enforced and arbitrated by a supreme judicial body can create and sustain a "dual-sovereignty" system. Congress cannot create mutually exclusive domains of legislative sovereignty through the passage of individual policies alone and, even though Congress can refuse to legislate in certain areas, inaction provides no guarantee to the states that Congress will not intrude upon state powers in the future; congressional inaction does not automatically imply that the states will enact a local alternative to the foregone national policy; nor can Congress confer powers upon the states that the Constitution doesn't give them (e.g., over interstate commerce). For these reasons, "dual-sovereignty" arrangements do not normally arise in the national legislative process and I did not consider them in the theoretical discussion of the scope of the standard.

There is only one legislative situation in which a choice between a "dual-sovereignty" alternative and a "nationalized" proposal could be presented to Congress. That one situation would arise if Congress were to consider a constitutional amendment to reestablish dual sovereignty as a constitutional principle. The rule of law position in that situation would favor the status quo and oppose the dual-sovereignty amendment because a dual-sovereignty principle would create an inherently ambiguous barrier between national and state authority. As the first century and a half of American judicial history demonstrates, such a barrier greatly expands the discretionary power of the federal judiciary and increases the instability of constitutional interpretation. In contrast, the prevailing interpretation of the Constitution clearly provides for the supremacy of national law. Thus we agree that the rule of law standard would prefer a unitary state to dual sovereignty, though Congress will probably never encounter the choice.

With reference to the second point concerning responsible party government, both the leadership of the committee system and the members of the president's party have been independently hostile to the rule of law standard. We both agree that the behavior of the modern committee system is consistent with "pluralist" ideology. We may disagree on the necessary connection between responsible parties and support for the standard. In the article, I presented evidence that relative support for the standard by the two parties depended on control of the White House (Table 1, p. 740) and that support for the standard, while related to support for the president's positions, varied between members of the two parties even when the propensity to support the president was taken into account (Figure 1, p. 741). In the absence of a modern example of a responsible party, I believe this strong relationship between control of the executive branch and opposition to statutory articulation provides supporting evidence for my conclusion. I agree that there is no apparent *logical* inconsistency between the standard and responsible parties as they have been ideally described. My response is that whenever the party system has produced a clear electoral choice, a decisive outcome, and a subsequent implementation of the victorious party's program, the result has been an orgy of delegated power (e.g., the New Deal, the 89th Congress during the Johnson years after 1964, etc.). In practice, "complete control of the government" by something approaching the responsible party ideal has meant the subordination of Congress (and the law) to the president (and executive discretion). Even in the absence of a formal connection, this pattern seems strong enough to warrant the implication of incompatibility. This conclusion is not affected by the apparent hostility of responsible party government to a strong committee system.

However, because we have no information on the attitude of Congress toward statutory specification during other historical periods, my willingness to draw the implication of incompatibility between responsible parties and statutory articulation is only tentative. A similar analysis of congressional behavior during an era in which congressional parties were relatively strong, the committee system was weak, party professionals controlled nominations, and the Supreme Court enforced a rule of law standard as a part of a separation of powers doctrine would produce more conclusive evidence concerning the relationship of the party system to the quality of legislative law. The progressive era from 1895 to 1915 would meet these requirements and, in addition, results drawn from that period would allow us to trace the origins of the discretionary state.

RICHARD F. BENSEL

Texas A&M University

Comment on Morgan and Pelissero (Vol. 74, December 1980, pp. 999-1006)

TO THE EDITOR:

In our article, "Urban Policy: Does Political Structure Matter?" John Pelissero and I examined the oft-researched question of the effect of urban government structure on public policy. From an analysis of 11 small and medium-sized cities that had changed their government structure between 1945 and 1978, we concluded that municipal reforms have virtually no impact on city taxing and spending policies. This effort was the first to analyze the effects of local government structure on policy using a quasi-experimental time-series design. We had no illusions then nor do we now that our research would be the last word on the matter. In fact, much to our surprise and embarrassment, a mistake in our article has been discovered. However, we are quite thankful that this error does not affect the basic conclusions of the paper.

Roland E. Dufault, senior staff assistant to the city manager of Worcester, Massachusetts, correctly notes that his city had been misclassified in our Table 1. We showed Worcester as having changed from a completely reformed city (council-manager, at-large elections, nonpartisan ballot) to a totally unreformed city. Worcester, in fact, did modify its form of government in 1950, but in just the *opposite* way from that indicated: from unreformed to completely reformed. Because of an unfortunate transposition in coding from the draft form to the final table, the government structure information shown for Worcester was backwards, and was wrongly included among the group of cities changing from reformed to unreformed structures.

This error necessitates a complete reanalysis of the data to determine how such a mistake affects our findings. First, a change in cities is required. Worcester remains in the data set, but now as a city that changed its structure from unreformed to reformed. This means, of course, that a new control city must be found to take the place of Hartford, Connecticut, which had been used initially to match Worcester. We now need an unreformed city (Hartford was reformed). After carefully considering the criteria for choosing the control cities, we have selected Providence, Rhode Island as the best match for Worcester. The appropriate data for Providence are as follows (to fit the original Table 1, p. 1002):

	Economic Base	**Government Structure**
9. Providence	Mm [manufacturing]	MC-W-P

Providence is completely congruent with Worcester on school and welfare assignment.

With Providence now included as the unreformed match for Worcester, we can undertake our reanalysis. To repeat briefly, our approach requires grouping the cities into four categories—those becoming more reformed (N = 9), those changing to less reformed (N = 2), and two groups of control cities. The interrupted time-series analysis includes seven revenue and expenditure variables for an eleven-year period—the year of change and five years before and after the intervention. The various dependent variables are averaged for each group of cities. The prediction equation employs a linear measure of time, a dummy variable to represent the alteration in level (intercept) following the reform, and a measure to represent the post-intervention change in slope. If the intercept and slope changes are not negatively significant (at .01), we accept a null hypothesis that reformed cities do not tax or spend less than their counterparts.

How much do the results vary upon reanalysis? Very little. A total of 28 equations are calculated, yielding 56 slopes and intercepts to be checked. The original analysis had produced nine equations with significant slopes and intercepts, only four of which were for cities undergoing structural change. (No autocorrelation for experimental cities was detected then or now.) Of the four originally significant changes, all were positive, indicating more than expected increases in taxing or spending. But two of these equations were for cities that became less reformed, contrary to the null hypothesis. In the reanalysis the following slopes or intercepts are significant (at .01 level):

more reform cities	2
less reform cities	1
control cities	4

Now, only three experimental groups show a significant change. For the two groups of more reformed cities, both regression coefficients are positive, indicating more than expected spending increases. The one significant change for an unreformed group also reflects a positive slope, suggesting, of course, that reforms do matter. The control group for this change (in an equation analyzing general expenditures) does not reflect a statistically significant intervention slope, although the sign is positive. In effect, then, only 1 of 28 interventions (4 percent) fails to support the null hypothesis. This figure compares with 7 percent in the original analysis.

A direct comparison might also be made of slope and intercept coefficients that changed from the original analysis. This reveals that only one originally significant coefficient (slope or intercept) for experimental cities became insignificant (one control group also became insignificant). No other changes occur. Again this result overwhelmingly confirms the original findings.

Although we regret the error in our original analysis, the reanalysis it occasioned confirms even more strongly the results initially reported. Again we conclude that a change in city government structure has very little effect on municipal taxing and spending behavior. The modified Table 2, presenting the new analysis, will be furnished upon request.

DAVID R. MORGAN

University of Oklahoma

Comment on White (Vol. 74, December, 1980, pp. 1007-17)

TO THE EDITOR:

In "Reason and Authority in Habermas," Stephen White covers more important material more lucidly in less space than I would have thought possible. In many respects it is a fine essay. Yet I object to his conclusion, which I take to be that Habermas' communicative ethics is neither intentionally nor inherently authoritarian because it has little to say about how institutions should be organized. I object because White's conclusion is only half the story. Precisely those aspects of philosophic discourse (White calls them "fallibilism and tolerance") which should find institutional expression are not readily translated by Habermas into institutional form either (White, p. 1013). Habermas' institutional irrelevance is a mixed blessing.

Habermas has actually been somewhat more concerned with applying discourse to real-world situations than White suggests (on "steering discourses" see 1970, pp. 72-80; on "advocacy discourse" see 1975, pp. 112-17). I will focus upon the status of discourse in situations of political conflict, which Habermas examines under the rubric "the objectivating application of reflexive theories" (1973, pp. 37-40). (This application of discourse is part of Habermas' response to Karl-Otto Apel's claim that the emancipatory cognitive interest tends to politicize science [*Wissenschaft*] by giving its interest in emancipation a particular political content [Apel, 1974, pp. 338-42; Habermas, 1973, p. 24, n. 42]. Habermas meets this objection, but raises the problem discussed here.)

Habermas recognizes that political conflict cannot be organized on a discursive basis, for the opponent generally cannot be drawn into meaningful dialogue. Consequently, it is often the case that those who would change society must engage in strategic action. However, it is important, says Habermas, that those who would change society organize themselves discursively. They should decide among themselves under discursive conditions what the goals of their action should be, and what means should be employed. The discursive organization of social activism realizes, on a small scale, the end of the revolution, a free and open society, in its means (1973, pp. 37-40).

The discursive organization of social activism has a peculiar consequence. Those who so organize themselves, suggests Habermas, cannot help but see themselves as being in the sole possession of enlightenment, even though this view is false, as its basis in discourse is incomplete, for the opponents have not yet been discursively consulted (1973, p. 40). This is a puzzling claim. I have little difficulty, for instance, imagining myself thinking along the following lines while about to engage in social action: "I believe I am right, though I am not sure; it could be that my opponents are correct. However, it is necessary to act; I have analyzed the situation as best I can, so here goes." The question is why this line of thought, which might be called a thoroughgoing fallibilism of action, seems unavailable to Habermas.

It must not be forgotten that for almost fifteen years Habermas has sought to develop an epistemological social theory (1971, pp. vii, viii). Discourse has played a number of roles in this project, but in either the foreground or background the claim has always been present that a discursively achieved consensus is a justified one. This seems to mean that consensus under the ideal speech situation is indistinguishable in its force from what used to be called philosophical first principles, or sufficient grounds. Strategic political action, however, is cut off from discursive justification. This leaves it vulnerable to the claim that it is merely a form of "decisionism" (*Dezisionismus*). Yet it is precisely the arbitrary character of "decisionism" that Habermas has directed so much of his dazzling brilliance towards overcoming. In this circumstance what he seems to do is to suggest that if social activists organize themselves discursively they can and perhaps must act as though the rightness of their actions is warranted by the "promissory note" that one day all will be discursively consulted. The idea of a "promissory note" which may be redeemed after the revolution explains *why*

> the strategic action of those who have decided to engage in struggle, and this means to take risks, can be *interpreted hypothetically* as a retrospection which is possible only in anticipation. . . (1973, p. 40, emphasis added).

There is no alternative in Habermas' work, no space, as it were, between grounded and arbitrary claims. It is for this reason that he sometimes writes as if the "halo" of discourse could be transferred to nondiscursive situations. In fact, the following scenario—precisely the type White wishes to exclude—is readily imaginable. Social activists through clever strategic action have turned the tide and begun to win the revolution. At this point former opponents begin in droves to "recognize themselves" in its goals, and rush to participate, first in "therapeutic discourses" (reeducation), and then in genuine discourse designed to legitimate the new order. And who is to say they are acting in bad faith? Seemingly genuine conversions of this type occur frequently. However, the truth and rightness of the winner's claims is hardly vindicated by the agreement of those who—if they failed to agree—would be excluded from those discourses which would run the new order.

This admittedly heavy-handed scenario does not support the claim that discourse is inherently authoritarian. For instance, the "negative freedoms" of those who continued to dissent might be stringently protected (though it is not so clear that White's interpretation of participation as a "preferred freedom" would be terribly relevant under these circumstances) (p. 1016). But, incipient authoritarianism is perhaps not the real issue in any case. Rather, it is that the fallibilism and tolerance of philosophic discourse should be institutionally reflected in procedures which allow for error. Habermas' failure to devote much attention to this problem reflects, I suggest, the momentum (though not the intention) of a style of thought in which all decisions are ultimately either grounded or equally arbitrary. Seen in this light, White's defense of Habermas on the basis of his limited institutional relevance is ironic.

In those societies in which peaceful change is possible, perhaps Karl Popper's (today not very fashionable) suggestions regarding "piecemeal social engineering" as the institutional embodiment of fallibilism need repeating. In those societies in which peaceful change is not possible, we must at least remind social activists that they are not operating with a "promissory note" of any kind. We should do so if for no other reason than that not all those who suffer, and certainly none of those who die along the way, will be available for a post-revolutionary consensus.

C. Fred Alford

University of Maryland

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Reply

TO THE EDITOR:

I would like to thank Fred Alford for his thoughtful response to my article. I must, however, disagree with the thrust of his criticism. First of all, Alford asserts that I have argued that Habermas' thought is nonauthoritarian "*because* it has little to say about how institutions should be organized" (emphasis added). Habermas has indeed offered relatively few specific recommendations about institutions, but that fact is not the *reason* for the nonauthoritarian character of his thought. The reason is, as I argued in my article, that his communicative ethics necessarily implies a strong emephasis on fallibilism and tolerance. If this argument is correct, then it would seem that communicative ethics could support institutional arrangements in some ways not so different from the Popperian-based ones Alford recommends.

Alford, however, does not want to accept such a conclusion. He claims that Habermas' emphasis on fallibilism and tolerance cannot be translated into institutional arrangements. The difficulty in this regard reflects, according to Alford, the fact that in Habermas' thought "all decisions are ultimately either grounded or equally arbitrary." Presumably this means that Habermas cannot endorse a "thoroughgoing fallibilism" because he feels that some decisions in politics are ultimately fully justifiable on discursive grounds. To support this line of argument, Alford attempts to show how Habermas' remarks in *Theory and Practice* (1973, pp. 37-40) can easily be used to support a heavy-handed sort of politics, in which revolutionaries can justify whatever action they take as long as they issue a "promissory note" to submit themselves to the ultimate source of justification, a post-revolutionary discursive consensus. Barred from participation in such a consensus will be those opponents who have still not seen the revolutionary light and, of course, those opponents who have been killed along the way.

This scenario in fact misrepresents Habermas' remarks on political action. He suggests only that political actors should *interpret* the political struggles in which they find themselves in light of the goal of possible social arrangements in which greater discursive formation of political will is allowed. These interpretations, however, can only "open up a perspective" for strategic political action; they *"cannot claim a justificatory function"* (1973, p. 40, emphasis added). In other words, political actors cannot justify violence against others simply by issuing a "promissory note."

STEPHEN K. WHITE
Virginia Polytechnic Institute
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ON BOOK REVIEWS

Comment on Sartori's Review (Vol. 74, December 1980, pp. 1060-61)

TO THE EDITOR:

Readers of Giovanni Sartori's review of my *Democratic Political Theory* (especially those who have not read the book) may be interested to know how I would respond to his claim that my method is "both maddening and as feeble as can be."

Sartori's major criticism is that my classification of democratic theories lacks a criterion. From this flaw others follow. He is mistaken. He has overlooked a critically important and oft-repeated statement and related distinction. As is clearly stated, the classification is based upon two criteria, both of which relate to human motivation. One (see for instance the general heading for rows in chart 2, p. 169) is *prescriptive,* dealing with the motivation the theorist in question holds ought to prevail in politics. The other (the general heading for the columns in the same chart) is *descriptive,* and refers to the political motivations (alternatively called "key values") the theorist believes actually do prevail. This classification is presented as provisional and crude, but useful. It is stated that some theorists deal only with one or the other of these elements. It is also pointed out that of the sixteen categories provided by the four-by-four matrix most theorists tend to fall in one of four—the one that assumes as a dominant political motive that motive the theorist in question values most highly.

These four categories approximate "pure types," so it is not surprising that few theorists correspond exactly to any one of them; but I see no ground for suspecting that "authors can be fitted in almost anywhere." The four categories, together with suggested candidates for each, are as follows: "radical individualislm," James Mill; "public interest theory," John Dewey; "rights and duties theory." John Plamenatz; and "collectivist theory," Karl Marx. I believe it is clear that no one of these authors could be fitted into any of my categories other than the one in which I have placed him. Nor am I uncomfortable, with these large major categories, about finding Locke and Rawls in the same box. Surely they have more in common with each other than either does with James Mill, John Dewey, or Karl Marx. That the categories sometimes merge into each other is not strange: it is as it should be. Some individualists are more individualistic than others; some public interest theorists grant more to individualism than do others (both prescriptively and descriptively). I see nothing "artificial" about admitting these facts; rather, a set of categories that did *not* admit of such qualifications would be artificial, departing from the nature of human beings and failing to register the complexity of matters political. Nor is it odd to have separate categories for Dewey and Marx, while admitting the fact of certain "natural affinities" between them. Indeed, the existence of meeting points between the types of theory is testimony to their logical interconnectedness, an element that Sartori finds lacking.

As to my consideration of Rawls, I treat him as a "deontological" theorist, by which I mean one whose main stress is on "rights and duties." I do not mean that he deals *only* with what he considers *correct* rights and duties and not at all with what the average individual's behavior suggests he *believes* to be his rights and duties. That should be clear from my statement, quoted by Sartori, that Rawls' theory is "stronger on the normative than on the operational side" (p. 195), as well as from my statement that Rawls' theory is "*largely* prescriptive, while Plamenatz's is *mainly* descriptive" (p. 193, emphasis added). When Rawls moves to discussing the application of his normative theory to actual institutions, one may "properly" wonder whether the operation of those institutions that he recommends would "make a good fit" with his prescriptive principles. At the very least this would alert one to the fact that what he is saying applies only to a society in which the citizens fully subscribe to his theory of justice and behave accordingly. My discussion of Plamenatz on the immediately preceding pages should prepare the reader, if preparation is necessary, for the fact that a deontologist may make use of "operational" theories of rights and duties as well as more narrowly normative ones.

Sartori's second and less central charge is that my distinction between "justificatory" and "operational" theories is "conceptually diffuse" and no improvement over the more familiar "normative"/"empirical" dichotomy. My concern was to find a positive term to cover all non-justificatory theories, not all of which are empirical. Necessarily, it would cover a lot of ground. Appropriate sub-categories are available for whatever purpose may be at hand. What one would not gather from the review is that more specific concepts *are* used when called for—particularly when it comes to confirming or disconfirming "grand generalizations." How I do deal with the adequacy of various types of theory as judged in the light of empirical evidence may be found in any of the relevant sections of chapters 6-12. See, for instance, pages 268-77.

No sensitive author can have a work seriously misread by a scholar of Sartori's standing without wondering whether he or she has contributed to that lack of understanding. If the points I have made above do not stand out clearly enough in the book, I welcome this opportunity to underline them.

J. ROLAND PENNOCK

Swarthmore College

Reply

TO THE EDITOR:

Roland Pennock's important *Democratic Political Theory* is—as he will, I trust, agree—an *ingens sylva*, a wilderness of richness. As such, it affords many readings and also, alas, possible misreadings. But I did not misread the point that Pennock's "classification is based upon two criteria." Perhaps I take the notion of classification more seriously than Pennock; but, as a matter of logic, it behooves me to insist: a classification cannot be based on two criteria. Conversely put, two criteria cannot yield classes that are mutually exclusive, and therefore cannot yield a classification. To be sure, we often combine (in typologies, taxonomies or pure and simple mappings) two or more criteria. But in such cases the logical power of classification is lost, and the entailed risk is that obfuscation may prevail, in the end, over clarification. In any event, I do welcome Pennock's response to my review. If I have been somewhat unfair to the author, it is only fair that the author should respond. It also pleases me to take this opportunity to say that I have always held Pennock in very great esteem, and that disagreements on a particular work do not affect in the least my admiration for his scholarly accomplishments.

GIOVANNI SARTORI

Columbia University

Comment on Tufte's Review (Vol. 74, June 1980, pp. 567-68)

TO THE EDITOR:

Unfortunately, Edward Tufte's review of our book *Can Government Go Bankrupt?* is vitiated by his disregarding the contents of the book. Two examples will suffice.

Tufte states that we assume citizens lack the sense to appreciate the value of welfare state benefits. In fact, we argue the opposite, devoting the whole of chapter 3, entitled "Everybody Benefits," to explaining the popularity of welfare state programs.

Tufte states that our "remedy is reduced government spending" (p. 567). Assuming Tufte is familiar with the difference between a decrease in public expenditure and a reduced rate of increase, we conclude that he must not have read the 12-page section in which we recommend the latter, captioned "How to Put the Brakes On."

Fortunately, persons such as Alan Peacock, Aaron Wildavsky, and even a Labour Cabinet minister, have read the book and commented favorably about it. We leave third parties to judge whether their opinion or that of your reviewer is to be taken more seriously.

RICHARD ROSE

University of Strathclyde

B. GUY PETERS

Tulane University

Reply

TO THE EDITOR:

Rather than defending their book by substantive argument, the authors say naughty things about the reviewer and then round up a hapless trio of book-chat authority. Although A. Peacock, A. Wildavsky, and a Labour minister have yet to endorse my review, I will stand by every word. The cited chapters do not refute in the slightest the two sentences of the review now grumbled about. My evaluation of the book remains (p. 568):

> Skepticism about the growth of the public sector has been advanced with far greater depth, sophistication, and clarity by many including George Stigler, William Niskanen, Assar Lindbeck, the public choice crowd, and indeed the median *Public Interest* essayist. . . .
>
> Aaron Wildavsky is quoted on the jacket flap: "This splendid book is essential for understanding the crisis of governability in the welfare state." It is nothing of the kind; rather it is the sort of book that leads economists to look upon political science as political scientists look upon sociology.

EDWARD R. TUFTE

Yale University

Comment on Cantori's Review (Vol. 74, March 1980, p. 224)

TO THE EDITOR:

The review of my *Egypt's Uncertain Revolution Under Nasser and Sadat* by Louis J. Cantori opens with the observation: "The merit of this book lies in the lucidness of its writing and the relative comprehensiveness of its coverage of Egyptian political history since 1952." Alas, the rest of the review is an argument that the book has little to offer the specialist in comparative politics or Egyptian affairs.

Cantori is, of course, entitled to his view. In fact, I think it is a revealing one about prevailing notions of the specialist. But I do feel obligated to point out that the evidence presented as the basis for the judgment consists of an incorrect assumption and a misreading.

Cantori writes that it is my "ignorance"—"unintentionally revealed" as a comparativist, "profound" as an orientalist—that excludes me from the circle of specialists. Consider now the reasoning that produced that judgment.

In the first instance, I am taken to task for writing in my preface that it "makes no effort to ensnare Egypt in yet another formal and ahistorical taxonomy" (p. vii). It is in this comment that Cantori finds "an unintentionally revealing confession of ignorance of dynamic theories of comparative politics (e.g., S. Huntington, *Political Order in Changing Societies*)." Cantori is deluding himself. If such theories do exist, they must be among the most closely guarded secrets in American social science. Cantori does not cite his own work. But I must protest that the reference to Huntington is unfair. After all, in his well-known "The Change to Change" article, Huntington did label clearly his own "transitional approach" the "*very modest first steps toward* the formulation of general theories of political dynamics" (1971, emphasis added). Readers of *Political Order* with an interest in Egypt welcomed the modesty. In that book Huntington developed the typological notion of Egypt as "praetorian society." It was precisely this interpretation of modern Egypt that stimulated my determination that *Egypt's Uncertain Revolution* would not be another arid, artificial, and misleading exercise in classification. (As a biographical note, I should add that my comparative work on Egypt began many years ago with a seminar paper for Huntington who subsequently chaired my general exams, and then got me my first job—in comparative politics!)

But it is the case of my inadequacies as an orientalist that reveals the reviewer's power as a critic. Cantori writes that my point of view "shows a profound ignorance of Egyptian poetry, religion, and music from the 1920s onwards." He bases his extraordinary view on a misreading of one paragraph. In a brief historical introduction I offer a logical reconstruction of the colonial situation in Egypt that led to the rise of nationalist resistance. My argument is that the choices offered to Egyptians in such a situation were intolerable and therefore rejected. In particular, the racist character of the British view of the Egyptians is underscored:

> In his classic study of the colonial experience Albert Memmi observed: "It is remarkable that racism is a part of all colonialisms under all conditions." British colonialism in Egypt proved no exception, despite the undoubted achievements registered by the British in the spheres of agriculture, finance, and communications. Lord Cromer, the all-powerful British proconsul from 1882 to 1907, best recorded the racist underside of British colonial rule. In his memoir, *Modern Egypt,* Cromer eloquently defended Britain's role in civilizing "barbarous" Egypt. To read Cromer is to understand the cultural amnesia of the colonized Egyptian: better to forget the "coarsenes" of his literature, the "cruelty" of his mores, and "the lack of harmony" of his music. To become human is to leave this cultural wreckage and barbarism of a "subject race." But to go where? The Egyptian cannot retrieve his humanity by assimilation, for to adopt the ways of the colonizer is to become the clever monkey, the good imitator, but never the genuine article. Cromer unintentionally has revealed the essential frustration of the colonial situation: for the colonized Egyptian there is no way out.

Here is Cantori's reading of this paragraph:

> Consider, for example, Baker's use of Cromer (the arch-colonialist) as a person who defined Egyptian culture in terms of its "coarseness," "cruelty," and the "lack of harmony" of its music. Egyptians are then said to have undergone "cultural amnesia" at the presumed inspiration of Cromer's remarks. This point of view shows a profound ignorance of Egyptian poetry, religion and music from the 1920s onwards.

I was pleased to note that Cantori's listing of the charmed circle of "truly original scholars of modern Egypt" includes no Egyptians (and no Soviets). In my own unhappy exile, it seems, at least, I have not been deprived of all my friends.

RAYMOND BAKER

Williams College

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Cantori, Louis J. (1980). Review of *Egypt's Uncertain Revolution Under Nasser and Sadat* by Raymond Baker. *American Political Science Review* 74: 224.

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Reply

TO THE EDITOR:

Two brief comments regarding Raymond Baker's response to my review of his *Egypt's Uncertain Revolution Under Nasser and Sadat.*

First, the point regarding my criticism of the book from a comparativist point is not the "theological" one of whether "dynamic theories of comparative politics" exist or not. It is rather Baker's failure to direct his concerns in the book to any social science generalizations whatsoever (short of a "genuflection" on pp. 11-13). Second, Baker has usefully made my point for me by quoting himself: "To read Cromer is to understand the cultural amnesia of the colonized Egyptian. . . ." I think that a reader is entitled to interpret this as meaning that Baker meant to castigate Cromer à la Memmi *and* to state that Cromer was essentially correct, Egyptians were undergoing "cultural amnesia."

My only comment would be that the major theoretical and literary works of the twenties and thirties, e.g., Shaykh Ali Abd al-Raziq, *Al-Islam wa usul al-hukm* (1925), Tawfig al-Hakim, *Awdat al-Ruh* (1926-27), Taha Husayn, *Fi al-shir al-jahili* (1926) and *Mustaqbal al-thaqafa fi misr* (1938) may have reflected intellectual controversy of a high order and even a crisis of cultural direction (Arab Islamic, Egyptian, Greco-Roman or Mediterranean) but in no manner can they be said to constitute "cultural amnesia."

LOUIS J. CANTORI

University of Maryland, Baltimore County

Clarification of Haviland Review (Vol. 74, December 1980, pp. 1100-01)

In response to two letters about his review of *Decision on Palestine: How the U.S. Came to Recognize Israel* by Evan M. Wilson (Hoover Institution, 1979), H. Field Haviland, the reviewer, submitted the following clarifications:

1. The citation of 1914 as the date of the Balfour Declaration was a typographical error. It should have been 1917.

2. The United Nations General Assembly's resolution of November 29, 1947, greatly advanced the target date, by which time the Arab

and Israeli states were to become independent, from September 1, 1949, recommended by the U.N. Special Committee on Palestine, to October 1, 1948.

3. It was in 1973 that President Truman made his comment about comparing the pressure exerted on him regarding the Palestinian question and the dismissal of General MacArthur.

EDITORIAL NOTE

The following articles have tentatively been scheduled to appear in the September, 1981, issue:

John Higley, Harvard University, and Gwen Moore, State University of New York at Albany, "Elite Integration in the United States and Australia"

Calvin C. Jillson, Louisiana State University, "Constitution-Making: Alignment and Realignment in the Federal Convention of 1787"

Robert J. Morgan, University of Virginia, "Madison's Analysis of the Sources of Political Authority"

Timothy J. O'Neill, Tulane University, "The Language of Equality in a Constitutional Order"

Frederick G. Whelan, University of Pittsburgh, "Citizenship and the Right to Leave"

Alfonso J. Damico, University of Florida, "Dewey and Marx: On Partisanship and the Reconstruction of Society"

Jack L. Noragon, Ohio Legislative Service Commission, "Political Finance and Political Reform: The Experience with State Income Tax Checkoffs"

Bryan D. Jones, Wayne State University, "Party and Bureaucracy: The Influence of Intermediary Groups on Urban Public Service Delivery"

Virginia Sapiro, University of Wisconsin, Research Frontier Essay: "When Are Interests Interesting? The Problem of Political Representation of Women"

Irene Diamond, Purdue University, and Nancy Hartsock, Johns Hopkins University, "A Comment on Virginia Sapiro's 'When Are Interests Interesting? The Problem of Political Representation of Women' "

Erratum

We apologize for an error that appeared in Richard D. Shingles' article, "Black Consciousness and Political Participation: The Missing Link" (March, 1981). On p. 81, cell *b* of Model I, Table 2, should read: "Optimal high-initiative policy behavior."

The corrected table is presented below:

Table 2. The Joint Effects of the Sense of Internal Political Efficacy and Political Trust on Different Modes of Political Participation, Controlling for Prior Motivations

Model I: Given no other motivation to participate

		Trust in Government	
		High	Low
Internal Political Efficacy	High	*a* Optimal high-initiative allegiant behavior	*b* Optimal high-initiative policy behavior
	Low	*c* Low-initiative allegiant behavior	*d* Total withdrawal from conventional politics

Model II: Given prior motivation to participate

		Trust in Government	
		High	Low
Internal Political Efficacy	High	*a* Optimal high-initiative allegiant and policy behavior	*b* High-initiative allegiant and policy behavior
	Low	*c* Low-initiative allegiant and policy behavior	*d* Total withdrawal from conventional politics

Source: Compiled by the author.

BOOK REVIEWS

Political Theory and Methodology

Social Justice in the Liberal State. By Bruce A. Ackerman. (New Haven, Conn.: Yale University Press, 1980. Pp. xii + 392. $17.50.)

Bruce Ackerman's book will doubtless remind most readers of the works of Rawls and Nozick, and it can be expected to have a similar if somewhat lesser impact. Lesser, because Ackerman lacks the panache and wit of Nozick, the philosophical maturity and meatiness of Rawls. Similar, because Ackerman attempts the same sort of comprehensive theory of justice, broadly within the liberal tradition, as they did.

All three share a methodological commitment to "constrained performance," that is, to the attempt to generate principles of justice from a performance or action undertaken in specially defined or constrained circumstances. This method is to supply the positive normative principles which, it is felt, can no longer be supplied through an appeal to nature or history, the traditional appeals of previous political philosophies. For Ackerman, the performance is a dialogue, brought into existence each time the legitimacy of a power relation is challenged. Power relations, including rules for distributions of the goods of the world, can be justified if and only if they can be justified through a dialogue, carried on within the constraint set by the principle of neutrality: "No reason is a good reason if it requires the power holder to assert: (a) that his conception of the good is better than that asserted by any of his fellow citizens or (b) that, regardless of his conception of the good, he is intrinsically superior to one or more of his fellow-citizens" (p. 11). At first glance, it appears that Ackerman overstates the role of the dialogue performance in his own theory; for all the *talk* of dialogue, what he really means is that all power relations must be justified by arguments that conform to certain criteria (e.g., neutrality), a position weighing the act of dialogue no more than any political philosophy which attempts to justify power relations. The appearance to the contrary notwithstanding, the performance of the dialogue remains central to Ackerman's enterprise. Without the performance, it would be simply impossible to generate Ackerman's liberal principles of justice, or perhaps any principles of justice, for the neutrality constraint supplies only a basis for disqualifying certain kinds of claims, and not for qualifying any. In emphasizing the performance of the dialogue, Ackerman attempts to side-step this problem by beginning with claimants and their claims to power. Their claims can stand, Ackerman seems to say if they, under challenge from potentially competing claimants, can give reasons which do not violate neutrality. Thus Ackerman attempts to turn the corner, deriving positive claims from his negative neutrality constraint. The question nonetheless persists, whether or not Ackerman's dialogic turn succeeds.

With great boldness and some ingenuity, Ackerman applies his dialogic method to a wide range of difficult issues, including not only such standard ones as justice in holdings, but also areas like genetic engineering and liberal education. He proceeds in stages, first developing an "ideal theory," characterized by a number of unrealistic simplifying assumptions, and then a second-best theory, relaxing the simplifying assumptions. Ideal theory assumes a "liberal commander," entirely dedicated to enforcing justice, and able to do so through possession of a perfect technology of justice. While Ackerman professes great hostility to state-of-nature theory, he begins in a very similar way. He assumes a group of human beings deliberating over how to distribute holdings on a virgin planet, where all holdings can be expressed in terms of a perfectly fungible item called manna. Under such conditions, the only principle of initial distribution that can be justified is equality. Since no generation can claim priority over any other, all members of later generations have a right to holdings at least equal to those of members of the first generation. From this follow the principles of population policy and inheritance law. Ackerman's principles for governing economic relations are somewhere between Rawls' and Nozick's. He presses the issue of initial holdings much harder than Nozick does, and demands that each generation has a right to what the first generation had, but more like Nozick than Rawls, he does not press for the imposition of a just pattern in holdings at every instant. Free exchange is one of the liberal freedoms that, with qualifications, Ackerman defends. Ackerman's relatively moderate position has much that is attractive about it, but also some problems as well. How, in the real world, do we discover the baseline first generation and its baseline holdings? Relevant also is Nozick's complaint against theorists who assume the problem of justice is essentially one of distribution of preexisting goods, like manna.

Second-best theory attempts to adjust to the absence in the real world of either a perfect will or a perfect ability to achieve justice. In place of the commander, Ackerman develops the institutional mechanisms of liberal democracy. In a world where all liberal rights cannot be secured, Ackerman proposes the principle of equal sacrifice: providing that no citizen may be asked to sacrifice more of his or her ideal rights in the real world than other citizens. The liberal state is obliged to take various forms of affirmative action to prevent and remedy such inequities.

Ackerman attempts to justify his method of liberal dialogue via a lengthy critique of the contractarian and utilitarian traditions, which finds them both necessarily violating neutrality in a similar way. Ackerman's critique of his predecessors suffers, however, from his failure in the case of, e.g., Rawls to understand correctly the doctrine of the original position or even the second principle of justice (consider, for example, pp. 266-69).

The core difficulty in Ackerman's book, however, lies in the tension between the commitment to dialogue and the Neutrality constraint. While he looks to Socrates as the paradigm of the liberal philosopher, he excludes at the outset the quest that animated Socratic dialogue—the quest for knowledge of the good. Neutrality rules out such knowledge in advance. He attempts to justify that exclusion via a route which this reviewer frankly finds unintelligible. In his last chapter he argues that a wide variety of routes, beginning at very different points, all lead to neutral dialogue. But then, he concedes, they don't necessarily lead there either. The argument from "liberal skepticism," i.e., that "there is no moral meaning hidden in the bowels of the universe," seems to have priority over other of the avenues to dialogue: Ackerman calls this position "the hard truth" (p. 368). But, echoing Nietzsche, Ackerman also concedes: "If God is dead, everything is permitted" (p. 369). Is Ackerman then merely a bright and decent law professor attempting to paper over the unresolved problem of nihilism as proclaimed by Nietzsche?

MICHAEL P. ZUCKERT

Carleton College

Against the Current: Essays in the History of Ideas. By Isaiah Berlin. Edited by Henry Hardy. (New York: Viking Press, 1980. Pp. liii + 394. $16.95.)

Those of us who do not save our *New York Review of Books* and who years later try vainly to cite details from what we remember as brilliant articles can be eternally grateful for this collection of Isaiah Berlin's essays. While several of them are old friends, the bulk of these 13 essays, having been originally published in much more obscure places, are new to most of us. What we have here is profoundly rich and rewarding. No student of modern political thought can read these essays without marveling at Berlin's ability to offer provocative and novel interpretations of writers like Machiavelli, Montesquieu, Marx, or Sorel. Whether one agrees with Berlin's own world view, with its skeptical, near tragically pessimistic variant of liberalism, or whether one agrees with his traditional history of ideas approach to the study of political thought, the reader of these essays stands in awe of their erudition, their range, and most significantly, their capacity to excite and to dazzle. Berlin is never boring or tedious. The reader is caught up in his prose and his arguments and wants only to have him there to agree with, to question, or to dispute.

It is hazardous to summarize a volume that contains so much and moves in so sprightly a way from the Italians Vico to Verdi or from the English Hume to Disraeli. But, surely, there is one thread that runs through most of these essays as it does through most of Berlin's writings. He is preoccupied with what Alfred Cobban years ago called "the revolt against the Eighteenth Century." Berlin is irresistibly drawn to write on those who repudiate enlightenment rationalism with its cosmopolitanism, its universalism, its optimism. While Berlin has characterized this revolt as a "German backlash against the French cultural domination of the Western World," he singles out as its first spokesmen a Frenchman and an Italian. Within the ranks of the *philosophes* themselves, Berlin sees Montesquieu subverting enlightenment dogma with his emphasis on the unique and the particular. The Italian, Vico, is much more central to Berlin's preoccupation, and several essays are devoted to him, emphasizing his rejection of a universal, immutable human nature, as well as the utopian belief in an ideal rational society. The "German backlash" is not neglected, however, for there are essays here on Hamann and Herder in which the heart of the anti-rationalist case is examined. While he is interested in their emphasis on spontaneous imagination, instinct, and insight, what most interests Berlin in their writings is nationalism.

Nationalism is, indeed, the aspect of the revolt against the Enlightenment that most fascinates Berlin and its understanding is at the heart of the best essays in this volume. Berlin moves from general speculations on the role of wounded pride in causing nations to exaggerate their own real or imagined virtues, to specific inquiry on the effects on Marxism of its systematic failure to appreciate the true nature of nationalism. Two essays in particular should be singled out in terms of the latter concern. There is a lovely piece on Moses Hess which helps us make sense of that enigmatic figure in the socialist tradition. More important, and indeed the most brilliant essay in the collection, is a discussion of Disraeli and Marx. Berlin links their writings and lives to their differing quests for identity as Jews. It is provocative, even outrageous, Berlin at his best, as he charts Disraeli's journey to the nation and aristocracy, and Marx's to the universal class and the proletariat, all as part of a desperate search for identity.

Few have written on romanticism and nationalism as well as Berlin has. One wonders, in fact, why his scholarship is so preoccupied with these themes. The answer may lie in Berlin's own skeptical liberalism, his vigorous denial in his other writings of the assumption in notions of "positive freedom" that there is a "final solution of the question of how men should live." Ultimately, Berlin's heroes are the "makers of pluralism." One such hero is Machiavelli who realizes "that ends equally ultimate, equally sacred, may contradict each other, that entire systems of value may come into collision without possibility of rational arbitration" (p. 74). Another hero is Herder, the critic of enlightenment hubris, who recognized the potential variety of cultures and nations, all offering equally ultimate paths to human fulfillment and none with a monopoly on reason or universal truth.

ISAAC KRAMNICK

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The Sexism of Social and Political Theory: Women and Reproduction from Plato to Nietzsche. Edited by Lorenne M. G. Clark and Lynda Lange. (Toronto: University of Toronto Press, 1979. Pp. viii + 141. $15.00, cloth; $5.00, paper.)

This edited collection contains essays on Plato, Locke, Rousseau, Hume, Hegel, Marx, and Nietzsche. It has the usual unevenness of edited collections but is useful both in substance and in clarifying an important methodological problem faced by any feminist re-reading of Western political thought.

Lorenne Clark and Lynda Lange state that each author has assumed, first, "that an adequate political theory must allow the same rights, duties, privileges, and liabilities to all persons regardless of gender" and second, "that reproductive labour is as socially necessary and humanly important as productive labour" (p. viii). This is the ground on which they assess the works of the theorists considered. The editors "hope that the essays in this collection show ample reason for concluding that traditional political theory is utterly bankrupt in the light of present perspectives" (p. xvii). This is too much to hope for from a series of essays, and the editors themselves add that what is needed is rather the development of theories which "reflect a deeper understanding of our historical position and a profoundly changed perception of sexual and reproductive reality" (p. xvii). The book as a whole indicates both the importance of a feminist critique of Western political thought and the complex theoretical repercussions of assumptions and arguments that women have no place in public life. The essays on Plato, Rousseau, Hegel, and Marx demonstrate the possible fruitfulness of approaching theorists from the perspective of the two assumptions the editors describe, while the essays on Hume and Locke illustrate some of the pitfalls of such an approach.

Lange's essay on Plato demonstrates that his recommendations for including women in the guardian class in *The Republic* do not "necessarily imply that *any* of them will become guardians" (p. 4). And her essay on Rousseau indicates that one cannot solve the problems posed by a theorist's exclusion of women from citizenship by simply reading the term "man" as "men and women." Mills points to the ways in which Hegel's assumptions about the nature and proper role of women involve him in circular reasoning (p. 90) and lead him to contradict "his own analysis of how one can achieve particularity" (p. 84). These non-obvious conclusions indicate some of the interesting theoretical results which follow from questions grounded on the assumptions of gender equality.

The essays on Locke and Hume, however, pose an important methodological question. Clark has a tendency to argue that to the extent that Locke held that (1) there is a natural inequality of the sexes, (2) that reproduction is not a central fact of political life, and (3) that the family is a " 'natural' institution" he can be accused of sexism (pp. 16-17). Hume is accused of sexism both because the double standard he supports implies that "men are the possessors of both property and children, and the only providers of goods from the world outside the home" (p. 56), and because his methodological commitments make it impossible to raise questions about sexist discrimination as a moral issue (p. 65). It is of course true that Locke and Hume hold such positions. But one is tempted to remark, "So what?" If one takes the kind of materialist position that most authors in this book seem to, one cannot expect the political theories of the past to share contemporary assumptions. The important question is not whether they share the assumptions of gender equality, but rather what role their treatment (or nontreatment) of women plays in their theories and what effects it has. The essays in this book outline several possibilities. The assumption of women's inferiority may (1) be consistent within the theory as a whole (Hume), (2) involve the theorist in circular or contradictory reasoning (Hegel), (3) undermine the strength of a theory as a whole (Rousseau), or (4) represent a deep lacuna in a theory which purports to be universal (Marx). This feminist reading of Western political thought should perhaps be more explicitly informed by Marx's argument that theorists can only confront the problems which history presents. O'Brien, despite her criticism of Marx for never coming to terms with the social relations of reproduction, argues that only very recently have issues of reproduction, and the possibility of rational control of reproduction (in addition to production), "become thinkable" (p. 113). While I am dubious of her location of this possibility in the technology of potentially universal contraception, her point that the writing of political theory takes place in a historical context indicates that to the extent that the tradition of Western political thought informs and justifies contemporary political practice, a careful study of the effects of what we now term sexist assumptions is essential. Despite the problems posed by some of the essays, this book makes a real contribution to this effort.

NANCY C. M. HARTSOCK

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Demand Revelation and the Provision of Public Goods. By Edward H. Clarke. (Cambridge, Mass.: Ballinger, 1980. Pp. xix + 237. $26.50.)

A decade ago two graduate students in economics (Edward Clarke and Theodore Groves, working independently) discovered what seemed to be a solution in principle to the famous "free-rider" problem of social choice. Part 1 of Clarke's book gives his own account of the "demand-revealing" (DR) mechanism and its significance; Part 2 is a discussion of how the practical difficulties of implementing DR might be handled.

This work came to wide attention only four years ago with the publication of lead articles in two of the most prestigious journals in economics (T. N. Tideman and G. Tullock, "A New and Superior Process for Making Social Choices," *Journal of Political Economy*, December, 1976; T. Groves and J. Ledyard, "Optimal Allocation of Public Goods: A Solution to the 'Free Rider' Problem," *Econometrica*, May, 1977). But Clarke's is already the second book-length exposition, following J. Green and J. J. Laffont, *Incentives in Public Decision-Making* (North Holland, 1979). In addition, there have been several compendia of papers; many individual articles; and at least two special issues of journals: *Public Choice* (Special Supplement, Spring, 1977) and *Review of Economic Studies* (April, 1979).

In terms of technical finesse, Clarke's book is not in the same class as the Green and Laffont book; but for political scientists interested in why economists have been so taken with the DR idea, Clarke's book will probably do better, and at almost half the price. However, my own perhaps overdeveloped sense of frugality leads me to recommend Dennis Mueller's *Public Choice* (Cambridge, 1979), which is a factor of two again cheaper and contains a good chapter on DR amidst a stimulating survey of the whole literature which attempts to apply economic theory to social choice. A (contrasting) view of DR, which I will sketch in a moment, is developed in a chapter of my *Selfishness, Altruism and Rationality* (Cambridge, forthcoming).

The problem that DR is intended to resolve was first formulated in a really clear way by Samuelson in an elegant three pages of the *Review of Economics and Statistics*, 1954. *If* we accept the distribution of income as fair (an "if" stressed by Samuelson but often ignored since), *then* the optimal level of supply for a public good will be where the summed (marginal) demand across all citizens just equals the marginal cost of the good. However, Samuelson argued that citizens would find it in their interest *not* to reveal their true preferences. Hence a political process would be required to make social choices. But what the proponents of DR believe they have shown is that, given (in Clarke's phrase, p. 110) "rational, egoistic, maximizing individuals with full information," it is possible to design a special tax (aside from an individual's normal tax, and paid only on the change in the social choice due to his own response) such that truthful demand-revelation is in the self-interest of each voter. What this amounts to is that it is possible to offer everyone an opportunity to buy a shift in the social choice with the cost (the special DR tax) arranged in such a way that we will be able to infer all voters' true demand from their responses to this incentive arrangement.

It is important to understand here that DR does not purport to overturn Samuelson's "free-rider" argument by changing his assumptions. To the contrary, Samuelson's own analysis is based on the same standard (within economic theory) behavioral assumptions. That is why Mueller can define the literature he surveys as the body of work which applies an axiom of rational egoism to politics.

Clarke's book mentions in passing the possibility that human beings may not actually be the egoistic maximizers of economic theory. But it mainly follows customary usage in treating this question as not really open to discussion. Hence Clarke's treatment of difficulties for DR is focused on problems relating to the artificiality of the "full information" assumption, asssuming the egoistic assumption to be a good enough approximation of human motivation.

However, it happens that DR provides an occasion on which it is especially simple to see that the customary assumptions about "economic man" are empirically indefensible. Clarke and other proponents of DR have pointed out that as the number of participants in a DR choice process increases, the incentive tax on a typical individual approaches zero. It turns out, for example, that for a DR choice in a country the size of the U.S. and a budget item on the order of $1 billion, the special-incentive tax on a typical voter would be on the order of 1/100,000th of a penny. This raises obvious practical problems: how would you collect such a tax, or reformulate DR in a way that the taxes would be small enough to be a plausible adjunct of the decision process, yet large enough to be meaningful to voters? It is concerns such as these that motivate Part 2 of Clarke's book and much else in the DR literature. But a more fundamental issue arises. Under DR, the effect of a vote depends on the size of the DR tax incurred. But DR proponents own calculations of plausible DR taxes for rational egoists are easily shown to imply that a voter willing to spend even a penny to increase the effect of his or her vote could increase that effect by a factor of 300. Then is it credible that a human being motivated to vote in the first place would not wish to spend even a penny to make that vote effective?

The difficulty here turns out to lie not in the assumption of rationality (internal consistency), or in any merely practical problems, but squarely in the assumption of egoistic motivation. Further, it is easy to show that if in fact real voters would spend a penny to increase vastly the effect of their vote, the demand information that would be elicited by DR *cannot* be the summable demand required to define efficient social choice.

Perhaps these results are not even very startling. The "economic man" model has never been able to provide a plausible account of the reason a person votes in the first place. Is it then any surprise that the same model of human choice also does not tell us much about how a person will vote, given that he or she votes?

HOWARD MARGOLIS

Massachusetts Institute of Technology

The Practice of Political Authority: Authority and the Authoritative. By Richard E. Flathman. (Chicago: University of Chicago Press, 1980. Pp. xii + 274. $20.00.)

In this book, Richard Flathman offers an analysis of the concept of political authority and a justification for the practice of it. The analysis is intended to tell us what political authority is and is not. The justification is meant to show that the practice of political authority, correctly understood, is morally acceptable.

As a framework for his discussion, Flathman identifies two types of theories about authority, the substantive-procedural (S-P) type and the formal-procedural (F-P) type. The former seeks to identify, understand and assess authority "in terms of its alleged end or purpose. . ." (p. 2). Flathman finds the "supreme instance" of this type in "Plato's argument concerning the philosopher king in the *Republic*" (p. 16). Contemporary representatives of this approach are writers "whose thinking about politics is substantially influenced by religious considerations, and . . . social scientists of a functionalist bent" (p. 16). By contrast, according to F-P theories, "Authority has to do with form not function, procedure not purpose, antecedents and credentials not substantive content or consequences. Considerations of content or merit, of ends and purposes, are categorically irrelevant to understanding authority and to participating in authority relations" (pp. 14-15). "Plato's most celebrated competitor among F-P theories," Flathman says, "is in Hobbes' *Leviathan*" (p. 16). And the "best developed and most uncompromising [contemporary] version" (p. 5) of an F-P theory, according to Flathman, is Michael Oakeshott's.

Flathman contends that the F-P approach is the more promising for achieving an adequate analysis of the concept of authority, while the S-P approach affords a better basis for a moral justification of the practice of authority (to which justification only a little less than the last third of the book is devoted). However, he finds all the F-P analyses of authority he has encountered inadequate since they fail to give proper weight to considerations which are prominent in S-P theories. Contrary to the F-P analyses he considers, Flathman argues that (1) "teleological or consequentialist considerations . . . are internal to rule making and rule following. . ." (p. 6), and that (2) "in order for there to be rules that carry and bestow *authority* . . . there must be values and beliefs that have *authoritative* standing among the preponderance of those persons who subscribe to the authority of rules" (p. 6). Because these values and beliefs do not acquire their authoritative status as a result of promulgation or adoption by agencies with authority, F-P theories tend to exclude them from their analyses of authority, thereby rendering "incomprehensible the distinctive feature of authority, that is rules and conduct guided by rules" (p. 7).

In the course of working out his own analysis, Flathman discusses at length various conceptual features of rules and rule following, the problems of reconciling political authority and moral autonomy, and how authority differs from apparently related phenomena such as power and force. In his defense of the practice of authority, Flathman considers various political arrangements in which the tension between authority and autonomy would be minimized, and the ways in which political authority, properly constituted, would actually enhance the agency of those subject to it. Because in both the analytical and justificatory portions of the book Flathman works toward his own position by considering critically the views of other writers on the subject, the book includes a wealth of information about the literature on authority. In addition to Oakeshott's, the views of figures as diverse as Hannah Arendt, Peter Bachrach, Steven Lukes, Joseph Raz, G. E. L. Anscombe, Robert Paul Wolff, Ludwig Wittgenstein, Richard Friedman, Hobbes, Kant, and Rousseau receive more than passing attention.

Indeed, the chief value of the book is to the non-specialist for whom it affords an excellent entrée to the main problems regarding the concept and practice of authority, the principal approaches that have been taken to those problems, and much of the traditional and contemporary literature on the subject. However, the specialist who already knows this topic and its literature will have reason to doubt that the book advances the discussion substantially, primarily because of the way Flathman has construed some of the competing views on which his position purports to improve. The prime example is his treatment of Oakeshott's position, in which Flathman ferrets out crippling mistakes which, as one learns on a fairer reading of Oakeshott, are not there in the first place. As a result, Flathman's improved version of Oakeshott's theory turns out to be virtually equivalent in substance though not in style to the theory on which it was supposed to improve.

LEON GALIS

Franklin and Marshall College

Educative Democracy: John Stuart Mill on Education in Society. By F. W. Garforth. (New York: Oxford University Press, 1980. Pp. viii + 256. $27.50.)

One ought to have a good reason for writing a new book on a theorist about whom so much has already been written. F. W. Garforth has one, and his focus upon John Stuart Mill's ideas on education should provide us with a good reason for reading this book on Mill.

Unlike Locke, Rousseau and even Bentham, Mill did not write anything dealing exclusively with the education of children. Only on the occasion of delivering the Inaugural Address at the University of St. Andrews did he devote himself to the question of what constitutes a good liberal education. As Garforth's discussion indicates, Mill's definition of education helps explain why he had not previously written a work on education or, more accurately, why he must have believed that all of his previous works were devoted to that subject. According to Mill (here he follows his father), education embraces the whole environment of men and women; it encompasses whatever helps to shape the human being. "In its largest acceptation it comprehends even the indirect effects produced on character and on the human faculties by things of which the direct purposes are quite different; by laws, by forms of government, by the industrial arts, by modes of social life, nay, even by physical facts not dependent on human will, by climate, soil and local position" (p. 38).

In defining education so broadly, Mill runs the risk of neglecting to analyze carefully the nature and purposes of educational institutions more narrowly understood. Not surprisingly, therefore, he retreats to a narrower definition in his *Inaugural Address* and in his specific discussions of education in *On Liberty* and the *Principles*. There he identifies education with a set of formal institutions whereby older generations endeavor to hand down knowledge, culture and moral values to younger generations. However, Mill's broader definition also has an advantage: it shifts our focus to the educative potential of social institutions other than schools. In particular, Garforth emphasizes Mill's idea that the political institutions of a good society ought not only to legislate, execute and administer laws, but that they ought also to educate citizens. Mill argues that representative democracy is the only form of government suitable to political education in advanced societies like England and the United States. Unfortunately, Garforth misses the opportunity of evaluating Mill's reasons for supporting nondemocratic rule in India and other less-developed countries. An "educative society" (Garforth's term) would be one in which all major social institutions, including the family, government and schools mutually reinforce five primary values that he attributes to Mill: happiness, individuality, truth, freedom and altruism. Or at least, the tension among the values fostered by these institutions should be a creative, rather than a destructive one, resulting in a "self-generating, self-nourishing education process which moves by its own momentum towards improvement" (p. 39).

Garforth realizes that there are obvious problems with this ideal, and he asks the most critical questions of it: "What values should be fed into the process? who will select them? may not the educative society degenerate into a totalitarian monster. . . ?" (p. 40). The answers that he attributes to Mill, however, are sometimes less than fully satisfactory in themselves or as interpretations of Mill's own views on the question. Ironically, given the claims of utilitarianism to be a practical theory which can resolve value conflicts, one strains to imagine the practical implications of this vision of an "educative society" devoted to the five values that Garforth credits to Mill. Garforth adds to this pantheon of values that "a primary function of education in any society is to promote the cohesiveness and continuity of that society." Yet he does not indicate how Mill might resolve the historically recurring conflict between those who argue that social cohesion is *the* primary function of education and those who propose above all to teach children how to think critically, to pursue truth or to discover the good life. Garforth quotes Mill's essay on Coleridge but fails to pursue the implications of Mill's claim there that the principles of individual freedom, political and social equality are "the only shape in which the feeling [of social unity] is likely to exist hereafter." Nor does he supply an answer to the second question, either in Mill's name or his own. Instead, he appreciates the tension within Mill's thought between his support for representative democracy and his desire for rule by the best. To answer the last question, Garforth offers Mill's view that the state ought to enforce but not to direct education (p. 123). Parents should be directly responsible for selecting a good education for their children and should be fined by the state if their children fail to meet minimum educational standards. Similarly, teachers should be held responsible for the results of their teaching. Garforth "excuses" Mill on historical grounds for the latter view (p. 141).

Perhaps the most critical and original educational claim Mill made was that only a representative democracy could be a form of self-education (pp. 178-80). It is a difficult claim to interpret literally, since if nothing else, democracy means collective rule by the people over one another. Taken seriously, therefore, this claim would push us in the direction of exploring Mill's understanding of the self and of the proper educative structure of democratic institutions—or else in the direction of criticizing his claims for representative democracy as a form of self-education. Garforth (rightly, I think) moves in the former direction, but the scope of his proposed enterprise combined with his frequent attempts also to account for Mill's ideas by referring to his personal life and his historical context prevent Garforth from reaching his proposed destination. Political theorists should still appreciate the fact that *Educative Democracy* reopens an extremely important question: What is the proper role of institutions other than schools in educating democratic citizens?

AMY GUTMANN

Princeton University

Manipulatory Politics. By Robert E. Goodin. (New Haven, Conn.: Yale University Press, 1980. Pp. x + 250. $17.95.)

Manipulatory Politics offers the analytical political philosopher's half of an analytical political philosopher's conception of a collaborative effort with empirical political science to study a topic that a generation touched by Watergate and other political scandals will find timely, that is in keeping with the fundamental traditions of the discipline, and that "blends the empirical and the normative in a particularly constructive fashion" (p. 1). Manipulatory politics is a subset of power politics: power exercised (1) deceptively and (2) against the putative will of its objects. Goodin argues that "manipulation constitutes the evil core of the concept of 'power' . . ." (pp. 8-9).

The study is couched in rationalistic terms rather than psychological ones, "not because they point to a more morally important side of the phenomenon (they do not), but rather because they point to a side of the phenomenon more subject to our control" (p. 29). It purports to offer descriptions of casess that "are thick with both theory and detail" (p. 34) in the "preliminary task of cataloguing various modes of political manipulation and assessing them from a moral point of view" (p. ix).

The bulk of the work is devoted to 20 mechanisms of manipulation, grouped in six categories: untruth, linguistic traps, rhetorical tricks, symbolic rewards, rituals, and obviousness. Goodin asks a standard set of questions about each: (1) Is the interference deceptive? (2) Is it contrary to the putative will of those subject to it? (3) How persistent is the effect of the manipulation? and (4) Are any distributive biases inherent in it? The general pattern of his conclusions "indicates how very little cause for concern most of the tricks surveyed in Chapters 2 through 7 provide" (p. 233). He argues that most of them are not necessarily manipulative at all, and even when they are, their effects are often easily overcome.

This "broadly reassuring" conclusion is subject to an important qualification. Goodin's catalogue is "nowise exhaustive." He claims to have discovered in the rigging of the obvious "one very troubling form of political manipulation which has long gone unrecognized" (p. 236) and which is both "perverse and pervasive" (p. 36). And we do not know what other manipulatory tricks may be succeeding behind our backs. Rigging perceptions of obviousness shares with the two other "altogether worrisome" techniques (debasing language and magico-religious political rituals) three traits: they are distributively biased in favor of the powerful, they are to a large extent impersonal strategies of domination, and they tend to be self-perpetuating. The upshot of the argument is that we need to "focus attention first upon the biases of the political system" (p. 238).

The book thus offers three sorts of points of departure to empiricists: one might argue against Goodin's very impressionistically supported claim that most techniques of manipulation are not that important. One might study the three worrisome forms in a richer substantive context. And finally, one might accept his invitation to search for undiscovered forms of manipulation. All three sorts of study appear to be established firmly enough by Goodin's effort that they will not self-destruct, and the basic suggestion that we attend to biased, impersonal, and self-perpetuating traits of political systems seems wholly appropriate.

Although *Manipulatory Politics* may succeed on its own terms, several reservations must be entered. The choice of a "rationalistic" model is admitted to neglect the more important side of the topic. A perspective with more analytic (psycho-, not linguistic) depth would not neglect the rational, but rather set it in a much richer theoretical context, behavioristic prejudice notwithstanding. And Goodin's combination of analytic philosophy and empirical science, of the normative and the empirical, still retains a strong positivist flavor. It is disappointing that the past 20 years of innovative work in the history and philosophy of science should still have so little impact on our discipline. Analysis is not a substitute for theory.

JOHN R. CHAMPLIN

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Liberal Equality. By Amy Gutmann. (New York: Cambridge University Press, 1980. Pp. xi + 318. $34.95, cloth; $10.95, paper.)

As liberty was the predominant theme of much of nineteenth-century political theory, so equality has come to dominate the scene in the twentieth century. How significant is this shift? Amy Gutmann has set out to discover how far liberal principles can be pushed toward egalitarianism. She has done well. By "liberal principles" she means those that can be derived from an individual's interests; and by "egalitarianism" she appears to mean the belief that justice calls for movement toward greater equality than now prevails with respect both to material welfare and psychological satisfaction. She argues that liberal principles, properly extended, go further in that direction than is commonly recognized. But ultimately one reaches a dividing line between liberal and communal egalitarianism. The latter calls for a radical transformation of the moral consciousness. It depends on the proposition that "what constitutes an individual's interest cannot precede an understanding of what constitutes a desirable political community" (p. 219). Even the most extreme liberal egalitarianism, on the other hand, "does not require a complete remaking of moral consciousness" (p. 229) and assumes some knowledge of an individual's interest at the outset. The philosophical issue in the final analysis, she says, hinges on who must carry the burden of proof—an unsatisfying but arguably inescapable conclusion.

After an initial chapter on the classical liberal foundations, the least satisfactory of the book, she deals successively with various liberal thinkers and groups of thinkers: to wit., J. S. Mill, the Fabians, "the relevant reasons approach" (Bernard Williams and Michael Walzer), John Rawls, and Rawls' critics. A chapter (the last except for a conclusion) on "Participatory and Distributive Equality Reintegrated" draws together and amplifies her positive arguments as they have emerged in the preceding discussions. Here most fully her own theory is set forth. It entails "participatory democracy" as an end state but not as a first step.

The classical liberals all assume some kind of human equality: with Bentham and James Mill it is equality of passions—self-interest, with happiness as its measure; with Locke and Kant it is rationality. Perhaps she makes the distinction more simple than it is and compression has forced me to state it more simply than she does. Both assumptions face challenges (as to the facts and as to what normative conclusions would in any case follow), which she considers and finds less than sufficient. It is, however, Mill's participatory ideal and his openness to state intervention that are particularly relevant to her concern, for it is the tension between the redistributive and the participatory ideals of liberalism that she wishes to resolve.

After an interesting discussion of the Fabians, she turns her attention to what she calls the "relevant-reasons approach" to liberal egalitarianism. She finds that both Walzer and Williams fail to follow the most profound implications of Marx's view of the self in accounting for the values of liberty and community, and that because of this failure we are left with no guidance as to "how far beyond the welfare state liberal egalitarianism can rightly extend" (p. 118).

Among the many points that Gutmann makes in her discussion of Rawls, one is especially worthy of note: Rawls is weak in relating his theory of justice to democratic procedures. Gutmann would modify and, in her judgment, improve his theory by placing substantive justice, in the form of welfare rights, ahead of the procedural justice of equalizing participatory opportunities. This she would do, in part, to protect minorities. Welfare rights would be given constitutional status as would the Difference Principle. Accordingly, she believes that the first stage of a liberal program must be the achievement of consensus on the theory of justice. The failure of many recent attempts to implement participatory objectives is attributable to the lack of essential pre-conditions, of which consensus on principles of social justice is perhaps the most important. Although at one point she professes ignorance as to how a socially just society can be attained with justice, that problem would seem to disappear (perhaps in favor of a still larger one!) if consensus on the rules of justice must come first.

The reader is left facing an even tougher question: is the Rawls-Gutmann theory of social justice even theoretically possible, given the Humean constraints, which she accepts? Will most people work hard enough in a society in which they have to improve the lot of the least advantaged before they can benefit themselves? Will executives devote time to inventing tax-avoidance schemes at the expense of socially productive thought? Questions of moral psychology—how to obtain a consensus on justice and how great self-serving incentives must be—are crucial. But this comment is less a criticism of the author than it is a description of the (reasonable) limits she has set herself.

The book has a useful bibliography and extensive substantive footnotes.

J. ROLAND PENNOCK

Swarthmore College

Philosophy and the State in France: The Renaissance to the Enlightenment. By Nannerl O. Keohane. (Princeton, N.J.: Princeton University Press, 1980. Pp. xii + 501. $30.00, cloth; $12.50, paper.)

The astonishing thing is the size of the gap which Nannerl Keohane has filled with *Philosophy and the State in France.* There are many studies of the Enlightenment, and especially of thinkers like Montesquieu and Rousseau; there is also much excellent scholarly work being done in the Anglo-Saxon world on Bodin and other political writers of the sixteenth century. Some of this work has filtered through into more general awareness. There is remarkably little on the French seventeenth century, however, and it is hard to think of any one who has turned into intellectual history the political ideas of the French in the three centuries before the Revolution. The very generosity of French writers such as Voltaire and Montesquieu in appreciating the virtues of the very different English political tradition has merely encouraged Anglo-Saxon parochiality, and the vast spread of interest in political ideas during the present century has done little to change the situation. Perhaps the cause is that Locke taught his countrymen to regard France as merely the source of a tyrannical absolutism; certainly the consequence has been a degree of blindness to the richness of what flowered when the *ancien régime* was at its height.

The most striking single feature of Keohane's treatment of the period is her emphasis on the tradition of *moralistes* from Rabelais and Montaigne onward. That she is enabled thus to extend the more familiar preoccupations of political theory results from the underlying schema used to help us focus the main features of French discussion in this period. The first and most familiar strand of thought is Absolutism which was distilled out of long-standing claims of the French monarchy and emerged with Bodin's theory of sovereignty. It came to be complemented in time with Richelieu's emphasis on *raison d'état.* In the rhetoric of Bossuet and the ambitions of the Sun King, this was the France which English Whigs found so terrifying they could barely distinguish it from the despotisms of Turkey or Russia. The second and perhaps less familiar strand was Constitutionalism which preserved France's mediaeval heritage of law and became "a distinctive blend of legalism, pluralism, and historicism" (p. 16). Keohane's third mode of argument is rather less conspicuously "given" in her materials, and she calls it Individualism. It "was not initially a way of thinking about politics so much as a philosophical exploration of the delights of privacy, based on a sharp separation between public and private life"

(p. 19). "The individual," she tells us, "made his appearance in French philosophy in the *Essays* of Montaigne."

The most striking thing about this individual was that he was developed out of disillusion and retreat from public affairs. The religious dissensions of sixteenth-century France encouraged many men of sensibility to retire from the brutalities and *bouleversements* of public life, often into philosophy. As with most developments in European thought, this exploration of individuality was strongly colored by the revival of classical doctrines, in this case Stoicism and, particularly, Epicureanism. Individualism resembled these classical doctrines in being an intellectual response to a world in which the individual seemed to have lost the capacity to influence events. Absolutism, apart from its historical roots in the ideology of the French monarchy, appears as a way to restoring peace to a nation ravaged for more than a century by strident sects and turbulent nobles. The internal emigration of many of Keohane's individualists is thus a response both to the disease and to the cure. There is, significantly, little like it in English thought, though it casts an illuminating light upon Hobbes to regard him as in some respects part of the province of French intellectuality. (His affinity, however, does not save him from being misunderstood as a believer in arbitrary despotism, as on occasion by Rousseau: Hobbes degenerates into Hobbism as Machiavelli into Machiavellianism.)

Thus it is that this most thoroughgoing exploration of the nature of modern individuality all took place under the aspect of a despairing realism, which imagined that men were steadily becoming more decadent and the world desperately lacking in classical virtues. Many French thinkers came to terms with religious dissension and commercialism, which initially bade fair to tear their society apart, by discovering that these things were based upon self-love; and then moving on to reflect that greed and the pursuit of selfish interests were, properly channelled, a useful form of social energy. The invisible hand, and the whole idea of the unintended consequences of human actions, were being explored in moral thought for more than a century before they finally flowered as the materials of a science of economics.

A concern with national wealth, however, was far from lacking through this period and already in the early seventeenth century the mercantilist idea of international trade as a zero-sum game was well established. In the *Testament* of Richelieu, particularly, we find a conception of the modern sovereign state as a bundle of resources, human and material, which must be bent to the direction of a single governing will. It is no doubt misleading to read into this doctrine the corruptions of later totalitarianism, but there is no doubt that all manner of despotic potentialities had been clearly grasped and that something altogether new and possibly sinister had appeared for the first time on the soil of Northern Europe. As Keohane is aware, the doctrine of *laissez-faire* which emerges out of mercantilism in this period had "more in common with the parent system than first meets the eye" (p. 158). She gives no warrant for the popular confusion of *laissez-faire* with liberalism.

Keohane writes in part to show that the seventeenth century in France, "far from being a wasteland in French political thought, was a pivotal and exciting period" (p. 13). A further preoccupation has been "to shed light on the political theories of Montesquieu and Rousseau, by showing how their arguments are rooted in earlier French thought" (p. 14). The result is to highlight, for example, Rousseau's enormous subtlety in analyzing the character of the individualist will in a modern society. Keohane sketches in the social and political background where necessary, but does not fuss about it. Her primary concern is with a set of ideas which are well worth attention even outside their social context, and there can be few students of modern political thought who will not find in the book much to be grateful for.

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Trust and Power: Two Works by Niklas Luhmann. By Niklas Luhmann. (New York: John Wiley, 1980. Pp. xix + 208. $28.95.)

Niklas Luhmann seeks to encompass major aspects of life in his book, and his ambitious intensity urges careful attention. *Inter alia,* he seeks to relate three theoretical approaches—system formation and differentiation, evolution, and an "as yet, tentative theory of symbolically generalized communication media" (p. 108).

Seven points may help orient the reader who takes on the tough task of tracking Luhmann's work. First, he is perhaps the most energetic "functionalist" since Parsons, although *Trust and Power* marks his first major exposure to English-speaking audiences.

Second, Luhmann is a theoretical dreadnaught who generates a turbulent wake. As Poggi gently notes in his very helpful introduction (p. viii): "Some reasons for the controversiality of Luhmann's work-in-progress may become apparent even from the following, brief presentation; others will perhaps suggest themselves to the critically-minded reader of the two works making up this volume."

Third, Luhmann reflects conviction about the promise of functionalism, as well as certainty about the failure of structural functionalism. This all but guarantees a range of critical attention. Those who loved Parsons' *Toward a General Theory of Action* will find much to critique in Luhmann's work even as they find some comfort in its gestalt; and those who could not abide Parsons' work may value Luhmann's revisionism even as they despair of his grounding in functionalism.

Somewhat more specifically, Luhmann insists that functionalism is a comparative method for relating "problems" to their range of alternative solutions. This contrasts sharply with the structural functionalist search for causal relationships. In Luhmann's words (p. ix): "The functional argument does not consist in proceeding from a previously ascertained performance to a corresponding need, thus justifying the existence of the performance. A logical equation only exists between a formulated problem of reference and the class of all possibilities of performance."

Fourth, Luhmann avoids encountering the fullness of these potentialities, e.g., how to define "a formulated problem of reference"? Parsons attempts a conceptual double-play: *the* problems are the ones associated with system maintenance, whose major parameters are specified as "functional prerequisites," action to meet which is normatively constrained. Luhmann seeks to avoid relying on values to provide direction. Rather, he fixates on *a* formulated problem of reference—what Poggi calls (p. x) "the problem of problems" for Luhmann—"complexity" and its reduction by differentiated systems of thought and action.

Fifth, I was not satisfied with Luhmann's "complexity," which gets only very general attention, and by design. As Luhmann explains (p. 6): "Complexity . . . has to be defined in very abstract terms [as] a distinction between system and environment and in terms of a system's potential for actualization. This concept then signifies a number of possibilities which are opened up through system formation."

Sixth, conceptual development aside, Luhmann's focus involves what seems a critical contradiction. Thus he continually extols the virtues of abstractness over concreteness, of contingent arrangements over those that are relatively fixed or determinate, and of "the class of all possibilities of performance." Yet he fixates on "complexity," his problem of problems, which apparent contradiction finesses the overpowering challenge of "all possibilities of performance." Having made that crucial value judgment, Luhmann otherwise urges that values are inefficient and ineffective devices for ordering complexity.

Seventh, reasonable readers also can entertain serious doubts about whether empirical analysis will be served by Luhmann's approach. For example, is his reliance on "abstractness" a matter of "evolutionary superiority," as Luhmann contends? Or does that convention simply distance analysts from the laborious selection of some manageable set of concepts-cum-operations that will permit valid and reliable descriptions of reality? Thus "trust" now gets measured in a score of major ways; moreover, "power" gets even more differentiated operational definition; and, understandably, research employing those concepts/operations generates diverse results. Luhmann acknowledges this crucial source of variance in results concerning his two conceptual targets in the volume being reviewed, but only to recommend neglect of that source of variance. Concerning the stuckness in research, he proposes (p. 108) that "to come to terms with its different meanings takes us no further—unless towards wariness and, in the end, resignation. In such circumstances it is not possible to proceed step by step; . . . instead, we must try to use more general concepts." Conceivably, at least, Luhmann may thus merely exacerbate a major problem as he seeks a way out of the inadequacies of structural functionalism.

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Karl Popper. By Anthony O'Hear. (Boston: Routledge and Kegan Paul, 1980. Pp. xii + 219. $25.00.)

Two problems of knowledge have puzzled philosophers for centuries. One, originating with Aristotle, is the infinite regress of justification. The other, elaborated by Hume, is justifying future events from events already experienced. Karl Popper's high standing in the philosophy of science is due to his innovative answers to the second, the problem of induction. His failure to resolve the first problem marks the limitations of his philosophy.

Anthony O'Hear provides an excellent critical guide to Popper's theories. Though finally rejecting Popper's basic system of thought, O'Hear judiciously discusses its defining ideas. We are able to see the lovely simplicity of Popper's solution to the problem of induction: replacing justificiation with refutation. From this replacement follows the view that a theory requiring more singular statements is better (more precise) because more falsifiable; and theories covering the same ground can be ranked according to their verisimilitude (truth content over falsity content). Science thus avoids justifying theories, even on the strongest of empirical regularities. The scientist is concerned only with falsifying claims on the future.

O'Hear discusses at length the well-known rivals and objections to Popper's falsificationist thesis. The first, developed by Lakatos, is that any theory's failure—even in a crucial test—can always be explained by additional hypotheses. Since there is no theoretical limit to the chain of saving hypotheses, no falsification can occur. Popper's attempt to answer this objection—the most serious for his philosophy of science—with a demarcation between critical and uncritical approaches to knowledge and an emphasis on background knowledge does not quite overcome Lakatos' critique. We are inclined to favor Lakatos' acceptance of a protective belt of statements in science that are insulated from falsification. But the discussion generated by these clashing views—on the metaphysical or dogmatic status of research programs, the problems of crucial experiments—still enrich the literature. O'Hear's treatment, though not comprehensive (more on Lakatos' alternatives would have been helpful), is accurate and helpful.

The second objection is that falsification cannot address probability statements, for no event can falsify a probability estimate. Here Popper is at his best and worst. The solution: statistical stability is the result of propensities in the circumstances in which events occur; and the deviation of events from a stipulated tolerance is counter-evidence for the estimate. O'Hear points out both the failures of this answer—its reduction to a frequency theory of probability—and Popper's misapplication of his reconstruction to the two-slit experiments in physics (though O'Hear's discussion does not include Popper's technical contributions here).

The third objection—the one most familiar to social scientists—is the restoration of relativism by Kuhn and others and the attendant claim that incommensurability makes choices among theories necessarily arbitrary. The easy dismissal of Kuhn's separate worlds (a dismissal needed even for Kuhn's study) should be required reading for social scientists. Unfortunately for Popper, any encounter with relativism raises the Aristotelian problem of infinite regress. Aristotle distinguished basic from demonstrative knowledge. The former is true without evidence or justification. Popper uses basic statements, arbitrary stops in a regress. O'Hear outlines the main arguments against Popper's consensual support for basic statements: the differences between appealing to a rule and headcounting, the more general denial (by Wittgenstein) of a private language, the non-evidentiary status of some statements (the use of Austin would have been helpful here). Generally, a sketch is provided of alternative philosophies that supplant Popper precisely on the failures of his skepticism.

Karl Popper is one of the important philosophers of this century. O'Hear's book captures both the successes and the failures of his work. The discussion of fallibilism in political thought is perhaps the weakest part of the book—but perhaps this is also Popper's weakest contribution. O'Hear's exposition of Popper's Darwinism, the three-world theory, Popper's intriguing use of the Tristran Shandy paradox, the Oedipus effect, and Gödel's theorem to deny complete description and, subsequently, prediction—are well done. O'Hear has written possibly the best examination to date of Popper's overall philosophy.

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Authority. By Richard Sennett. (New York: Alfred A. Knopf, 1980. Pp. 206. $10.00.)

The concept of authority is no doubt central to the works of political theorists. It has also been a concern of a variety of social scientists who, in recent years, have examined American government, political economy, ideology, and culture, and have written about America's crisis of authority and/or crisis of legitimacy. Most often, these analyses begin with Weber's assumption that authority and legitimacy are intertwined, that authority is legitimate power. Hence, a crisis of authority is at the same time a crisis of legitimacy, and vice versa. Richard Sennett, however, stands Weber on his head: his claim is that in modern society authority is *illegitimate* power. There is, he argues, a crisis of legitimacy in modern political and cultural life which maintains forms of authority which people believe are destructive.

The central insight in the book is that human beings create authority, images of strength and weakness, and its meanings. Sennett examines how we feel and experience authority and interpret it into being. He concludes that Americans lack a sense of their power to imagine new forms and meanings of public authority and that, as a result, authority tends to be understood as total, permanent, and external to the self. Prevailing forms of authority we perceive as deceitful and unworthy of our trust, even as we remain psychologically dependent on them. Our fear and rejection of authority actually tie us more closely to and reinforce the power of images of authority we regard as negative. Having lost "a sense of some humane value of power" (p. 82) and still having a need to believe, Americans feel attracted to strong figures of authority even as they doubt their legitimacy and the legitimacy of the institutions they seek to govern. Sennett's analysis suggests—but only suggests—how advanced societies can experience a crisis of legitimacy and, at the same time and without new efforts at repression, produce no significant movement for social and political change.

For Sennett, there are two dominant images of authority in modern society. Both are rooted in organizational life. First, the paternalistic authority of bureaucratic organizations, like that of nineteenth-century capitalists, is a false love which promises care, but delivers only when it is in the interests of the masters and when those who are dependent remain passively quiescent. If a deceitful paternalism has created a culture of disbelief in all forms of authority, then the image of personal autonomy as authority has further privatized modern culture. Operating on "the principles of indifference to others and self-sufficing expertise" (p. 131), the autonomous figure arouses feelings of shame and dependence and offers an illusory view of freedom to subordinates. Seeing autonomy as a way to be free under unequal conditions, subordinates seek to defend themselves against the world of power and to find freedom and self-fulfillment in private life. Sennett, following Tocqueville, claims that this privatization of experience impoverishes public life and opens up the possibility of vast powers for a state which is a soft tyranny. A public realm of freedom, the work of imagining authority as legitimate, has been eroded in modern society.

The problem, then, is not that there is a crisis of authority in American society, but rather that there isn't. Sennett proposes that people need to create frequent crises of authority, to link authority and disorder, so that they will come to have a sense of the limits of public power and the ability to make and remake its terms and create the conditions of their own freedom. In short, democratic forms of authority-making will enable people to limit and respect authority. While Sennett indicates how this might be done in private life and, to a lesser extent, in organizational life he leaves us with only scattered hints on how this can be accomplished in the polity.

For the most part, the guide is Hegel and the unhappy consciousness. Sennett probes the dialectical relationship between master and bondsman to show how people can imagine into existence new forms of power in private life which are not omnipotent, fearful, and mystifying. He suggests that if productive conflict can end psychological dependence on authority in private life, then a strategy of democratically deforming authority can achieve the same end in modern bureaucratic organizations. Subordinates must disrupt the universalism of paternalistic and autonomous authority and initiate an ongoing process in which authorities are clear about their actions and subjects continually negotiate over and decide about the meaning of power in their lives. Authority can be legitimized in democratic organizations where conflict is stuctured so that subordinates lose their shame and interpret and reinterpret the meaning of power—by locating the source of all orders and treating them as propositions to be tested, discussing the categories to which the rules apply, encouraging a variety of obedient responses to rules, role exchange which will suggest the limits of power, and face-to-face negotiation over the need for nurturance and support.

This is a wise and provocative book. Its analysis of the psychology of authority in modern society is excellent. What Sennett has not given us, however, is a full analysis of how and why his strategy of democratic deformation will work. He is most suggestive when discussing organizational life, although nowhere is it indicated why masters need or will respond favorably. Further, Sennett's strategy is only weakly tied to political life. There is an interesting passage about the need to break the link between the suffering and moral claims of victims and the reinforcement of the prevailing psychology of authority and power of managers. His discussion about the ways in which authorities can rightly be perceived as not having complete control is helpful. But there is little mention of the contemporary social and organizational conditions which apparently serve to reinforce a convenient conspiracy between masters and subordinates. Where is it written that the powerful will be responsive to democracy and that the subordinates will want their freedom? At some point in his truly impressive works, Sennett needs to analyze why this is politically—not just theoretically—possible.

CHARLES E. ELLISON

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Karl Marx and the Anarchists. By Paul Thomas. (Boston: Routledge & Kegan Paul, 1980. Pp. ix + 406. $40.00.)

The history of ideas can be done in a number of ways. One is to look at writings that hitherto have gone unnoticed. Another is to draw new (and perhaps surprising) connections between thinkers and theories. In his introduction Paul Thomas indicates that he will be doing both. The subject of his book is Marx's "oddly neglected" responses to his anarchist opponents—Stirner, Proudhon and Bakunin—a study of which, Thomas believes, will lead to a deeper understanding of both Marx's theory and the nature of politics. But in the course of exploring these neglected disputations, Thomas wants to establish a fairly striking thesis: whereas Marx is a descendant of Rousseau and Hegel, he maintains, the anarchists are the children of the *philosophes* of the Enlightenment. This revisionist genealogy is fundamental to Thomas' enterprise as it provides the basis for his central cleavage between Marx and the anarchists. While the latter pursue the logic of the Enlightenment's distrust of authority and politics to its limit, Marx sees revolutionary change as a creative and essentially political endeavor.

Thomas pursues the Hegel-Marx connection at great length, occasionally bringing in Rousseau as well. The core of this tradition is the distinction between state and civil society which, according to Thomas, Marx sees at the root of *political* alienation. In modern social orders, civil society is separated and isolated from the political, providing a distorted, "alien," political life. The opposition between public and private life, requiring as it does the splitting of personality into discontinuous roles, is thus "the measure of alien politics" (p. 121). Now, Thomas argues, Marx saw emancipation from such alienation to be in large part a political task, involving a transformation of both civil society and polity. But at this point a fundamental ambiguity enters into Thomas' account, for on the face of it this sort of "political" action would take place within the "bourgeois state" with an eye to its destruction. But Thomas adamantly insists that Marxian politics does not require a state, and that "the solution to alien politics cannot be no politics, but only politics of a different kind; it cannot be further depoliticalization, but only a repoliticalization of the required type" (p. 350). But one never gets a clear idea of the nature of this second sort of politics and its relation to revolutionary political action within the "bourgeois state." (Surely they must be different.) Admittedly, Marx may not have said much about "post-revolutionary politics," but if Thomas wants to argue that "political" is a critical category in Marx's thought—as he certainly does—then we need to get much clearer about the distinction between these two senses of "political."

In contrast to the Marxist genealogy, the anarchists' links to the Enlightenment pretty much drop out for the remainder of the book (except for a short reappearance in the conclusion). One can certainly understand why that family tree wouldn't be pursued too far: showing the communitarian, romantic, anti-scientific Bakunin to be any sort of descendant of the *philosophes* (and not Rousseau) isn't an inviting task. Moreover, of the few connections that do surface in the body of the book, Thomas' insistence that anarchists steadfastly maintain a "liberal," negative, conception of liberty is less than totally convincing, especially in light of Bakunin's claim that freedom has both negative and positive aspects (*Bakunin on Anarchy*, ed. Sam Dolgoff, Random, 1971, pp. 234-42). But though Thomas does not pursue this part of the genealogical thesis very wholeheartedly, he does constantly stress the anarchists' fixation upon the "political." Anarchists, we are told, detest the political realm because they are so obsessed with it. In contrast to Marx, their diagnosis is not premised on the alienating bifurcation of man into social and political selves, but the mere existence of authority, politics and the state. And thus they are said to both overemphasize and underemphasize the political. They overestimate what the state's destruction alone can bring about, while they underestimate (indeed are blind to) the value of liberal-democratic reforms as first steps to "human emancipation" (pp. 239-42).

It isn't really surprising that Thomas develops the Marx side of the genealogical thesis so much more thoroughly than the anarchist side, for this book is very much about Marx and his disputes with the anarchists. And because Marx did not dispute with Kropotkin, perhaps the greatest anarchist theoretician, the relation between Marxism and anarchism remains unclear. Thomas' insistence that means cannot be separated from ends (p. 342) notwithstanding, it certainly seems that anarchists like Bakunin and Kropotkin shared Marx's central idea of an integration and intensification of individuality and community (cf. pp. 9, 52). If so, then although Thomas has demonstrated fundamental differences in the two diagnoses from which their "tactical" divergence stems, the rift dividing them is still not quite so deep as Thomas would have us believe.

GERALD F. GAUS

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A Discourse on Property: John Locke and His Adversaries. By James Tully. (New York: Cambridge University Press, 1980. Pp. xiv + 194. $22.50.)

The "major aim" of *A Discourse on Property* "is to recover the meaning which John Locke intended to convey in his theory of property in the *Two Treatises of Government.*" As a student of Skinner and Dunn and the "new history" of political theory, James Tully argues that the means for accomplishing this objective involve, on the one hand, an exploration of the "normative vocabulary and conventions [of] seventeenth-century natural law and natural rights 'discourse' to which Locke is a contributor," and, on the other, a consideration of the social issues and political action to which Locke addresses himself in the *Two Treatises* (p. ix). *A Discourse* achieves a notable success in fulfilling these promises, especially with respect to the first set of issues.

This is a thoughtful, carefully reasoned, and tightly argued little book. Indeed, some readers may find the author's painstaking examination of the shifts and turns in natural law reasoning a bit overdone; nevertheless, there can be no doubt that it is both a serious reading of Locke's argument and an admirable contribution to the "new history" of political theory. I emphasize this point here because the real danger with respect to Tully's *Discourse* is that the intricacy of its arguments is likely to obscure the importance of its radical conclusions, and it is upon the latter that I therefore propose to focus my attention in this review.

Tully begins by establishing the basic framework for viewing Locke's treatment of property in the *Two Treatises*, which he suggests lies in a "workmanship model" derived from the theory of creationism. Essentially, this means that the maker of something has a right over and knowledge of that which he makes. Although this may sound a bit odd at first, it will easily be seen how it can account for man's ontological status as "the workmanship" of the Creator; his theological relationship to God his maker, as well as for the epistemological constraints Locke imposes upon men with respect to their knowledge of "real substances" which they have not made. That "the workmanship model is a fundamental feature of all Locke's writing," and constitutes "the ground of property relations as well as of many political relations" is convincingly demonstrated by Tully (p. 4).

In addition to placing Locke's thought within a traditional theological perspective, Tully argues that "Locke never considers a congeries of presocial and isolated individuals." On the contrary, "Locke's analysis always presupposes men organized into a unified community," as an "irreducible datum of man's existence" (p. 49). From the standpoint of historical research, the atomistic interpretation of Locke has never had much credibility, being an anachronistic application of a later view to seventeenth-century thinkers. By examining in detail the issues in the contemporary debate over natural law, Tully is able to show why Locke so frequently allied himself with the traditional views of Aquinas and Hooker. It is only when these Aristotelian dimensions of Locke's thought—his insistence upon beginning with individuals in families and organized communities under natural-law obligations to each other and to God—that his truly "radical intentions" (p. 89), as Tully calls them, can be appreciated.

"Locke's main ideological conclusion," he argues, is "that fixed property in land does not have a natural foundation" (pp. 123, 146). Whatever land an individual "enclosed and . . . tilled and reaped" in the state of nature, Locke wrote, became by "his peculiar right" his property. "But if either the grass of his enclosure rotted on the ground or the fruit of his planting perished without gathering . . . this part of the Earth, *notwithstanding his enclosure*, was still to be looked on as waste, and might be the possession of any other" (*Second Treatise*, par. 38). Tully develops the implications of Locke's account of property to show that an individual has "no right in land as such, but only a use right in improved land conditional upon the use of its products" (pp. 122-23). Locke's model of property ownership is the English Commons (pp. 124, 129, 135). "The only form of property in land which he endorses in the *Two Treatises* is the English Common." Moreover, Locke mounts a radical attack on primogeniture, and his "undermining of primogeniture clearly would have the effect of redistributing landed property into much smaller estates" (p. 169). In ideological terms, therefore, "Locke's theory serves explicitly to legitimate the rights of the commoners against the enclosing landlords" (p. 154).

Through a consideration of the interrelationships between the Lockean notions of "person," "liberty," and "property," Tully integrates the *Two Treatises'* discussion of property into a political theory in which "revolution against arbitrary government is legitimated" (pp. 130, 146). This aspect of the author's argument is less well developed in *A Discourse*. But there are indications of the direction in which it could be pursued. Tully notes Locke's assumption that laborers and tradesmen own their own tools (p. 140). Against Macpherson's conception of the bourgeoisie, he intimates that an "economic organization of skilled workmen" may be closer to the socioeconomic paradigm of Lockean thought (pp. 136, 143). In other words, the social audience for whom the *Two Treatises* was written was largely composed of artisans, tradesmen, independent yeomen, and small gentry. Hence the radicalism of Locke's political message.

We ought, therefore, to pay greater attention to the political and intellectual linkage between Locke's political theory and the position developed by the Levellers a generation earlier. Locke's treatment of property, according to Tully, incorporates most of the points at issue in the army debates at Putney between Ireton and Colonel Rainborough, with Locke siding with the latter (pp. 169, 174). In fact, Tully affirms Locke's belief in adult male suffrage, although the evidence for this is extremely thin (p. 173). Tully may be right, but he has a tendency to wander from the "historical" pathways of his argument, claiming, for example, that "it cannot be seriously doubted" that Locke read Aquinas, Suarez, or Cumberland (pp. 6, 65-66), when there is no hard evidence to support any of these claims. Certainly, every interpretation must have some inferences, but their overuse will soon exhaust the author's credit with the reader, leaving no basis for distinguishing between "recovering" a theorist's intentions and an interpreter's arbitrary assignment of intentions to him.

Nevertheless, *A Discourse on Property* presents a serious case for revising our thinking about Locke's attitude towards property, and the role played by his discussion of that institution in the context of the political argument of the *Two Treatises.*

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American Politics

Before the Convention: Strategies and Choices in Presidential Nomination Campaigns. By John H. Aldrich. (Chicago: University of Chicago Press, 1980. Pp. xiv + 257. $25.00, cloth; $7.95, paper.)

Before the Convention is a self-consciously theoretical treatment of the American presidential nominating process, based largely on an analysis of the Republican and Democratic contests of 1976. It represents a significant advance in the theoretical level of the study of the nominating process, and it further represents a desirable trend in which some of the profession's most sophisticated formal modelers confront data and do theoretircally informed empirical work.

The intellectual structure of the book comes from rational choice theory, based on the assumptions that candidates are goal-oriented expected utility maximizers, and that voter behavior can be explained in similar terms. There are two main sections of the book. Chapters 2 to 4 review the familiar background topics of who runs for president and why, the institutional context, and citizen participation and choice. While the topics are familiar, Aldrich sheds new light on them. The most original parts of the book, however, are in chapters 5 to 7, where Aldrich deals with the strategic problems of resource allocation and ensuing campaign dynamics, the choice of states in which candidates compete, and competition in policy space.

Among the strengths of the book are an operationalization of how willing political candidates are to take risks, and an enlightening discussion of the tradeoff between choosing candidates who are most preferred and candidates who are electable. Aldrich also develops an innovative and useful measure of candidates' competitive standing, based on the proportion of delegates won relative to the number yet to be selected. He develops and tests propositions regarding candidate choices of states in which to compete, and analyzes candidates' policy positions and issue emphases.

The most important part of the book is probably that dealing with the dynamics of campaigns. Performance in campaigns is based on resources available to candidates and measured against expectations. Performance in excess of expectations generates new resources and new expectations and vice versa. Aldrich works out the logic of these dynamics for two-candidate and multi-candidate contests. The mathematical basis is developed formally in an article in the September 1980 issue of this *Review.* The substantive treatment of the dynamics revolves around the question of stability.

Aldrich's basic prediction is instability in both two- and multi-candidate races, where instability is defined to include oscillation about a point of equilibrium in two-candidate races, and to include an inexorable rise to victory along with winnowing out opponents in multi-candidate races. The situation in which an initial front-runner continues in the lead and wins the nomination is clearly ruled out.

Thus the Aldrich instability theme is diametrically at odds with the Keech-Matthews theme in *The Party's Choice* (Brookings, 1976), that early front-runners often exist, and when they do, they usually win. Of course, that observation was wrong for the 1972 out-party nomination and irrelevant (in the absence of a clear front-runner) for the 1976 out-party case. Ironically, in the year in which Aldrich's much more theoretically sophisticated book appears predicting instability, early front-runners won nominations of both major parties.

Of course a changing context can help explain changes in the pattern of outcomes, and here the Aldrich book is again superior. While *The Party's Choice* underplayed the significance of recent reforms and rules changes, emphasizing continuities, Aldrich rightly points out that these new developments have strengthened the dynamic forces and the forces of instability in current campaigns. Yet the brief section relating the findings to previous campaigns is one of the least satisfying in the book.

While Aldrich's book has a strong theoretical base, its readability and its use to a broad audience is not hampered by its theoretical character. In fact, the book is strong on descriptive material regarding the 1976 campaigns. And the presentation of this material in an analytical context makes it a rather painless way to learn some rational choice theory and to see its relevance to a very concrete problem. This is an important book about presidential nominating politics, representing only a portion of Aldrich's contributions to our understanding of this subject. Its utility extends from the specialist to the general reader and the undergraduate.

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The Reserve Mining Controversy: Science, Technology, and Environmental Quality. By Robert V. Bartlett. (Bloomington: Indiana University Press, 1980. Pp. xi + 293. $17.50.)

Scholars of public opinion consistently note that the American public can become politically motivated as a result of certain societal events. Such infrequent public interest has consequences for the policy process. Robert V. Bartlett describes and analyzes the nature of such consequences for the policy process resulting from a widespread change in social values. More specifically, Bartlett focuses on the controversy surrounding the Reserve Mining Company of Silver Bay, Minnesota, a controversy that well illustrates the policy consequences of a major public concern for the environment.

This study, however, goes beyond a description of specific conditions surrounding charges that Reserve Mining Company's policy of dumping millions of tons of mining wastes into Lake Superior resulted in aesthetic as well as health damages to the surrounding area. A broader question is addressed: "Is it possible then, for a society to obtain simultaneously the benefits of industrial technology and of a high quality national environment?" (p. 234). Although this question is not, and cannot be, answered at this time, Bartlett does provide more evidence of the conflictual nature of a policy process attempting to reconcile two interrelated, yet often contradictory, forces: industrial technological advances and attempts to protect the environment. The study shows that prior to this time of national concern for the environment, development of industrial technologies represented a major goal of society. What to do with the external effects of such development remained a secondary consideration. It therefore came as a major surprise to industry when the public demanded that the environment be cleaned up, and government responded to those demands.

The Reserve Mining Controversy vividly illustrates various characteristics associated with policy conflicts resulting from environmental concerns. In the first place, whether an industry pollutes the air or the water, thereby affecting those living in the surrounding area, requires scientific evidence that may or may not be available at that time. Yet when the public generates enough pressure, policy makers tend to react as a way of placating public demands. The ultimate decision, however frustrating it may be to those presenting scientific evidence, will be political in nature. In short, a course of action for a complex, technical issue will be largely based on subjective definitions of what that issue is and what should be done about it. In fact, Reserve Mining Company was ultimately forced to invest millions of dollars for an on-land dumping site even though no scientific evidence existed to prove that the health of citizens in the Lake Superior area was in immediate danger.

Second, this controversy nicely illustrates the federal nature of policy making in this country. A multitude of actors and institutions at all levels of government interacted continuously over the 12-year period to develop a plan of action. At a minimum, three states, several communities within these states, as well as the federal government, became involved in this controversy, not to mention the various private groups representing industry and environmental positions. Allowing so many access points to individuals and groups perceived to be affected by industry's actions leads to lengthy negotiation and efforts to find compromises. It is no wonder that it took 12 years to develop an acceptable program.

Third, Bartlett's study points to the changing nature of problem definition and the impact of such problem definition on policy deliberations. More specifically, the event that significantly altered policy debate in this case occurred with "aesthetic" considerations were replaced by "health" considerations. As soon as the possible presence of asbestos was linked to waste dumpings, and the possibility arose that the drinking water in the area was contaminated, the policy context changed dramatically. Such a redefinition led to public concerns that had not existed before, since more people were now perceiving this problem as affecting them directly. This pressure, in turn, forced policy makers to be firm with the Reserve Mining Company. It is unlikely that the federal courts would have required on-land disposal sites on purely "aesthetic" grounds. Only after health considerations became the center of controversy did it become clear to all parties involved that drastic action, with or without conclusive scientific evidence, would be required.

Bartlett presents a very detailed, well-researched case study, one that certainly adds to our disciplinary knowledge. Policy-related courses would benefit from this research as a supplementary source of information and analysis.

DIETER MATTHES

Francis Marion College

The Army Gets an Air Force: Tactics of Insurgent Bureaucratic Politics. By Frederick A. Bergerson. (Baltimore: Johns Hopkins University Press, 1980. Pp. xii + 216. $14.00.)

Foreign scholars are occasionally perplexed or amused by the attention given to bureaucratic politics by American political scientists. But it is clear that the practice and study of this phenomenon have been nourished by our distinctive social structure and ideological tradition. American egalitarianism encourages governmental processes which are open enough to enable outsiders to describe them in detail. At the same time, it develops enough entrepreneurial initiative in public officials to guarantee that the detail will be delicious. A direct line runs from Lincoln Steffens through early exponents of the case method like E. Pendleton Herring and Harold Stein to contemporary practitioners of the bureaucratic politics approach.

Frederick Bergerson's book, which won a Leonard D. White prize, is a high-quality, thoroughly researched example of what that approach can accomplish. His topic is the revival of an army air capability that had been all but extinguished after the creation of an independent air force. He attributes this rebirth to "bureaucratic insurgency" a species of "social movement" that develops within large-scale organizations.

In the struggle for an army air capability, the insurgents were a loose fraternity of middle-ranking officers and technicians held together by a common conviction that victory in combat was in jeopardy because the air force placed too low a priority on close air support of land operations.

Bergerson's time frame runs from 1941 to 1970. Initially army aviators were used only for artillery targeting and unit liaison, later for medical evacuation and the transport of cargo and infantry, still later to operate gun platforms. But the victorious insurgents then developed a fixation on technology akin to that of their air force rivals, and overreached by requesting the Cheyenne. This high-speed, high-cost assault helicopter was rejected by higher authority as needlessly sophisticated for most Third-World environments while insufficiently lethal or survivable for NATO.

Yet the insurgents had come a long way. Why? Bergerson supplies several reasons. One was technological: the advent of the helicopter. A second was the acumen of the insurgents. As specialized "fly boys" unlikely to make general, they sought allies among credentialed generalists destined for high command. A third was the strategy of the opposition. The air force sought to limit army aviation to logistics and communications, to confine it to a 60-100 mile battlefield zone, to restrict the weight of fixed-wing planes to 5,000 pounds, etc. At the same time, it neglected its own responsibility for close combat support because its leaders felt that their organization's "essence," to use Morton Halperin's term, lay in strategic bombing, that is, in ball-carrying, not in blocking or tackling for others. Moreover, the air force ignored the army's challenge too long. Bureaucratic insurgencies, Bergerson claims, are most easily put down at the outset.

So much for the tale itself. Of more widespread interest is Bergerson's concept of insurgency. While the term has a harsher connotation in common speech, he uses it to denote a rather moderate reaction in a spectrum of possible responses (vocal protest, slowdown, resignation, insubordination, mutiny) by individuals who disagree with organizational policy. The insurgent is an activist reformer, but not an outlaw. He remains loyal to the organization and its mission. Indeed, he feels that new policy is required for the good of the organization and the attainment of its mission. But he is not set on storming heaven. Like a missionary, he works gradually but persistently to convert the heathen. At the same time he takes advantage of loopholes, skirts the edge of regulations, evades formal orders, and is less than fully compliant when he cannot ignore them. Bergerson is aware of the normative implications of even such modest finagling. The problem is that challenges to hierarchical authority are also challenges to civilian control and thus to democracy itself. Against this, however, he sets the danger of an unchallenged, hence stolid, plodding, ponderous bureaucracy.

Since all of this is unexceptionable, let me add a few comments on the empirical side. One is that the degree of pluralism evident in American aviation—commercial as well as military—is sustained not only by the underlying social forces noted earlier but also by the wealth of the nation. No other country can afford four air forces. Pluralism is also more likely in the absence of peril. Conversely, central planning and control of aviation are more likely when the state itself is in jeopardy. Finally, just as technology made army aviation possible, so it may make it obsolete. If so, Bergerson's insurgents will prove to be transitory historical figures, though insurgencies of other kinds will recur.

LAURENCE I. RADWAY

Dartmouth College

Representative Democracy: Public Policy and Midwestern Legislatures in the Late Nineteenth Century. By Ballard C. Campbell. (Cambridge: Harvard University Press, 1980. Pp. xi + 260. $20.00.)

In recent years political scientists interested in Congress have benefited from a number of studies of nineteenth- and early twentieth-century congresses that have provided valuable historical perspective. Now students of state legislatures (and particularly those interested in voting patterns in the legislature) can benefit from a study of state legislatures in the late nineteenth century.

This book by a historian examines the lower house of legislatures in Illinois, Iowa, and Wisconsin for the period 1886-1895, a period of socioeconomic change, considerable political ferment, and higher levels of party competition than in the past. Ballard Campbell combines the tools of historical analysis with those techniques used by political scientists—particularly roll-call analysis. The study is heavily dependent on roll-call analysis, which, while relatively simple, is adequate for the author's substantive analysis, which focuses on the importance of political party alignments and on major areas of public policy.

A few of the substantive findings can be summarized. The Democratic and Republican legislative delegations in each of the three states wre distinguished primarily by the ethnic and religious composition of the constituencies (and not by their urban or rural character). The individual legislators for the most part shared the dominant characteristics of the district. (Irish Catholic districts then, as now, produced Irish Catholic legislators.) Political party alignments appeared on a limited number of issues, but when party was not important, no other variables (such as urban-rural differences) proved to be consistently useful in explaining voting patterns.

The issues that created the strongest, and most frequent partisan divisions were those labeled "community mores" by the author. The most frequent of these was the regulation or prohibition of liquor; other issues involved educational policy, regulation of social behavior, individual rights, and nationalism. These were the issues on which the religious and ethnic groups that dominated the two political parties had sharply different views. Certain governmental issues such as the political structure or elections, in which the parties had a vested interest, also produced party voting—as would be expected. Most economic issues, however, did not produce strong, consistent party alignments.

The findings of this study are remarkably similar to modern analyses of voting in state legislatures conducted by political scientists, although Campbell pays little attention to the comparison. Then as now there were partisan voting alignments on those issues that were most salient to, and that distinguished, the groups and interests that regularly voted Democratic and those that voted Republican. Then as now the maverick legislators were ones who represented districts that were untypical for that party—for example, the Yankee Protestant district that elected a Democratic legislator. The difference between the voting patterns of the late nineteenth century and those that are found today is that the constituent bases of the two parties were ethnic and religious and the issues that divided Democrats from Republicans were social. Today the constituent bases and the partisan issues involve economic interests and questions, as a consequence of the New Deal legacy. In fact, the social issues that united the parties of the 1880s and 1980s are somewhat comparable to the social issues that divide the parties today.

Although the study of voting alignments and the role of party may be the most pertinent aspects of this study for political scientists, there are also valuable accounts of the issues that captured the attention of legislatures, the background and recruitment of legislators, and the operation of the "amateur legislature," most of whose members were freshmen. Campbell makes a convincing case that state legislatures deserve more attention for historians seeking to understand political developments in the late nineteenth century. Such studies also offer valuable perspective to those trying to understand state legislatures today.

MALCOLM E. JEWELL

University of Kentucky

The Economic Crisis and American Society. By Manuel Castells. (Princeton, N.J.: Princeton University Press, 1980. Pp. xiv + 285. $20.00, cloth; $7.50, paper.)

In this exceedingly ambitious, stimulating and frustrating work, Manuel Castells attempts to begin to explain the economic crisis that has shaken advanced capitalism for roughly a decade by trying to explain the crisis for the core case of the United States. He characterizes the crisis not merely as one of stubborn stagflation, but, more fundamentally, as a structural crisis in which reproduction of the political economic system requires basic structural reform or transformation, "as multidimensional, political and ideological, as well as economic" (p. 14). He seeks not only to explain the crisis' origins and trajectory, but also to identify the major coalitional and strategic options that might effect its resolution.

Most works eschew highly complex *explanada*, or pursue them by means of rather blatant simplifications. They engage in narrative, analytic or theoretical prolegomena to actual explanation, or they argue the power of simple theses. Castells boldly pursues the complex explanatory ideals of a broadly "structuralist" neo-Marxism. As regards the economic crisis, this entails two major tasks: (1) articulation of an *abstract*, multidimensional, dynamic theory of crises in advanced capitalism; and (2) *specification* of the abstract theory to the concrete instance of contemporary U.S. capitalism—in a manner that elaborates the theory in light of the idiosyncracies of the case, before adjoining it to the facts of the case in the task of explanation. It also entails a primary reliance upon such key Marxist concepts as exploitation, class struggle and uneven development.

Castells' abstract theory of crises emphasizes factors that tend to dampen or buoy profit rates. Of course, for a theory of crises, dampening factors are particularly important. Castells emphasizes the dampening effects of increases in the "organic composition of capital" (i.e., the value of physical capital relative to that of labor inputs)—despite considerable equivocation. His logical and empirical grounds for this theoretical choice do not overcome the serious objections to an "organic composition" theory of declining profits that had been raised by Joan Robinson, Paul Sweezy, N. Oshikio and others before Castells' writing and have been compounded by Thomas E. Weisskopf, John Roemer and others since. As a result, Castells' major abstract thrust emerges rather lame. Abbreviated discussions of other more promising profit-decreasing factors do little to overcome this misplaced, or, at best, misargued, emphasis. Neither do excellent delineations of such profit-buoying "counter-tendencies" as sundry state-facilitated expansions of demand, since Castells must explain *declining* profits.

The *specification* of the abstract theory to the U.S. is enhanced by the extent of accompanying theoretical elaborations. Perhaps the most interesting elaborations are those arguing that state and corporate effects to stimulate demand have tended in the "long run" (of Keynes' perhaps nervous joking) to lower productivity, stimulate inflation, weaken comparative advantage and otherwise reduce profits. Also noteworthy are Castells' discussions of the class character of corporate authority, the dependence of "post-industrial" tertiary expansions upon industrially mandated state expansions, the fundamental conditioning of state economic interventions by heterogeneous industrial and market structures, and many other antidotes to the conventional wisdom. Unfortunately, these riches may be too tarnished by a mode of presentation which abounds in logical ellipses, facile uses of evidence, and easy convictions to appeal to those not persuaded of the force of Castells' approach and formulations from the start. These riches' failure to come together in the intricate and compelling final design anticipated by Castells' introduction will leave as many readers (of all persuasions) feeling as disoriented as enlightened.

Nevertheless, instructors in courses on political economy, neo-Marxist theory and the ongoing economic crisis will want to consider using this book as a major text, despite its unevenness and shortcomings. Scholars pursuing its *explanada* will want to mine this book for insights. Those erudite and enterprising enough to approach explanation of the economic crisis without assuming away its complexity have much to learn from coming to terms with this major precedent in their course.

ALEXANDER HICKS

Northwestern University

Dean Rusk. By Warren I. Cohen. (Totowa, N.J.: Cooper Square Publishers, 1980. Pp. xii + 375. $22.50.)

Twelve years after the Johnson administration we have the first serious study of Dean Rusk's impact upon our foreign policy in the latest (Vol. 19) of the lengthy series upon American secretaries of state and their diplomacy. So ramified have influences on policy become that the secretary of state frequently can seem one of peer officials and consultants projecting views upon the president. Personality then can prove more important than formal position. To describe the secretary's role may involve fitting random pieces from a piled jigsaw puzzle. A further complication may be source availability. Secretary Kissinger's precedent reserved the substantive files of his incumbency for a durable period for his own use.

To examine Secretary Rusk's place under Presidents Kennedy and Johnson, Warren Cohen lightens his load. In nearly 100 pages he explores Dean Rusk's earlier significant career, his mindset through events of his time, his disciplined conceptions of service, and his style of working with associates and superiors. This proves an excellent entry into the portrayal of his eight years as secretary. The perspective is of a good soldier who gives recommendations and who, even if he loses, loyally supports the decisions.

Military staff service and Department of State positions under Marshall and Acheson shaped his practices. Marshall, more his model, sought major sides to a question, heard the views of all contributors, held his cards close, and then privately, as he deemed his duty, expressed his opinions to the president. This explains accounts of presidential meetings with advisers which frequently depict Rusk as probing those present rather than openly expressing his opinions.

Rusk saw the tragedy of World War II as stemming from failure to face down ever-bolder aggression. Confronted by the faceted menace of communism, the United States had to show friend and foe that it would stand by even the vaguest commitments and steadfastly oppose this aggression in whatever form as in the national interest. It became a trial of wills.

President Kennedy and Rusk had an initial shakedown cruise. There was the contrast of impetuosity and action with thoughtful realization of limits. Accounts of people like Robert Kennedy include jabs at Ruskian caution, though Cohen finds that the president appeared satisfied with their relationship. But he did not press Rusk to stay when the latter stated his intention to leave after the 1964 election. With Johnson there were such excellent relations that at his term's close he wrote that Rusk "has served me most intelligently, faithfully and nobly" (p. 316). In fact, Rusk was the last of the 1960 top policy advisers.

A working concept for Rusk was his "little threads," meaning that by varied agreements all countries, including the communists, should acquire interdependencies to maintain peace. As a strong advocate of the United Nations, Rusk tried to use it wherever feasible. Differentiating him from his Europe-oriented deputy, Ball, was a reach of strong interest as an American liberal toward the Third World, Eastern Asia, and their problems.

Much has been written of the dangers of nuclear war from the Cuban missile crisis. Little of the timely crucial information from America's reliable Kremlin spy Penkovsky, radically reducing that prospect by reporting the Soviets had a crushing nuclear inferiority. Accordingly, Cohen asserts that the crisis was perhaps avoidable, and dryly declares that "the only clear and imminent danger . . . was [to] the political future of the Kennedy Administration" (p. 160).

Vietnam, the overwhelming issue, is traced from early American anti-communist support through the tragedy of full involvement. Amid the hawks around Kennedy, Cohen shows Rusk as most reluctant to resort to the military weapon and invariably seeking to limit it. He rightly believed South Vietnam needed basic civil institutions, or any military shoring would be fruitless. A deep service loyalty carried him in the Kennedy years. Johnson then used his tremendous persuasive powers, Rusk signed on for the duration, and Vietnam became a test of his principles. While others left the ship, he sought ways within his grasp to aid American interests and his president. He had given his word—despite some courses of which he was not aware and men's purposes not always his. A political liberal, Cohen strongly faults him for this, while conceding that he was a better man than his presidents. Here is the insoluble dilemma: moral man acting in a morally deficient political world.

Besides documents, Cohen has happily relied heavily upon interviews with well-placed people, including two with Dean Rusk. Surprisingly, a culminating chapter depicting how Johnson changed his Vietnam course has no reference to a Rusk interview or to his comments upon this decisive period, while apparently citing him through others. We have as yet no documented references to the all-important Tuesday luncheon meetings of Johnson with his core advisers. Only now Secretary Dulles is being reevaluated from recently declassified Eisenhower library material. Rusk has written no memoirs. Thus, while the present important, varied sourcebook provides valuable personality and factual insights, we must still await the definitive work.

ROY M. MELBOURNE

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National Health Insurance: Conflicting Goals and Policy Choices. Edited by Judith Feder, John Holahan, and Theodore Marmor. (Washington, D.C.: Urban Institute, 1980. Pp. xxi + 721. $25.00, cloth; $12.50, paper.)

For many of us in public health, the question of national health insurance has become such a continuing item of unfinished business that the thought or prospect of still another volume devoted to this question seemed not only unnecessary, but even redundant. Almost everyone associated with the field has provided analyses of not only why there is no national health insurance program in place, but also why the current proposals are inadequate, however needs are defined or perceived. Thus it is refreshing to examine a book offering a different perspective and avoiding many of the pitfalls to which other attempts to illuminate some of the more vexing issues in this arena have fallen prey.

My generally positive feelings about this volume derive primarily from its focus, which parallels a strong bias of mine. It has long been my judgment that the critical dimensions in the success of any program—be it health insurance or highway construction—are those relating to management and administration. Good policy arises as much from good choices as from how that policy is implemented and how it ultimately affects the citizenry. In their introduction, the editors clearly suggest that one, if not the principal, concern of this work are those aspects of policy choices which deal with how any given policy concerning national health insurance will, in practice, meet the goals set forth. To a large degree, they have succeeded in keeping the several essays in this volume directed towards that central issue.

Second, the essays avoid many of the ideological arguments which so frequently clutter debates about national health insurance. While the salience of ideology is clearly recognized, most of the essays give primary emphasis to what can be accomplished under various schemes, given the existing governmental, corporate and professional systems currently involved in providing medical care. It is contended, and correctly so, that many of the issues of implementation are common to all of the present proposals for a national health insurance program, and that how these questions of execution are met will have a greater impact on whether care is actually received by those in need, than the degree to which a plan may conform to one or another of the political or professional prejudices which abound in this area.

This volume differs from others in arguing that existing state and local governments should be involved in implementing a national health insurance program. This posture is in stark contrast to most discussions in this area which tend to be heavily centralist in their tone. The opening chapter, "Administrative Choices," by Feder and Holahan offers a first-rate analysis of the various institutional and organizational arrangements for providing medical care and an assessment of these for any prospective national health insurance endeavor. By avoiding the assumption which has affected so many policy analysts that wisdom and goodness primarily resides in Washington, the authors point to clear examples of where and how states and localities can participate effectively in the management and financing of a national health insurance program.

For political scientists and others familiar with the substantive questions involved in national health insurance, the book provides little new information. What it does do is to take such matters as the breadth and level of eligibility, scope of benefits, payment and regulation of providers, assurance of quality, utilization and the experiences we have had with the various means for dealing with these issues as a basis for analyzing several different national health insurance proposals. This tack places such questions in a different perspective and leads one to look at what specific policy choices may mean in the context of given resources, organizational arrangements, professional needs and related considerations.

The concluding section summarizes the several essays and, at the very end, projects some policy recommendations. These, at least in my judgment, deserve more direct attention than they have been given. An example is the statement that broader access to health care and cost containment can only coexist if government asserts its responsibility for allocating resources. This, in turn, requires formal mechanisms for deciding what we as a society want to spend on medical care. Such a strategy contrasts directly with present policy in which levels of expenditure are largely determined by providers who negotiate acceptable levels of reimbursement for specific items of cost. A further exploration of the implications of this change in policy which reverses roles of the public and professional sector would be most illuminating, not just for the discrete elements of a national health insurance program, but in an overall sense.

This volume is further enhanced by a good bibliography, and an appendix in which the features of eight of the most recent proposals for national health insurance are outlined in tabular form. As both an introduction to this complex field and as an antidote to some of the rhetoric which surrounds the debate in this area, *National Health Insurance: Conflicting Goals and Policy Choices* is worth having on one's shelf.

JOHN H. ROMANI

University of Michigan

The New Senate: Liberal Influence on a Conservative Institution, 1959-1972. By Michael Foley. (New Haven, Conn.: Yale University Press, 1980. Pp. x + 342. $17.95.)

In *The New Senate: Liberal Influence on a Conservative Institution, 1959-1972,* Michael Foley deals with an important topic and touches on a number of important themes. Foley takes as a point of departure the descriptions and analyses of the Senate of the 1950s by William S. White (*Citadel: The Story of the U.S. Senate,* 1956) and Donald R. Matthews (*U.S. Senators and Their World,* 1960): a conservative institution with structures and norms which supported the status quo and where influential and effective senators were those who conformed most closely to the norms; liberals tended toward nonconformity and frustration, and were generally unable to produce liberal policy outputs. Foley focuses on the activities, behavior, and policy successes of Senate liberals in order to "examine the questions of change and to assess how much, if any, of White and Matthews' classic model remains intact after the developments of the 1960s" (p. 14) and, in the final chapter, to "attempt to assess not only the degree to which the liberals changed the Senate, but the extent to which the Senate changed them" (p. 15).

Foley's conclusions are not surprising—an identifiable group of liberals was active in the Senate during the period studied, and changes in norms, structure, and legislative output occurred. More specifically, (1) the number of liberals in the Senate varied, reaching a peak in the first Johnson Congress (1965-1966); (2) liberals are not all alike: the size of the liberal group varies from issue to issue (civil rights had the most support and civil liberties and congressional reform the least); (3) liberals became more effective as their numbers increased and they began to occupy positions of power; and (4) the external political environment, especially presidential positions and citizens' attitudes on issues, are important factors in group effectiveness.

The chapter which deals with American liberalism and Senate liberals is the strongest and most successful of the book. Foley recognizes the problems of conceptualization and specificity in the designation "liberal." His solution is ingenious and sophisticated. Using the public record—e.g., newspapers, journals, Senate documents—he identifies concerns which were perceived as dividing the polity and the Senate along liberal-conservative lines. Then, using individual roll-call votes and Guttman scaling, he identifies liberal-conservative issues in the Senate and liberal senators. For each Congress he constructs three to seven scales (civil rights, education, foreign aid, etc.); a senator's position on a scale results in a score. Foley derives a composite index for each senator based on the proportion of "liberal" scores. He then classifies each senator on a continuum from liberal to conservative. (These data are displayed in some detail in the appendices.) Foley's decision rules are clearly stated and the data carefully interpreted. As he notes, his techniques of measurement are not without problem: for example, all liberal issues are treated as of equal importance. Nevertheless, the approach is useful. The results of the operational definition fit the real world (it is not just a skillful technique), and the measures take into account the multidimensionality of liberalism, overcoming a major problem of impressionistic data and single-index measures.

Chapters on Senate folkways, the liberals as a group, and the liberals as a legislative force are less successful. Foley asks the right questions: have norms and folkways changed; are liberals a group, with ongoing communications networks, formal structure, and cohesiveness; and how have liberals affected policy outcomes? The conclusions, however, are based primarily on illustrative evidence from case studies and interviews, and it is difficult for the reader to judge the extent of change or the validity of the findings. The analysis would be richer and more persuasive if Foley used his extensive interview data more systematically.

Throughout the study Foley deals with a number of important themes: the relationship between the political system and a political institution; how organizations, and in particular legislative institutions, change; the interaction between an institution and its members. There are also interesting questions about ideological groupings in legislatures: for example, whether informal and changing coalitions are truly groups, and the effect of formal structure on policy outputs. The discussion, drawing on organization theory, is often perceptive although the treatment is uneven with little systematic analysis.

The strengths of this study lie in the questions raised, in the definition and measurement of liberalism, and in going beyond roll-call analysis to explore the operation and effect of ideological groups in Congress. But readers will also find the study frustrating because the questions raised are not fully explored and there is too little systematic evidence to support many of the conclusions.

SUSAN WEBB HAMMOND

American University

Jimmy Carter: In Search of the Great White House. By Betty Glad. (New York: W.W. Norton, 1980. Pp. 546. $19.95.)

The official excuses for Jimmy Carter's failure to win reelection have already been articulated by the president and his White House aides. No president can govern, they say, in the face of fragmented political parties, single-issue interest groups, a democratized Congress, and unprecedented economic problems. The president, they add, did not worry about "politics" but faced the hard questions about the Panama Canal, the energy crisis and human rights around the world. Anyone who does this, they argue, will wear out his welcome.

This defense leaves no place for skill by a president at winning political support for his policies. It is fashionable among political scientists today to argue that the importance of the skill factor has been overrated by presidential scholars. The Carter presidency is an interesting test case of the thesis.

The argument will not work. Carter's failure was that he did not address himself to the problems which were worrying most people. Few people care about government reorganization or even the Panama Canal. But inflation and the power of the U.S. in the world are of concern to people.

Carter lacked any sense of political strategy and thereby the majority of citizens came to believe that he was not in control of the events which most concerned them.

A biography of Jimmy Carter is therefore more than the story of a man. The study of his presidency may permit us to assess the importance of skill in relation to the many constraints on leadership in American politics today.

Betty Glad does not address these larger questions in her biography of Carter. She wishes to understand him as a person. She begins her search with two puzzles. First, Carter's career displayed great drive for political office but no evident passion for the achievement of principles like justice. Second, he is good at winning but poor at governing.

She documents with exasperating thoroughness the early life of a young man who had no outstanding achievements, yet believed himself destined for greatness. Although she does not make the point, the contrast with Harry Truman is apparent. Truman attained more than he sought and his humility was genuine. Glad focuses on Carter's habit of pretending to be more than he is in the public persona which he has created. For example, he later falsely described himself as a finalist for the Rhodes Scholarship when he had been eliminated early in the competition. There are many little stories like this and they add up to a man who wishes to be more than he is.

In his election to the Georgia Senate, Carter manifested the political style which was to be his trademark: intensity, ability to create a good press, the portrayal of himself as an innocent, and quiet manipulation of political organization. Glad points out but does not explain the combination of moralism and willingness to go for the jugular of his opponent.

As a gubernatorial candidate, he was a centrist who avoided labels and sent mixed messages. He ran as an outsider against Atlanta and its "establishment" but his populism was "stylistic" rather than substantive. His program was one of "good government" with administrative reorganization as the top priority.

Glad foresees Carter's style as president in his governorship. He achieved most of his legislative programs through a combination of frontal assault and private trimming, but he was never comfortable in his relations with legislators and resented opposition as somehow immoral. She concludes that he was a good but not extraordinary governor, who might have had difficulty being reelected.

Carter reenacted the entire Georgia story on the national stage. Glad originally set out to tell the story of the 1976 campaign, but since Carter won, she gives us all the reenactment except for the denouement in 1980. However, there is more material on the campaign than anyone would wish to know. The primary systems and heightened role of the media were perfect for nan outsider candidate who made himself the issue. Carter understood that Americans yearned for a restoration of "good authority." He asked them to trust him and projected himself as a man capable of greatness.

Her chief criticism of Carter as president is that he brought no economic or social philosophy to government. We again see the emphasis on procedures—reorganization, zero-base budgeting, civil service reform—and efforts at comprehensive solutions which floundered for lack of political preparation. But we also see successes like the Camp David agreement which stemmed from the same intensive homework which blocked any larger vision. She ends the narrative with Carter's skillful use of the hostage issue to protect himself from criticism in the 1980 primaries. We see the campaigner who cannot govern.

Her final picture of Carter is a long way from James David Barber's "active-positive." She sees him as a man who is so in control of himself that he lacks creativity, who lacks a sense of the tragedy in history and believes that good intentions and practical reason can solve all problems. Inevitably, he promises more than he can achieve.

The author invokes Karen Horney's model of the expansionist, narcissistic personality to explain Carter. The expansionist seeks to attain an idealized self who has mastery over all problems. The drive has a compulsive nature and reflects a deep psychological need for victory and recognition. It is winning that matters, not what we do with victory. Thus, the skills which enabled Carter to win elections served him poorly in office because one does not govern by manipulating images of the self.

It now appears that Jimmy Carter was defeated in 1980 because many voters did not believe that he was in command of events or of the presidency. Betty Glad helps us understand the psychological roots of this failure. Carter was never fully in touch with the issues that most concerned people. He never gave sustained attention to economic problems or relations with the USSR or even relations with the Congress. His energies were devoted to many peripheral issues in the search for the appearance of mastery.

Any Democratic president might have failed in these years in the face of the exhausted agenda and divided coalition of liberalism. But Glad would not regard that as an excuse for Carter. Her criticism is devastating, for she charges that the man is incapable of dealing with the world in realistic terms. She does not provide the final or definitive word on this question, but future biographers will have to take the argument very seriously.

ERWIN C. HARGROVE

Vanderbilt University

A Policy Approach to Political Representation: Lessons from the Four Corners States. By Helen M. Ingram, Nancy K. Laney, and John R. McCain. (Baltimore: Johns Hopkins University Press, 1980. Pp. xviii + 270. $22.00.)

Representation, with good reason, has been a central concept and a continuous source of dispute wherever democracy has been tried. And that has given rise to a long debate among political theorists about the problems of fair representation, of even the question whether the many can be fairly represented by the chosen few. More recently the subject has been a focal point of empirical research, producing an enormous body of literature devoted to comprehending the opinions of the represented and comparing those with the ideas and acts of the representative. That literature has emphasized the public's lack of sophistication in political matters; indeed in some instances the public appeared downright stupid and self-contradictory. V. O. Key, however, reviewed the findings of these analysts and devoted a book (his final one) to the "perverse and unorthodox argument . . . that voters are not fools. . . ." Helen Ingram, Nancy Laney, and John McCain take up a position beside the master, finding considerable agreement about policy preferences between legislators and voters—at least in their corner of the world.

In many ways this work is a tidy little gem; its research techniques are imaginative but sound; its analysis is persuasive. And if the writing is occasionally cluttered with jargon, Ingram, Laney and McCain sin far less than most of their contemporaries doing comparable work. Essentially, they have sought to find links between state legislators' actions and the opinions of their constituents on matters of significant public policies. The scene is the "Four Corner States," so called because the borders of Arizona, New Mexico, Colorado and Utah meet at a single point, the only place in the country where that happens, but it is not this geographic coincidence that is important; rather they are states with rapidly growing populations and a host of environmental and other problematic questions—like the question of water supply in that often-arid countryside.

The raw material of the work consists of interviews with legislators and questionnaires sent to voters. The writers sought to interview all of the senators from the four states and managed to converse with 9 out of 10 of them. The questionnaire went to 50 voters in each senatorial district and nearly three-quarters of the recipients responded (some after persistent prodding). They then proceeded to compare the opinions of both voters and state senators across a fairly wide range of policy questions. In a number of graphs showing the distribution of voters' and legislators' positions on 10 environmental questions, there is a surprising consistency of viewpoints between voters and representatives. Of course there are also differences between the two groups; legislators were more inclined to be guided by party positions and ideology than were voters. Moreover, there were differences among the four states, and one might expect that states with other traditions—particularly states with strong party influence and tight discipline—might differ even more were these methods to be applied there.

Nevertheless, one should give a fair hearing to good omens regarding the operations of democracy these days. It is true that the authors do not deal with the divisive conflicts that now agitate older, more highly urbanized states, but what they have given us merits attention and repetition elsewhere.

DUANE LOCKARD

Princeton University

Public Representation in Environmental Policymaking: The Case of Water Quality Management. By Sheldon Kamieniecki. (Boulder, Colo.: Westview Press, 1980. Pp. xi + 131. $20.00.)

It is encouraging that there are political scientists who still see the environment as a significant issue worthy of study. Sheldon Kamieniecki examines comparisons of leaders' and citizens' beliefs and leaders' perceptions of public beliefs on water quality issues. It includes a concise history of federal water pollution legislation, noting the effects of lobbying in Congress which has resulted in past postponement of federal deadlines. Section 208, of the 1972 Federal Water Pollution Control Act Amendments requires citizen participation in the development and implementation of regional water quality plans to meet national water quality standards. Kamieniecki states his primary concern as the need for greater citizen participation.

The major survey from which the data were drawn was conducted by the Environmental Studies Center of the State University of New York at Buffalo and consisted of questions asked of leaders and citizens of the Niagara Frontier region. Some of the survey data results were not surprising, namely, that most people are reluctant to take an active role in planning, and the less educated are more willing than the general public to leave planning to the specialists.

A difficulty with some of the questions was whether the questions asked really prove the hypothesis posed. For example, Hypothesis 2 stated that blacks will be more supportive of policies that improve their surrounding environment than the general population. According to the author, the data did not support the hypothesis. But the specific questions asked of respondents concerned planning, relinguishing local power to a regional body and a government's right to property—questions which do not necessarily satisfy the meaning of the hypothesis. Blacks may yet support more specific or qualitative environmental goals such as clean drinking water, parks or clean air while not understanding how the governmental mechanisms described in the questions would improve the environment.

An interesting finding was that the residents of the Niagara Frontier "were willing to trade-off their freedom to do as they wish in favor of planning what was best for their community" (p. 52). Kamieniecki argued that restrictions on personal freedom might be necessary to improve water quality. But the respondents were not asked if they would give up personal freedom for some specific end. The use of a question with some boundaries, e.g., mentioning what the goal was or how giving up personal freedom to obtain that goal, might yield a more meaningful result.

On the question of how well leaders perceive the public's views on environmental issues, it was found that "208" planners and environmentalists hold beliefs similar to those of the broad public but that they fail to predict the beliefs of the public. Even if leader perceptions were more accurate, many of the questions asked in this survey were of a general nature and could not serve as a guide for leaders.

Nevertheless, the data presented by Kamieniecki raise some interesting questions about the disparity of leader's perceptions of citizen opinions and public opinion itself. The author offers some explanation in chapter 5, i.e., leaders are overly cynical and misjudge public opinions because of their eagerness for results and their complete involvement in their work. It might be worth asking whether this is characteristic of environmental policy makers or other leaders as well.

The final chapter deals more directly with the nature of citizen participation. The data reveal that the actual beliefs of citizens and leaders are similar on many issues. It is the perceptual accuracy of leaders which is problematic. The author suggests that because of the disparity between public opinion and leader's perception of public opinion, citizen input is inadequate. Further research might focus on whether the coincidence of beliefs between leaders and citizens is a stable relationship or if it is susceptible to change as new issues emerge.

The author concludes by saying that officials should be given more accurate information on public opinion (p. 88). This final chapter discusses mechanisms (public hearings, workshops and ad hoc committees) which could be employed toward that end. However, for the public to register meaningful public opinion, they also must have timely and accurate information.

This book, published as a Westview Replica Edition, is representative of a trend to accommodate the increasing number of publishable, specialized and scholarly works on an economical scale through the use of camera-ready copy. However, the lack of professional editing (correction of spelling, typographical errors, incomplete sentences and awkward grammar) can detract from the appearance and impact of a book. Greater use of headings and transitions between sections in this instance would have added to the clarity of thought and would have alerted the reader to subtle changes in the discussion. Nevertheless, Kamieniecki's effort should be commended for its attempt to link political participation with environmental issues in a thoughtful and concise manner, and I hope that other political scientists will continue to address issues concerning political participation and the democratic process in an environmental context.

REGINA S. AXELROD

Adelphi University

The Press as Guardian of the First Amendment. By John Lofton. (Columbia: University of South Carolina Press, 1980. Pp. xiv + 358. $14.95.)

The press has not served as an effective guardian for freedom of expression, according to this first historical survey of newspaper reaction to free press and speech issues. John Lofton, an editorial writer for the *St. Louis Post Dispatch*, a historian and attorney, contends that the general circulation press has usually not supported those with unpopular views. The newspapers ordinarily champion free expression only when their own vested economic interests are involved.

In this critical analysis of the press, pre-World War I incidents receive extensive coverage. Relying on James Morton Smith's *Freedom Fetters: The Alien and Sedition Law and American Civil Liberties* (Cornell, 1956), Lofton states that the Federalist press called for prosecution of Republican editors under the 1798 Sedition Law. Making extensive use of Russell B. Nye's *Fettered*

Freedom (Illinois, 1972), Lofton contends that the southern newspaper editors "applauded restrictions" (p. 101) on the abolitionist press. Destruction of an abolitionist press in Cincinnati was supported, with one paper calling it "the most systematic, orderly and well behaved mob we have ever witnessed" (p. 90). In the post Civil War era big newspapers pursued the attitudes of big business and did not defend the radical labor press.

More restrictions were imposed on freedom of expression during World War I and its aftermath. Yet there was almost total abdication of the general circulation press of any responsibility to uphold freedom for dissent. Lofton repeatedly criticizes the *New York Times* for its lack of concern for free expression and its support of anti-civil libertarian court decisions, legislation, and executive actions such as the Palmer Raids.

The press has been reluctant to support freedom of expression against government claims of national security as during the McCarthy era. In the Pentagon Papers case of 1971, Lofton contends the *New York Times* made "narrow and timid arguments" (p. 259) and not a lofty appeal to the First Amendment rights. Even though the resulting limited victory against prior restraint was widely supported in the press, the media did not mount a campaign to end excessive government classification and secrecy. C.I.A. censorship received limited editorial attention even from the papers enjoined in the Pentagon Papers controversy. The right of the *Progressive* magazine to publish a hydrogen bomb story in 1979 received ambivalent media support.

Lofton recommends that the newspapers should be active and articulate champions of the rights of free expression. Citing problems raised by Herbert Marcuse, he urges papers to allow substantial access on a regular basis for those who do not own a press and who have conflicting or unpopular views.

Several methodological problems can be identified. Less than half of the book covers events since World War I—the time when Supreme Court decision making on First Amendment rights began. Detailed accounts are given to problems prior to World War I while some recent issues are inadequately discussed. Furthermore, parts of the book are concerned primarily with general first amendment history and not press reaction. Lofton makes extensive use of the primary source of newspaper editorials, but he fails to explain why particular papers were selected, i.e., circulation, reputation, section of country or availability. Tables and charts on his content analysis research would be useful in showing the extent and dimension of media support for different issues.

Lofton's heavy reliance on newspaper editorials as a primary source causes one to question the comprehensiveness of his account. While newspaper editorials are one significant factor in determining the response of newspapers to First Amendment issues, additional research approaches could be used. A study of interest-group tactics and strategies in *amicus* briefs, lobbying, funding, and education could show other patterns of reaction.

Lofton's work is an informative historical account on the failure of newspapers to champion free press and speech editorially. His general arguments are persuasive; however, a definitive and systematic analysis of press reaction to free expression issues still needs to be pursued.

BYRON G. LANDER

Kent State University

The State and Human Services: Organizational Change in a Political Context. By Laurence E. Lynn, Jr., with the assistance of Timothy C. Mack. (Cambridge, Mass.: MIT Press, 1980. Pp. xii + 217. $15.00.)

This book is in a series of monographs that combine substantive public policy analysis with "insights into the workings of American political institutions" (p. vii). Lynn analyzes processes of organizational change in human service programs and agencies in six states: Arizona, Florida, Georgia, Minnesota, Pennsylvania, and Washington. He focuses on decisions made by the originators of change proposals to ascertain what they were seeking, the processes of debate and deliberation, and the results. The governors of Georgia and Washington initiated changes and had to combat legislative and bureaucratic inertia. Arizona and Florida legislators were the advocates of comprehensive reorganizations, and they had to fight indifferent or reluctant governors. Minnesota officials adopted a gradual, modest approach to reorganization, while the advocates for change in Pennsylvania encountered "numbing inertia" from the governor and legislative leaders.

Guided by the assumption that most public policies are products of a continuous political debate over the proper scope of authority for the states and the national government, Lynn prefaces his cases with a chapter on "The Changing Shape of Federalism." In the evolution of human service programs and organizations, a fundamental tension has developed between the advocates for the needy (reformers and service professionals) and public officials. This schism, Lynn says, was born out of each group's definition of need. Professionals tend to be absolutists and define need in philanthropic terms: "A person who can be helped should be helped." But officials take a more pragmatic view "and weigh the claims of advocates for service expansion against the availability of resources and the preferences of the larger community" (p. 14).

By the 1970s, several important strands of organizational change meshed to shape state reorganizational debates: greater executive control, growing resistance to federal control, and a call for more service integration. Lynn shows how these issues were chewed up and digested in the states. He also shows how individual state politics renders different substantive results. In Florida, Georgia, and Arizona, for example, changes were introduced from the "top down," and emphasized "service integration . . . as an economy measure or as a means of achieving greater control over categorical program interests" (p. 150). In Minnesota, on the other hand, because of the strong role of county government, change was designed from the "bottom up" to emphasize improvements in service delivery. Lynn's prescription for success is the Minnesota approach. "Though significant changes in status quo have occurred in Florida, Georgia,, and even in Washington and Arizona, there is simply no firm evidence that economies, cost containment, or more effective services have been the result. Gradual change built on consensus may be, in the end, the only kind of change that can bring about genuine and verifiable improvements in service delivery" (p. 151).

Case studies can cause analytical headaches. After disassembling the cases, analysts must then deal with the thorny problem of what they mean individually and collectively. As they proceed through this exercise, they may feel as frustrated as all the King's horses and all the King's men when they tried to put the pieces back together again. But, where they fail, Lynn succeeds. He not only unravels the nuances of individual cases, he deals with the Humpty Dumpty problem by formulating "general propositions about both the processes and the character of organizational changes in the states. . ." (p. 173). While a short review cannot do justice to Lynn's synthesis, two conclusions merit mention. First, regardless of the initiator, Lynn concludes, "The pace, character, and success of organizational change are colored by the governor's performance" (p. 174). Second, "elected state officials have on the whole a greater sense of responsibility toward economy and efficiency in governmental operations than they do toward the effectiveness of coverage in human services" (p. 178).

Lynn has simply written a successful book. With the aid of a Mack, he trucks us across six states pointing out individual characteristics or reorganizational politics. Because he is guided by a well-thought out plan of analysis, at journey's end he is able to provide us with an overview of where we've been and the significance of the trip for future excursions into the morass of state politics and organizational changes. What more could we ask?

ROBERT D. THOMAS

University of Houston

Unelected Representatives: Congressional Staff and the Future of Representative Government. By Michael J. Malbin. (New York: Basic Books, 1980. Pp. viii + 279. $15.95.)

Two minor liberal myths about staffing cohabitate uncomfortably. One is that the heavy workload of modern politicans surely can be made manageable if they have adequate expert staffs; the other is that there is an inevitable Parkinson's Law in staff growth which ultimately leads to staff control of elected politicians. Three decades ago, the clamor was for extraordinary acceleration in the growth of congressional staffs. Today, there is a kind of thermidorean worry that things have gone too far, that the staff has come to be on top rather than on tap. Recently, congressional staffs were said to be "the invisible force in American lawmaking" (Harrison W. Fox, Jr., and Susan Webb Hammond, *Congressional Staffs* [Free Press, 1977]). Now, they are "unelected representatives." The implication is, of course, that Congress is being run, and public policies legislated, not by our elected representatives, but by the invisible, behind-the-scenes manipulators serving on members' and committee staffs who are not accountable to the voters of the country. No close observer of congressional life would want to deny that staffs are influential, but that is a far cry from the claim that staffs control Congress.

In fact, no such claim is substantiated in this book. Michael Malbin is properly concerned about the appropriate role of congressional staffs. His interests in the extent to which these staffs have alleviated the heavy burden of congressional work, and effected changes in the capacity of members of Congress to serve as representatives, lead him to focus upon staff activities in various legislative settings. His book is an exploration of the staff role in these settings, and so it reads as a rather nicely integrated series of case studies. The staff director of the Senate Select Committee on Small Business shapes the committee's working agenda; the staff of the Senate Government Operations Committee's Subcommittee on Intergovernmental Relations "shepherds" the Sunset Bill through the Senate; House and Senate staff members negotiate intercameral differences over the

GI Bill (the "case of the phantom conference"); the staff of the House Energy and Power Subcommittee engages in oversight activity; the nonpartisan staff specialists of the House Budget Committee make policy recommendations to members based on their economic projections; staffs of the Congressional Budget Office, the General Accounting Office, and the Congressional Research Service provide "policy analysis" on the issue of natural gas deregulation.

Malbin's principal purpose in telling these stories is to make the argument that congressional staffing has developed in such a way as to encumber the deliberative function of Congress. First, he argues that the large congressional staffs, and particularly the so-called "chairmen's staffs" of committees, attenuate the flow of reliable information from staff to committee leaders, and from committee leaders to rank-and-file members of Congress. Moreover, as gatekeepers for the schedule of members' time, the staff screens information and contact in a self-serving manner. Here, amid a melee of anxieties real and imagined, is the crux of Malbin's argument:

> Since the staff people are not themselves elected, and since their future careers in the Washington community often depend on their gaining reputations as innovators, it is to their interest to spend time listening to people whose ideas may help them put something new on the oversight or legislative agenda, even if those people have no political constituencies of their own (pp. 248-49).

This kind of staff screening tends, Malbin believes, to be quintessentially technocratic and ideological. To the extent the information flow is interdicted in this way, the congressional process is depoliticized.

Second, he contends that the staffs, and especially the "entrepreneurial" ones, exacerbate, rather than assuage, the problem of congressional workload. Thus, "instead of freeing the members to concentrate, the staffs contribute to the frenetic pace of congressional life that pulls members in different directions, reduces the time available for joint deliberation, and makes concentration all but impossible" (p. 243). Third, the growing tendency for staffs to negotiate legislative issues, instead of members negotiating with one another directly, leads to mere "deals" between self-interested parties, much as lawyers make settlements for their clients out of court, rather than the kind of give-and-take among elected representatives which is aimed at making decisions for the common good.

Malbin offers little in the way of proposals to improve the Hobbesian condition of the modern Congress. He momentarily toys with the idea that each committee should have both a non-partisan professional staff and a personalized, entrepreneurial, political staff, but despairs that this would solve anything. He considers the possibility of reducing the magnitude of the congressional agenda, but concludes that the complex agenda of government as a whole would have to shrink to effect a smaller congressional workload—a prospect he does not foresee. He cannot prescribe a solution for the increasingly technical character of public policy issues, or the tendency for members of Congress to deal with them in technical, rather than in ordinary political, terms.

This piece of investigative reporting presents a challenge to the adequacy of representative government, as representation was understood by James Madison in *Federalist* No. 52. Like the Harvard undergraduate who wrote his honors thesis on the Newfoundland fisheries issue from the point of view of the fish, Malbin has adumbrated the problems of congressional deliberation and representation mainly from the viewpoint of the employees. The result is myopic. A more adequate assessment will require taking the perspectives and activities of members of Congress themselves into account. Or, more broadly, the perspective of the institution. Congress as an institution is more than a happy hunting ground for head-hunting, aggrandizing staff. Accordingly, Congress is more than an arena in which staff influence comes into play, as Malbin occasionally admits. It is also a system of constraints—of loyalty to the chairman, limited advocacy, deference, anonymity, specialization, limited partisanship, leadership, organization, and isolation. In recent years these constraints may have diminished, but they have not evanesced. A more balanced inquiry would want to consider such constraints on staff behavior along with staff capabilities for performance.

SAMUEL C. PATTERSON

University of Iowa

The Emigration Dialectic: Puerto Rico and the USA. By Manuel Maldonado-Denis. (New York: International Publishers, 1980. Pp. 156. $9.50, cloth; $3.25, paper.)

The Emigration Dialectic is the English translation of *Puerto Rico y Estados Unidos: Emigración y Colonialismo* which won Cuba's Casa de las Americas prize for the best essay written in 1976. In this study of Puerto Rican emigration to the United States, Manuel Maldonado-Denis states his assumption clearly; this exodus "is not a fortuitous . . . social phenomenon, but rather something profoundly connected to the capitalist-colonial mode of production" (p. 29). Simply put, when the capitalist center needs a cheap and abundant labor force, the peripheral nations provide a ready pool of workers on which to draw (p. 14). For Maldonado-Denis, the labor flow from Puerto Rico to the United States closely parallels the flow of southern European "guest workers" northward—a flow that has helped to create "economic miracles" as in the Federal Republic of Germany.

We are told early on in the work that the choice of "emigration" rather than "migration" to describe the Puerto Rican case is based on politics rather than semantics. One "migrates" within one country but "emigrates" from one country to another. To Maldonado-Denis, Puerto Rico is not part of the United States, but "a Latin American nation which has been under U.S. colonization since 1898" (p. 16). Therefore, he uses "emigration" to highlight the exodus of hundreds of thousands of islanders to a *foreign* nation.

The main contribution of *The Emigration Dialectic* is that each of its seven chapters raises issues that should stimulate further exploration and analysis. In chapter 2, for example, the author discusses "neo-Malthusianism" on the island as seen in the controversy over the sterilization of large numbers of Puerto Rican women. Sterilization and emigration are analyzed as two processes directly related to the island's economic development program and on both issues, according to the author, the Puerto Rican government has acted irresponsibly.

The high points of the book are chapters 6 and 7 on "cultural assimilation" and "those who return" to Puerto Rico. First, Maldonado-Denis analyzes the process of cultural assimilation both on the island and in the States, and concludes that "the difference in the process . . . in Puerto Rico and in the metropolis is one of degree and not of type" (p. 104). In this chapter, the author's discussion brings to mind some of the more moving passages in the works of Memmi or Fanon on the ravages of colonial culture on the colonized. The chapter on the "New Yorricans" who return to Puerto Rico is another perceptive discussion; "New Yorrican" is a term loosely applied to all Puerto Ricans born on the mainland. The mainland-born Puerto Rican may not only feel alienated in the States but may also feel alienated on his return to Puerto Rico. "The so-called New Yorrican is the target of the hostility of those on the island. . . . [He] becomes a scapegoat, the victim of a national chauvinism" (p. 127).

The Emigration Dialectic is a book written with passion, and so is likely to invite criticism from those hoping for a more balanced expository style. As one who has been critical of Puerto Rico's dependent development and its concomitant emigration process, I found that at times Maldonado-Denis' broadsides against policy makers on the island and the mainland were not sufficiently documented and sometimes his evidence was inconsistent. One example involves unemployment statistics for Puerto Rico. In 1976, the official unemployment rate was 19 percent, but critics of the island's development program often cite an unofficial rate that is about 10 points higher. Throughout the book, Maldonado-Denis shifts between a 30-40 percent rate for the same period and usually neglects to mention that this is an unofficial guess. Another problem involves the book's translation from Spanish to English. At points the translation is not merely clumsy, it renders an idea meaningless.

Despite these reservations, I recommend this as a provocative discussion of Puerto Rican emigration and the dynamics of emigration more generally. By examining the socioeconomic "push" factors in Puerto Rico and the "pull" factors in the States at the height of the Puerto Rican exodus in the late 1940s and 1950s, we can better understand the motives of more recent emigrants from other Caribbean islands, Central, and South America. Finally, students of ethnicity will find this work useful. Maldonado-Denis sheds light on the reasons why Puerto Ricans and other ethnic groups may be resisting the pressure to "melt" in the great American "melting pot."

SHERRIE L. BAVER

City College, City University of New York

Promise and Performance: Choosing and Implementing an Environmental Policy. By Alfred A. Marcus. (Westport, Conn.: Greenwood Press, 1980. Pp. xix + 204. $18.95.)

The Environmental Protection Agency (EPA) and the Clean Air Act Amendments of 1970 (CAA) were more than merely major new bureaucratic and policy mandates. They also represented ideas generated by three important reform movements. Substantively, they reflected the environmentalist demand for a major new government effort in pollution control. Organizationally, they exemplified the long-standing proposal offered by a variety of presidential commissions that specialized regulatory agencies be headed by single administrators who are accountable to the president. And legally, they responded to critics who called for clearer and less ambiguous policy mandates from Congress. It is in these qualities that the EPA and CAA showed great promise; but, as Alfred A. Marcus demonstrates through this interesting case study, it is in these very same areas that environmental policy efforts were doomed to failure.

The lesson Marcus draws from his analysis is at once both simple and profound: if you try to satisfy everyone, you're likely to please almost no one. The EPA's unsuccessful attempts to respond to the contradictory objectives of its two masters (Congress and the White House) as well as the ever-expanding demands of its primary clientele (environmentalists) were further aggravated by the diverse viewpoints which emerged from among the policy, program, and research components of the newly formed agency. Marcus does a credible and insightful job of describing EPA's dilemmas during the 1970s and how that agency tried in vain to contend with them. It is only as he moves away from this major thesis and descriptive task that his study becomes awkward, incomplete, and frustrating to the reader.

Minor themes in the work seem to slip from Marcus' grasp. In a number of places he raises issues and proceeds to abandon them with less than complete analysis or without having joined the specific issues to the case study. Not only is this a source of frustration but it also gives the impression that the author lacked confidence in his own analytic capabilities since he is often diverted from his task in a search for a coherent framework. These problems are most in evidence in the first and final chapters. Rather than focusing on the major theme of the study at the outset, Marcus opens his work with what proves to be an irrelevant comparison of policy evaluation criteria. Only after the first ten pages does he get to meaningful information. Ironically, the final pages of the book suffer from the opposite problem, for it is here that Marcus offers several relevant points on the difference between divided and concentrated authority patterns—points which, if provided much earlier in the text, would have vastly increased the value of the case study. As it is presented, the reader is left "hanging" in this and other parts of the work.

The problems that Marcus faced are not unique among those who study policy implementation and use the case study approach in their analyses. When Pressman, Wildavsky, Bardach and others launched this field of study, it seemed to hold considerable promise as a means for explaining policy successes or failures, and many political scientists picked up the challenge and ran with it. But recent implementation studies indicate that the runners have hit a "wall" and that the work of these analysts has come to a point where the lack of new and insightful concepts and analytic frameworks might render future studies redundant. Marcus' somewhat awkward search for a relevant perspective is a sign that all is not lost.

MEL DUBNICK

University of Kansas

Three Mile Island: Prologue or Epilogue? By Daniel Martin. (Cambridge, Mass.: Ballinger Publishing Company, 1980. Pp. ix + 251. $14.50.)

Like "Pearl Harbor" and "Watergate," "Three Mile Island" has become part of our language. It is the place where millions of people believe that what was supposed to be impossible almost happened: a catastrophic nuclear accident.

Daniel Martin's fine book can be read and enjoyed as a gripping minute-by-minute, even second-by-second, account of what did happen at Three Mile Island between March 28 and April 2, 1979. And, as they say of all good thrillers, "few readers will be able to put it down." I certainly could not, nor could several friends to whom I have passed it along.

But, to Martin's considerable credit, his narrative moves beyond the inherent melodrama of the episode into some fertile territory for reflection about the operation of government. It has been many years since Karl Deutsch first taught us about the importance of "the nerves of government," about the meaning of variations in the ways that organizations receive, process, and respond to information. When he asked his students about the difference between refrigerators and chickens, he was not looking for the observation that you cannot unplug a chicken. Rather, he was making the point that even chickens have rudimentary cybernetic capabilities enabling them to adapt their behavior to their environment. Martin, unhappily, confirms that during the accident at Three Mile Island the responsible agencies of the U.S. federal government behaved more like refrigerators than like chickens. His story is one of the total failure of the Nuclear Regulatory Commission, and other agencies as information-processing entities. This failure was so complete, so utter, and so consistent that Martin's account is hilarious at the same time that it is deeply troubling.

About half-way through the book, I began to underline phrases and verbs with a recurrent theme: "did not believe," "did not know," "garbled," "did not know how," "could not find out," "no answer," "misquoted information," "misinterpreted data," "could not get through," "invalid assumption," "inaccurate data," "mistakenly understood," "was not aware."

The result was that "the federal actors in charge helped turn a crisis into a new calamity."

For several years, Tony Oettinger, Director of the Harvard University Program on Information Resources Policy, has been warning of the risks of concentrating on the "bones and muscles" of government and society while ignoring the nervous system. Martin's account of the events at

Three Mile Island is a vivid demonstration of the urgent need to listen to this warning.

Many readers will, perhaps, be frustrated that Martin's book does not so much end as it merely stops. Its brevity and overall tone of haste is probably partly due to the author's understandable desire for timely publication of research with high contemporary interest. No doubt it is also partly explained by his understanding that the accident at Three Mile Island by no means ended in April 1979, or in 1979 at all, or even in 1980. The melodrama on the Susquehanna River continues into 1981, and by most recent estimate will go on for another *five years*.

I urge Martin to return to the scene and, in a more leisurely manner, to assess whether the Nuclear Regulatory Commission and other responsible government organizations have indeed learned some lessons and are behaving with, at least, the cybernetic capacity of chickens.

IRVIN C. BUPP

Harvard University

The Cost of Good Intentions: New York City and the Liberal Experiment, 1960-1975. By Charles R. Morris. (New York: W.W. Norton, 1980. Pp. 256. $14.95.)

While neo-conservatives vie with each other in disparaging the quest for social justice, Charles R. Morris lends himself to the debate. The title of this book seems to recall Samuel Johnson's observation that "the road to hell is paved with good intentions" and Carter Glass' allegation that "a liberal is a man who is willing to spend someone else's money."

New York City should not have to apologize for the fact that its heart was bigger than its purse. An estimated 70 percent of all Americans or their ancestors came through this port of entry and benefited from its educational system, housing stock, job market, health care, transit facilities and social services. Now, safely ensconsed within the establishment, some who profited from the city's humanitarianism denigrate efforts to guarantee opportunities for new generations.

Morris, former Assistant Budget Director for the city and current vice-president of Chase Manhattan Bank, has written a compact chronicle of the origins, evolution and aftermath of our nation's most traumatic municipal crisis. In 1975, New York City ran out of money. It was unable to repay loans that became due and simultaneously maintain normal operations. The narrative surveys state and federal rescue efforts, creation of the Municipal Assistance Corporation to stretch out short-term debt, and establishment of the Emergency Financial Control Board which placed the city in virtual receivership.

Morris advances the thesis that the fiscal crisis could have been avoided if officials had cut back on spending years earlier instead of borrowing to the brink of bankruptcy. He sharply criticizes the ineptitude of local politicians, the demands of municipal unions, and the inclination of voters to elect candidates who promised the greatest litany of services. Over the years, demographic changes altered the ethnic and socioeconomic composition of the city and eroded the tax base. In the end, hapless officials lost control and failed to comprehend the catastrophe that engulfed them.

This is a tale of three mayors. The vignettes may be useful for a primer on city politics, but contain few original insights. Mayor Robert F. Wagner is portrayed as a well-intentioned executive whose indecision caused him to drift from crisis to crisis as he proved incapable of responding to changes buffeting New York. John V. Lindsay emerges in the familiar garb of puritan and moralist who plunged into warfare against power brokers but contributed to the crisis by failing to retrench after the economy declined. Abraham D. Beame, a former comptroller, knew the gravity of the problem but overestimated his ability to survive the storm. Ultimately, Beame's budgetary legerdemain and public squabbles with his comptroller—an institutional dilemma in the city—undermined investor confidence in the administration.

Morris maintains that New York's ordeal had nationwide ramifications for state and local borrowing, and threatened market stability at home and abroad. To this extent, the book contains material of interest to students of intergovernmental finance. Valuable sections focus upon dubious fiscal practices, e.g., underestimating expenditures, inflating revenue, skipping reserve deposits, shifting accounts from one year to another, alternating between cash and accrual systems, borrowing against uncollectible receivables, and transferring expense items to capital budgets.

Without condoning municipal mismanagement, the reader may legitimately question the extent to which Morris absolves the financial community of responsibility. Bankers who blew the whistle on New York's mounting difficulties or refused to recommend city securities to their clients were not innocent bystanders. They had reaped profits from the city's easy borrowings. One looks in vain for any acknowledgment that the bankers signaled a "run" on the city to lighten burdens on business or used the fiscal crisis as a load-shedding device to force New York to curtail services to its sprawling, pluralistic population.

The scholarship is uneven. Morris concedes that he was not involved in most of the events and directs the reader to "official records or other standard sources." Unfortunately, he has failed to compile a formal bibliography. Journals of major public bodies (e.g., City Council and Board of Estimate) have not been used. The index contains only passing reference to the city's unique Board of Estimate which shares decision-making powers with the Mayor. Morris has relied on newspapers rather than the readily available statutes for provisions of the state's emergency legislation. The account of the rescue effort is surprisingly sketchy. The 1975 city charter, which mandated fiscal reforms, is largely overlooked. Factual errors occur. Governor Nelson Rockefeller's resignation in December 1973, not his acceptance of the vice-presidency a year later, elevated Lieutenant Governor Malcolm Wilson to the executive mansion. Elsewhere, the author confuses the name of the Brooklyn Democratic chairman who opposed Mayor Wagner's bid for a third term. Value judgments abound. Morris cites the intelligence of budget directors who employed him but overuses the epithet "martinet" when describing three commissioners with whom he did not work.

This is not a definitive sequel to Wallace S. Sayre's and Herbert Kaufman's *Governing New York City* (Norton, 1965), which was far more clinical and comprehensive than the present book. As banker and budgeter, Morris views recent city history primarily in terms of management and finance. He never examines the impact of the fiscal crisis upon the people or the ramifications in the loss of home rule. He appeals to those who inhabit corporate board rooms rather than the sidewalks of New York.

LEONARD P. STAVISKY

Columbia University

Subliminal Politics: Myths and Mythmakers in America. By Dan Nimmo and James E. Combs. (Englewood Cliffs, N.J.: Prentice-Hall, 1980. Pp. xiii + 256. $10.95, cloth; $4.95, paper.)

This is a lively (even breezy: the title of chapter 7 is, "Brother, Can Your Paradigm?") review of a topic that is receiving increasing attention in American government studies. It covers much ground in a perceptive way. But its intent is uncertain: is it a simple *exposé* of myths and mythmakers in America, or is it a critical analysis of an important, functional element in the American political system?

The book's topic is not "what people know about American politics." It is about "what people *think* they know about American politics." Specifically, the topic is the "subliminal" element in American politics, "the taken-for-granted set of assumptions, conceptions, and ideas about American politics that lies below the threshold of consciousness, that substantial realm of dramatic accounts whose accuracy and plausibility go largely unquestioned" (p. xii).

But what if, as this book intends to force us to do, they get questioned? Is a consciously believed myth, one that is no longer subliminal, still a myth? This question is not satisfactorially dealt with in the book's central arguments.

The book has two parts. Part 1 defines myth, in the light of the literature on the subject, and identifies the content of America's founding myth and its many contemporary myths. This part shows the essentiality of myth to social organization and how the accretion of it over time has given Americans a "usable past" by which to identify and feel proud about themselves and their country. But this part also systematically shows that much of this "usable past" is contrived; at least it is embellishment of history, at worst it is imaginary history. Throughout, the treatment argues nearly explicitly that mythic thinking, however socially useful, is to be compared unfavorably to scientific thinking.

Part 2 carries forward this ambivalent but generally unfavorable view of myth in politics. In a series of chapters, it examines the myth-making of politicians and their aides, the purveyors of popular culture in the entertainment industry, reporters and TV commentators in the news media, and political scientists. Although many of the points are obvious, this is a useful survey and the last chapter is as particularly interesting, as readers of this *Review* might expect. For it begins by asking, would not political scientists, "above all groups, be immune from the disease of mythifying?" (p. 190). The answer is "simple but severe: Political science, a learned discipline, makes myths" (p. 220). The best part of the chapter is on political science as a service industry to the community.

The chapter ends with the question, can political science demythologize itself? The authors would put that question to the community at large as well. Their answer to it is that, because of human limitations, we cannot live without myth altogether, but we can come measurably closer to that ideal by elevating scientific modes of thought as best we can and, above all, by living with uncertainty, by being willing to live without the certainties that uncritical myth provides.

This answer fudges the central question: what does myth do? If, as I suspect, myth gives meaning—which no amount of "science" can—then "demythologizing" political science, America, or whatever is not the issue. The issue is, which myth should we believe and why?

H. Mark Roelofs

New York University

Presidential Secrecy and Deception: Beyond the Power to Persuade. By John M. Orman. (Westport, Conn.: Greenwood Press, 1980. Pp. xv + 239. $22.95.)

John Orman is concerned that political scientists have been unwilling to deal with the problems of covert use of power in a theoretical manner that would permit both a method of evaluating presidential acts and a guideline for future presidents. His attempt to fill the gap is an important addition to the meager literature on the subject for it does suggest why presidents in different eras have felt it necessary to resort to secrecy and deception. Unfortunately, the book is less successful in outlining an evaluative scheme or usable guidelines.

Orman divides the reasons for presidential deception into four categories, defined in detail: personality, bureaucratic, pragmatic, and institutional. Using them, he examines Kennedy's program to undermine the Castro regime, Johnson's secret war in Laos, Nixon's actions against the Allende regime, and Ford's covert funding of the FNLA and UNITA in Angola. His analyses deserve to be read; for the most part, they are persuasive. One of the points he makes without being aware of it, however, is that it is not only the personality of the president that can be important to an analysis. Orman writes about the central role of Henry Kissinger in both the Nixon and Ford examples, but says that personality was not a factor in the Angola decision because Ford's was a "normal, well-adjusted presidential personality type" and he had "the final say" over foreign policy (p. 155). Without joining the current movement to equate Kissinger with Rasputin, it is still possible to raise the question of the extent to which any president, however mentally sound, is dependent upon his advisors in various areas, and to what extent his final control of foreign-policy decisions can negate the impact of an advisor's advice and personality. No president can be better than his information system; no information system is personality-free. Kissinger's personality may have been as important to Angola as Ford's.

Orman's analytical categories work well, even if one might quarrel with specific applications. What he meticulously describes as his "value framework" does not. Orman argues that illegal, unconstitutional, or unethical acts are never justified, but may be necessary; that means and ends must be compatible; that the president should tell the truth whenever possible; and that he should favor open over secret alternatives. The difficulties here should be obvious. One example: Orman, assessing the four examples of presidential deception according to this value framework, concludes, amazingly, that Ford's covert Angola policy was acceptable: it was not illegal, unconstitutional, or unethical. He admits that Ford "was not behaving consistently with the intent of the congressional measures that required consultation with Congress"; nonetheless, Orman finds Ford's actions ethical because Ford "believed he was acting consistently with his responsibility . . . his duty . . . and his discretionary power" (p. 185). That makes nonsense of any attempt to delineate limits to presidential secrecy and deception. The ability of presidents to delude themselves about the ethics of their behavior must be evident to anyone who has read through Johnson's or Nixon's memoirs.

The ability of political scientists to delude themselves that all political action can be segmented into neat scientific categories is equally great. Orman's suggested guidelines for presidential behavior are admirable. However, just as the generalized language of laws cannot cover all situations, so also Orman's or any other criteria cannot foresee all possible circumstances or be used as strict guidelines for presidential action; the interpretation and application is all. Were presidents to follow the *spirit* of "Orman's law," there is no question that there would be less presidential secrecy and deception. The same result would be achieved, however, if presidents followed the spirit of the Constitution.

Philippa Strum

Brooklyn College, City University of New York

State Income Taxation. By Clara Penniman. (Baltimore: Johns Hopkins University Press, 1980. Pp. xiii + 292. $22.50.)

It has been 20 years since Clara Penniman and Walter Heller wrote their first volume on the administration of state income taxation. In the interim, studies of tax progressivity, income redistribution and tax efficiency have littered the literature on state income taxation. Penniman's latest work on state income taxation is a relief from these often repetitive and narrow economic studies. Returning to more solid ground, Penniman directs our attention to the subtle effects of organizational structure on the operation of state income tax.

Penniman sets out to examine the organizational structure and operation of state income tax offices. Maintaining that organizational capacity structures administrative efficiency, Penniman presents evidence that confirms the wide variation in structure, management, processing and auditing operations of state income taxes. The text begins with a historical description of the state income tax, establishing its fiscal and political importance to the operation of state government. A general typology of state tax administrative structure is outlined with the degree of structural integration as the central organizing concept. The virtues of integration and its accompanying benefits for the operation of tax systems are not overstated. Penniman notes that "over integration may defeat the values of specialized experience and knowledge that can produce a needed overall view of administration" (p. 100).

The remaining chapters examine the specific tasks associated with the operation of a state income tax. The organizational typology is applied as a means of making comparisons across states employing an income tax. At each stage of the process Penniman offers provocative explanations for the breakdowns and inefficiencies in the tax system that go beyond traditional cost-benefit analysis.

A separate chapter on intergovernmental relations underscores the reliance states must place on each other and the federal government in order to achieve an effective income tax. In spite of more conflictual state-federal relations, Penniman concludes that there is greater federal-state than interstate cooperation in the administration of tax collections. Presumably the federal government's dominance of the income tax forces states to rely on the Internal Revenue Service and its superior collection and auditing procedures. This issue is not fully explained and remains the only major weakness in an otherwise thorough review of intergovernmental relations and tax administration.

Students of public administration and public finance should find this text a primary source for the study of state revenue systems. There is, however, a potentially broader audience for this book. Policy analysts and innovation theorists should find Penniman's historical and comparative analysis on the evolution and diffusion of state income taxes enlightening. Penniman's own data suggest that adoption of income taxes—frequently studied by others as a policy innovation—has often been a function of revenue dependence (i.e., percent of total revenues raised from income taxes) and existing administrative structures.

Penniman's only failing is her reluctance to analyze fully her own questions and hypotheses. In many instances she avoids quantitative analysis of her own tables. The attentive reader, however, will note that many of the tables provide ample opportunity for further analysis. When examining the variation in the administrative structures of state income taxes, Penniman ignores the tables which show a regional (i.e., southern and southwestern) bias toward the use of integrated administrative structure.

Penniman presents a clearly written and well-documented description of income taxation in the American states. This volume outlines a research agenda which should occupy scholars for another 20 years.

ROBERT STEIN

Rice University

The Electorate Reconsidered. Edited by John C. Pierce and John L. Sullivan. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. 293. $18.95, cloth; $9.95, paper.)

Need it be said for the hundred-thousandth time that the papers in a collection such as this vary in quality? The present volume contains—out of 13 papers and essays—a couple of choice ones and several more which are solidly informative and useful. It is introduced by the editors' necessarily sketchy review of the mainstream of voter behavior literature.

Pamela Johnston Conover's paper on "The Perception of Political Figures" is rather marred by obviousness. She shows that people who were convinced that Nixon *was* involved in Watergate came to believe that he was the sort of person who *would* engage in that sort of activity. She tries to give theoretical import to her finding by the use of something called "attribution theory." Unfortunately, the predictions she deduces from attribution theory are either inconsistent with the data or identical with the predictions more conventional theories would produce. For example, she finds that those who were already most convinced of Nixon's culpability were the most attentive to media coverage of Watergate; a classic example of that old social-psychological warhorse, selective exposure.

Paul R. Hagner and John P. McIver tackle the question of response instability of survey respondents—the familiar "attitudes versus nonattitudes" problem of Converse, Achen, et al. They bring to the debate, happily, a new set of panel data, and an interesting suggestion: many "unstable" patterns of response are consistent with real attitude change. For example, a respondent may agree, agree, agree, and disagree over four waves of a panel. He or she may have simply changed his mind. However, the respondent also may be answering randomly. Hagner and McIver never properly deal with this latter possibility, and their data are quite consistent with a "black-white" model where 25-45 percent of respondents actually had an opinion, and the rest responded at random.

James Piereson, John L. Sullivan, and George Marcus reevaluate the classic findings on political tolerance by measuring intolerance, not merely toward left-wing groups, but toward right-wing groups as well. They show that earlier studies were in error in important ways—greatly overestimating the relative tolerance of the educated, city dwellers and non-Protestants.

In contrast to the psychological orientation of most of the essays in the volume, Herbert F. Weisberg's and Morris Fiorina's is an important contribution to rational choice theory. Such theory has usually posited that voters see candidates as occupying a single position on an ideological continuum—candidate A is seen as a moderate conservative. But suppose voters lack good information on candidate positions, and must view any candidate as a probability distribution—candidate A is likely a moderate conservative, possibly a moderate liberal, and just barely possibly a socialist? Orthodox theory suggests that voters would view such a situation as a lottery and vote for the candidate offering the largest expected payoff. Weisberg and Fiorina suggest another possibility: voters may favor the candidate offering the smallest probability of some unacceptable outcome. This theory of "defensive voting" is of course, an application of the "minimax regret" decision criterion. A silly contrivance when applied to voting turnout, the theory has plausibility here. Perhaps Goldwater lost, not because he advocated unpopular policies, but because he offered a perceptible probability of a nuclear war. The authors' attempt to test this theory, though ingenious, fails, partly because nobody has collected the appropriate data and partly because for the great majority of respondents this and more traditional theories yield identical predictions.

John Petrocik's concluding essay deals with the important controversy surrounding the "revisionist" view of the electorate presented in *The Changing American Voter.* As a co-author of that volume, Petrocik would be expected to rebut criticisms of it. He does so ably, and clarifies several issues in the process. Only a couple of methodological blunders mar the piece. Petrocik attempts to assess issue voting by regressing the vote on party identification, and then looking for an ideological pattern in the residuals. Elsewhere, he analyzes variance in the vote contributed by party identification, variance contributed by issues, and "shared variance"—the latter being a nonsensical category.

Space allows only the tersest mention of several other essays. Christine B. Williams considers problems of cohort analysis, particularly the difficulty of separating out cohort, aging, and period effects. The methodologically sophisticated will find little new here; others may find this a worthwhile introduction to the subject. Conover and Stanley Feldman deal with the question of ideology or "constraint" among issues. They argue that—rather than a single liberal-conservative continuum—multiple *schemata* serve to organize specific attitudes. They demonstrate, admittedly in a preliminary way, the utility of schemata in analysis of 1976 CPS data. W. Phillips Shively contributes a most valuable review of the literature surrounding the question: what does it *mean* to call oneself a "Democrat," "Republican," or "Independent"? Edward G. Carmines and James A. Stimson analyze racial attitudes, and show that they are very stable relative to other attitudes, and central to any structure of ideological constraint among the public. They also show a modest but significant realignment of party identification around racial issues in the 1960s.

Voter behavior specialists will find this volume worth the purchase price (in paperback). Others will benefit from selective reading herein.

JOHN MCADAMS

Marquette University

Presidential Decision Making: The Economic Policy Board. By Roger B. Porter. (New York: Cambridge University Press, 1980. Pp. xi + 265. $18.50.)

This book is a case study focusing on the structure and operation of an advisory body during the Ford administration, the Economic Policy Board (EPB). Roger B. Porter, formerly executive secretary of the board and presently an assistant professor of public policy at Harvard, chronicles its history, notes its functions, and details its organization. He presents three controversial policy issues (the 1975 State of the Union tax proposals, the U.S.-USSR grain agreement, and the 1976 footwear import decision) to illustrate the operation of the board and provide criteria for evaluating its performance.

But the book is also more than a case study. While it focuses on the EPB, the author is concerned with advisory processes in general. He places the EPB within a conceptual framework and even amplifies that framework in a concluding appendix on processes for decision making. Porter's objective, broadly conceived, is to examine and assess these processes in the light of the fragmentation of the executive branch and the interrelationship of policy issues.

He is successful. He has written a detailed institutional history, an insider's account of who said what to whom, when, and with what effect. Most of the book consists of a nuts-and-bolts treatment of the EPB. Its meetings, agenda items, even policy papers, are noted and illustrated with numerous examples. Roles of the principal actors are discussed and evaluated. The president's involvement is indicated and his economic decisions seen within the context of EPB advice. Porter describes an active, formal, systematized structure, one that operated to exchange information, to coordinate advice, to resolve interagency disputes, and most significantly to provide the president with a variety of policy recommendations within the economic sphere.

The description is rich but almost burdensome in detail. It is also curiously antiseptic. Very little of the excitement of presidential decision making is conveyed. On the contrary, the board's attempts to coordinate conflicting positions read as if they were sanitized (much as Ford's own memoirs read).

If judged by the performance of basic advisory functions, Porter believes "the EPB was a success" (p. 212). He argues that it provided the president with an early warning system, helped define critical issues and focused administrative attention on them, worked to mobilize information and analyze policy, and presented a range of options for decisions. Most importantly, it did so within an advocacy system characterized by due process. In the light of the previous administration's less systematized and more hierarchical economic advising structure, Porter suggests that this was no small accomplishment.

The weaknesses of the board are mentioned, but quickly dismissed. The time spent on trivial matters, the delays in reaching policy judgments, the undercutting of cabinet secretaries in their areas of responsibility, are seen as minor problems. Porter concludes with an unqualified endorsement for the board from the president and his aides:

> Gerald Ford described it as "the most important institutional innovation of my administration." Indeed, the EPB not only had the reputation of being the most effective policy council in the Ford period, but also the most sustained, comprehensive, and successful collegial attempt ever to advise a president on economic policy matters (p. 212).

Porter and his colleagues, of course, cannot be considered impartial observers and judges. As executive secretary of the EPB from 1974 to 1977, Porter contributed significantly to its daily operations. The extent to which his service on the board affected his own observations and evaluations and his interviews with other key participants is difficult to assess from the outside. What is clear, however, is that despite the outstanding grades which the Ford administration gave its EPB, President Carter chose not to continue it, although, like Ford, he too desired a cabinet approach to policy making; he too wanted advocacy with due process.

Roger Porter has written a valuable work for students of the presidency. It is conceptually sophisticated and empirically useful. Scholars should benefit from it regardless of whether they accept the author's appraisal of the board's work or his prescriptions for organizing economic advice to the president.

STEPHEN J. WAYNE

George Washington University

Case Selection in the United States Supreme Court. By Doris Marie Provine. (Chicago: The University of Chicago Press, 1980. Pp. ix + 214. $18.00.)

Doris Marie Provine has written one of the most authoritative accounts to date of the process through which the justices of the U.S. Supreme Court decide whether to consider cases on their merits. Considering both macro- and micro-level implications of these important decisions, she exhaustively and creatively exploits the unique data set created by Justice Burton's compulsiveness. And because it is impossible to doubt the importance for legal and public policy of this stage of the decision process, her work stands as an important contribution to the understanding of policy making by the Court.

In the early chapters of her book Provine provides an historical review of the efforts of the Court to maximize its discretion over deciding which cases to hear. Basically she argues that the Court has purposely adopted ambiguous standards for case selection (e.g., Rule 19) in order to avoid any constraints on its discretion, and to reinforce diffuse support for the institution by maintaining the myth that citizens can ultimately have their day in the highest court in the land. A latent effect of this ambiguity, however, is the substantial advantage that accrues to what Galanter has called "repeat players." Great discretion also allows the Court to manage cases which might otherwise threaten the prestige or political stability of the institution, as, for example, her analysis of major race-discrimination cases from 1946 through 1957 reveals. The historical and logical analysis of the first two chapters of the book make interesting reading.

In chapters 3 through 5 the author considers the determinants of case selection decisions. The Burton data are used to reexamine the utility of cue theory (as developed primarily by Tanenhaus and Ulmer). Provine basically rejects cue theory, arguing that it cannot accommodate the finding that factors determining the decision to "special list" a case also control voting decisions after discussion. In its place she argues for a process of weighting the "salient aspects of all petitions" (e.g., case subject matter and litigant expertise). Most important, however, seems to be the justices' shared ideas about the proper work of the Supreme Court. "A justice who stresses the obligation of the Supreme Court to correct lower-court mistakes, for example, will vote for review when a justice with the same interpretation of the results, but a different view of the responsibility of the Court, will not. . . . Even the shared goals for Supreme Court adjudication . . . can occasion disagreement in case selection because the justices can be expected to differ in how they evaluate specific cases in light of these goals" (p. 105). Provine divorces the propensity to vote for review from the ideology of the justice, arguing instead that the propensity flows from role conceptions or orientations. Judicial attitudes affect these votes only indirectly. The primary conclusion of her analysis is adequately summarized as follows:

> The Burton data suggest that Supreme Court justices are not ordinary political decision makers, because members of the Supreme Court share a conception of their role which prevents them from using their votes simply to achieve policy preferences. Thus we have seen that none of the Burton-period justices either combined forces to achieve the results they preferred on the merits, or voted individually in a way that would suggest routine calculation of probable outcomes in the case selection process. Nor did the Court as a whole appear to make the calculation of political harm to itself central in even the most politically volatile case selection decisions (pp. 174-75).

One can quibble with portions of the analysis, of course. Her statistical analysis and procedures are seldom rigorous, and the unit of analysis is not always optimally suited to addressing the issue under consideration. Too little attention is given to the matter of the relationship between role orientations and ideology. As role orientation is the major independent variable in her model, and as variability exists, some discussion of the origin of these beliefs seems in order. The implications for generalizability of the uniqueness of the data base, the limited time period analyzed, and the restricted range of major environmental variables have also been ignored.

More importantly, while the empirical analysis of this important data set is descriptively interesting, little theory is developed or tested. For instance, the author skirts the boundaries of role theory on several occasions but is never quite willing to consider a full-blown role-theory treatment of her subject (and this despite her belief that "the self-imposed limits of role conceptions" constitute one of the most important parameters of the model). Similarly, while her analysis of the manipulation of symbols by the Court is insightful, much would be gained by placing the analysis within the context of theories of support. The analysis of decisions by justices ignores the complex and theoretically sophisticated model developed by Ulmer. Indeed, little attention is given to Ulmer's recent work. Provine considers, for example, the question of strategic or outcome-oriented voting in FELA cases from the perspective of Schubert's early 1960s game-theory approach rather than from Ulmer's more recent (e.g., *APSR,* 1978) and more subtle theory. More self-conscious attention to theory would have substantially improved the analysis. As a consequence, these data will only fully be exploited as they become the grist for another theoretician's mill.

As a handy descriptive reference work on access decisions, this book will be useful to many. As a theory of judicial decision making, its utility is somewhat limited.

James L. Gibson

University of Wisconsin, Milwaukee

Old Folks at Home. By Alvin Rabushka and Bruce Jacobs. (New York: Free Press, 1980. Pp. vi + 202. $10.95.)

Adequate housing ranks along with income security, nutrition, health care and physical safety among the basic needs of this country's old-age population. A number of government programs have been developed to assist elderly people in meeting their housing needs, and yet for the most part policy in this area has evolved with no very substantial empirically based understanding. There has been only limited knowledge of the actual physical status of housing for the aged and little grasp of how old people themselves feel about their residence and its physical condition. Leading authorities in the housing field have underscored the scarcity of data concerning how most elderly people live and their living arrangements, and have worried over the tendency for policy to be decided essentially on the basis of conjecture. The knowledge gap probably has contributed to the absence of notable success in government housing policies for the elderly.

Compounding the difficulty has been a tendency for certain of the more widely read books in the field of social gerontology—including at least one leading textbook and a Pulitzer Prize-winning book intended for general readers—to come dangerously close to the assertion that old age is synonymous with poverty. The implication is left that the aged on the whole are also poorly housed, since income levels and housing quality are closely connected.

Against this background, the research findings reported by Rabushka and Jacobs represent a valuable corrective, and their analyses and interpretations amount to what is probably the most balanced picture of the present situation. The authors' interest centers on the 70 percent of elderly Americans who own their own homes. (The tenants who comprise the remaining 30 percent are excluded from discussion.) Their data were derived from a survey of 1,575 older-aged respondents situated in seven communities chosen as reflective of the diversity of the United States in terms of size, region, climate, race and culture.

The findings do not fit easily into either the preconceptions of those on the political right, convinced of the potential of the private market to generate adequate housing for old people at acceptable costs, nor of those on the liberal left, who are inclined to attack housing needs through direct subsidies to elderly homeowners and other governmental assistance based on an age criterion alone. The "free market" proponents will have difficulty reconciling their stance with Rabushka's and Jacobs' finding that fully 60 percent of elderly homeowners live in a residence with at least one significant "system defect," meaning a lack of either (1) some basic facility such as a toilet or working stove, or (2) a significant health or safety hazard, or (3) some long-term repair need such as a ceiling or wall fixture in major disrepair. Even if one confines attention to the second of the above categories, namely hazardous and dangerous features that directly threaten the safety of occupants or render the house uninhabitable, it was found that 40 percent of surveyed homes had at least one defect of this sort. Many had two or more. On the other hand, liberals will find it sobering to contemplate the authors' findings regarding the resistance of most elderly homeowners to direct government aid or subsidy, given the latter's fierce sense of pride and independence. Notwithstanding the prevalence of serious "system defects," elderly homeowners are satisfied with their house (96 percent), have no desire to leave it for a condominium offering more amenities and no structural defects (93 percent) and are content with its size, even though the current household may consist of only one or two people occupying a dwelling large enough for four, five or more. Even where serious defects exist—including ones directly endangering health and safety—elderly homeowners express decided satisfaction with their residence, being either unaware of the existence of the defect or not perceiving it as a serious impediment to their living arrangement.

This lack of congruence betwen objective reality and subjective feelings on the part of homeowners—termed by the authors as the "needs/preferences mismatch"—carries with it a major public policy challenge. Rabushka and Jacobs are not opposed to government housing programs of all kinds, but they are disturbed by the premise that the gap between accepted standards of housing decency and the elderly's actual housing situation can be filled only by direct public intervention and regulation. They see the government's proper role as one of facilitator—aiding homeowners to perceive housing defects where they exist and then enabling them to correct problems with property tax deferrals, "reverse annuities," government-underwritten loans, and so forth.

Particularly disturbing is the image presented in the book of distress among elderly black homeowners. While a number of factors—age of structure, and homeowner wealth, income and health status—are revealed as moderately associated with the presence (or absence) of critical structural defects, these pale in significance compared to homeowners' race. Black homeowners have four times the number of critical structural defects in their houses as whites; 95 percent of homes owned by elderly blacks have at least one critical defect, as against only 44 percent among their white counterparts. Black/white income differentials obviously account for some of this disparity, but less so than one might expect. "White" houses are better maintained, even with income held constant. The authors speculate that black homeowners still suffer restrictions and discrimination in their ability to buy repair and maintenance services, and also that differences in taste or preference along race lines may be involved.

The book is not without its flaws. As noted, Rabushka and Jacobs avoid any treatment of housing conditions among the 30 percent of the elderly not in the homeowner category. From the standpoint of understanding the housing needs of older people generally, this gap in the evidence represents no small problem, especially since earlier research has revealed that renters and homeowners comprise distinctly different categories of the elderly population.

There is also an unsettling difference in the general mood conveyed by the book's opening two chapters and that of the closing three. The opening section strongly emphasizes the need to challenge so-called "experts" in their view that old-age dependency and severe housing needs are more prevalent than is commonly recognized. Against this, the authors assert that "the touting of crises create[s] an impression of a large number of problems and current needs of the elderly. But this impression applies only to a small number of the elderly and an even smaller number of homeowners" (pp. 12-13). And again, in remarking on the dismal image conveyed in the mass media of elderly people confined to "rural shacks, nursing homes and overcrowded hotels," the authors affirm that "elderly homeowners, in particular, enjoy a standard of living and personal independence unknown to prior generations" (p. 33). All this does not quite prepare the reader for the subsequent disclosure that, contrary to observers who maintain that "America has almost met the goal of a decent house for all Americans" (p. 92), the actual extent of deteriorated housing among the elderly is rather high as measured by professional standards of housing quality. Even though many of these defects are not perceived as problems by homeowners, they are nevertheless *real* problems, both for the individuals involved and for local communities. As the authors remark somberly toward the end of the book, "A deteriorating stock of elderly-owned housing *does* raise serious issues for national housing policy involving not only the well-being of the elderly but also affecting millions of their tax-paying neighbors" (pp. 131-32).

Despite the flaws, however, Rabushka and Jacobs have made a useful contribution to the public policy literature on housing. Their book will be read with special interest by those interested in the social and economic needs of the elderly and in what lawmakers and public officials can do to alleviate those needs.

HENRY J. PRATT

Wayne State University

Congressional Odyssey: The Saga of a Senate Bill. By T. R. Reid. (San Francisco: W.H. Freeman, 1980. Pp. xi + 140. $4.95.)

The legislative case study is a potentially important but not particularly fashionable genre of literature for contemporary congressional scholars. One can count on the fingers of two hands the number of such first-rate case studies since World War II. These studies tend to be explicitly atheoretical, although they are replete with the raw materials useful for the development of broader verbal theories of the legislative process. T. R. Reid's volume is a small gem which should be received gratefully by congressional scholars and their students, for it represents a fine addition to the case study "tradition." The book is the polished end-product of a series of *Washington Post* articles during the 95th Congress which were intended to educate readers on the intricacies of getting a rather unglamorous piece of legislation enacted. At first glance, there would seem to be nothing captivating about Republican Senator Pete Domenici's struggle to establish a waterways user charge upon the U.S. barge industry; the bill in one form or another had been a doomed "hardy perennial" for decades. But first glances are often misleading, and as Reid tells the tale it becomes enormously fascinating and instructive.

Among the themes which receive skillful treatment in the study are the importance of strategy, the crucial role of staff, the interplay of personalities (best illustrated by certain of Domenici's actions and the extraordinary maneuverings of Senator Russell Long), the triangular relationships among legislators, executive branch personnel, and interest-group representatives, the occasionally baffling exercise of parliamentary ploys and prerogatives, the nature of committee and party leadership, and—not least—the force of ideas. Although Reid never brings together the pieces of the legislative process puzzle in a consciously theoretical manner, his narrative is so clearly focused and judicious that even a relatively unsophisticated reader will emerge with a good sense of how legislation gets made. I know of no other book of its ilk which so adeptly orders congressional complexity in so short a space. For this reason, its classroom appeal is considerable.

This is not to say that the book is unflawed. In spots it gives us a quick shuffle on technical and parliamentary matters, and it occasionally avoids explanation or speculation about participants' motives or strategies. Readers who have had the opportunity to be participant-observers on Capitol Hill may well conclude that Reid somewhat underplays the tedium, frustration, and deadlock which can characterize the struggle for passage. However, one never gets the sense that important chapters in the story have been missed.

Another issue which deserves mention is the possibility that Domenici's bill is a case study in the operation of a "Hawthorne effect." The extensive *Post* coverage of the legislation vaulted what might otherwise have been an obscure bill into a position of much higher priority on the agenda of official Washington. We can only wonder how different behavior and outcomes might have been if the newspaper had not anointed the waterway user's charge bill as one worth following.

This book shows that there may be real benefits to be derived from future collaborative efforts between journalists and political scientists in the execution of congressional case studies. Only through the disciplined accumulation of case study evidence drawn from a variety of types of legislation will it be possible to generate a body of grounded theory. And only by generating such theory will we come to know why Congress makes policy as it does. Whether the interests and talents of journalists and scholars can be made to mesh remains to be seen.

CHARLES M. TIDMARCH

Union College

The Politics of Privacy: Planning for Personal Data Systems as Powerful Technologies. By James Rule, Douglas McAdam, Linda Stearns, and David Uglow. (New York: Elsevier, 1980. Pp. xii + 212. $14.95.)

Personal privacy is an elusive albeit primary value in a free society. In the twentieth century, privacy emerged as an important issue in American politics, reflected in mounting litigation and legislation. Although the Constitution does not expressly guarantee a right of privacy, the Supreme Court broadly interpreted the First, Fourth, and Fifth Amendments to safeguard some privacy interests. More dramatically, in a herculean exercise of judicial activism in *Griswold* v. *Connecticut* (1965), the Court fashioned an independent constitutional right of privacy. The denomination and subsequent expansion of the right under the Fourteenth Amendment invited further litigation, as well as engendered considerable controversy over the nature of privacy per se and the basis for, and the necessary limits to, claims of a constitutional right of privacy. The growing concern over achieving protection for personal privacy in a technological and information-oriented society also prompted congressional and presidential action. Numerous study commissions were established and Congress enacted the omnibus Privacy Act of 1974, along with other legislation designed to safeguard citizens' informational privacy interests in financial, medical, educational and criminal records. The political salience of safeguarding privacy interests was further registered by the increasing number of states adopting privacy statutes.

James Rule and his colleagues, all sociologists, purport to give a comprehensive examination of the privacy controversies arising over public and private organizations' reliance on computerized information systems. They also offer a critique of the incremental development of federal privacy protection politics and, thereupon, an alternative proposal *à la* solution to the dilemma of preserving privacy in an information-oriented society. Unfortunately, this authors' description of the informational privacy controversy is shallow and their prescriptions neither well-thought-out nor particularly illuminating.

The authors' discussion of the emergence of privacy as a political issue is analytically and descriptively weak. The authors attribute the controversies in the 1960s and 1970s to the social structure of an advanced industrial society in which complex organizations place a premium on gathering personal information about their clients. Beyond such sweeping generalizations, however, the authors fail either to ground their analysis in the distinctive features of the American political process or systematically to address the dynamics of the formulation and articulation of privacy protection as an issue worthy of political actors' attention and policy making. No attention is given to the import of privacy litigation—constitutional or other varieties—and the book contains only a superficial discussion of what the authors call "official" responses to privacy controversies. For example, a meager chapter of 2½ pages is devoted to the crucial provisions of the Privacy Act.

A comprehensive and detailed discussion of the development of privacy protection policies would have made a contribution to the literature. Moreover, such a discussion might make plausible the authors' conclusion that further legislation and procedural reforms are inadequate to safeguard privacy interests in computerized record systems. The authors are critical of present policies, as embodied in the Privacy Act, for balancing organizations' information needs with individuals' privacy interests. Indeed, the authors reject outright any balancing approach and find legal, procedural safeguards insufficient, since they are only "matters of social convention" (p. 141).

After a rather cavalier dismissal of public policies designed to accommodate organizations' legitimate information needs and individuals' privacy interests, the authors propose that *"the alternative to endless erosion of personal privacy through increased surveillance is for organizations to relax the discriminations which they seek to make in their treatment of people"* (p. 154, emphasis in original). If organizations were less discriminating in their treatment of individuals, then the demands for personal information would ostensibly drop and thereby enhance the prospects for personal privacy. The authors suggest, for instance, that simpler tax laws would decrease the amount of information required by the Internal Revenue Service and concomitantly diminish its intrusiveness into citizens' private affairs. Fundamentally, the authors are not so much concerned with privacy safeguards in an information-oriented society as dismantling the bureaucratic structure of complex organizations, substituting "rational planning" for incremental policy making, and cultivating what they "term a desireable 'looseness' in social relations" (p. 118).

The Politics of Privacy is a superficial work that neither clarifies the dynamics of the privacy issue in American politics and law, nor offers cogently argued policy alternatives.

DAVID M. O'BRIEN

University of Virginia

The American Establishment. By Leonard Silk and Mark Silk. (New York: Basic Books, 1980. Pp. xi + 351. $13.95.)

The term, the "American Establishment," has been the subject of much speculation over the years, particularly since the publication of Richard Rovere's brief article in the *American Scholar,* which was reproduced in a widely read book in the early 1960s. Yet, curiously, there has been no in-depth analysis of this powerful complex by either serious researchers or more popular writers. Now *The American Establishment,* a study prepared by the *New York Times'* Leonard Silk and his son Mark, has attempted to fill this void.

How well have they succeeded? Not too well, judged by academic standards. This book is basically a journalistic effort which, while well written, relies heavily on an unspecified number of personal interviews. It is consequently a very mixed work. To their credit, the authors focus on selected key components of the American Establishment—namely, Harvard University, the *New York Times,* the Ford Foundation, the Brookings Institution, the Council on Foreign Relations, and, though they are dealt with in much briefer fashion, the Council for Economic Development (CED), the Business Roundtable, and the Trilateral Commission. As a result of their research, the Silks have come up with some revealing insights concerning these important bodies.

But, unfortunately, their treatment of these institutions is generally superficial, often consisting of little more than interesting anecdotal accounts of recent major events. The Silks' book also contains many misleading statements (see page 314 for Richard Nixon depicted as a populist!) and has serious gaps. For example, with regard to Harvard, the authors concentrate primarily on the school's social and religious life and its substantial public service contribution to American government. However, one might ask, what about the role of the Harvard business and law schools as trainers and suppliers of talent to the nation's large corporations? And/shouldn't Yale and Princeton have been included as almost equally influential institutions? Similarly, serious questions can be raised about the authors' treatment of the other civic and economic bodies described in this work (for a much fuller and more accurate account of the origins of the CED, see Robert Collins' article in the Autumn 1978 issue of the *Business History Review*).

Another even more grievous deficiency in the Silks' analysis of the American Establishment is the fact that it grossly underestimates the role played in this complex by major corporate interests. The pro-business makeup of such groups as the Council on Foreign Relations is simply glossed over by the authors in their attempt to portray it as an essentially high-minded, though sometimes divided, civic group deeply concerned about the nation's security and diplomatic affairs. No mention is made of the fact that almost all of the men who have served as president of the Council on Foreign Relations in the postwar period have had important corporate directorship ties. (Also, for a broader look at the recent economic links of many of this organization's directors, see the latest edition of Thomas Dye's *Who's Running America?*) And for some reason, the Silks fail to treat one of the often overlooked key arms of the American Establishment, the Business Council (or as it was known up to 1961, the Business Advisory Council). Above all, they do not show how the institutions they describe fit into the overall structure and operation of the American Establishment; each is analyzed as a separate unit. To give just two examples, this book does not delve into any of the links which exist between the Business Council and CED and later the Business Roundtable, or, in the diplomatic realm, between the Council on Foreign Relations and the Trilateral Commission.

Yet another closely related failing in the Silks' work is their distinctly conservative view of the role of the American Establishment as an essential "third force" in the country, one outside of business and government (though with links to both), dedicated to promoting truth, liberty, and, to use that elusive phrase, the "public interest." They also claim, toward the end of their book (see p. 326), that the Establishment has acted as a mediating force between "rich and poor, black and white, management and labor, industrialists and environmentalists, hawks and doves," or, as they put it elsewhere, between capitalism and democracy. This is an extremely naive and misleading position for two supposedly well-informed writers to take, for, although certain large concerns have acted as "outsiders," big business is an integral part—in fact, some would say the very heart—of the American Establishment.

Thus perhaps the best judgment one can make about *The American Establishment* is that while it clearly represents a contribution to the sparse literature on the subject, one can only regret that the authors did not do a better job of analyzing this intriguing topic. (It certainly does not measure up, in terms of economic analysis, to Peter Newman's *The Canadian Establishment.*) In short, a sound study of the American Establishment has yet to be written.

PHILIP H. BURCH, JR.

Rutgers University

The Prospective City: Economic, Population, Energy, and Environmental Resources. Edited by Arthur P. Solomon. (Cambridge, Mass.: MIT Press, 1980. Pp. xx + 491. $27.50.)

Arthur Solomon and his colleagues have provided an up-to-date review of the socioeconomic trends shaping American cities. This is a valuable service, if for no other reason than that nonspecialists now have much useful data analysis conveniently collected in one place. The papers are authored by many of the best-known students working in the urban field, including William Alonso, Thomas Pettigrew, Ira Lowry, Bernard Frieden, Roger Vaughn and George Peterson. The papers by Vaughn and Peterson are particularly interesting as both attempt to establish the considerable impact federal policies have had on urban form and economic vitality. For those who still doubt it, they provide overwhelming evidence that any attempt to explain the significant features of cities, including the policies they pursue, without reference to national politics and policies is not likely to be a rewarding enterprise.

Two other themes, which are the subject of a good deal of present controversy, are also taken up by several of the papers. Among others, Alonso, Lowry and Franklin James consider whether the rehabilitation of older central-core neighborhoods by young professionals will alter the economic and social prospects of cities. All of the authors appear to be suspicious of the more sweeping claims made by editorial writers concerning a comeback for cities (once again!). No one who has ever actually traveled around a larger older American city and observed the comparative size of the "gentrified" neighborhoods ought to be surprised at the critical view taken in these papers. Several of the papers, particularly the one by Harvey Garn and L. C. Ledebur, also illuminate further the question of regional variations in the economic and social well-being of cities. Here again is a warning against unrewarding approaches to the study of cities. The Garn-Ledebur findings demonstrate that significant regional variations exist and any urban analysis must have this as one of its central propositions.

With all the useful data and discussion in this volume, there is one thing missing: politics. Most of the authors appear to be writing of that mythical land where the most serious matters are the policy alternatives to be given decision makers. Whether decision makers have the remotest incentive to listen to the policy proposals or to consider the data on which they rest is not discussed. Moreover, if indeed such decision makers thought it politically valuable to try yet another urban policy, the question must be raised whether it could in fact be sustained over time, given the present institutional and political arrangements. However, behind all this is a more serious question altogether. What do we want cities *for?* Is an urban policy merely one more instrument (or set of instruments) interchangeable with many others (poverty strategies or the latest member of the stable, viz. reindustrialization)? Is the only real question which one is most efficient, since none has any intrinsic significance? In short, are cities, and thus city politics and policies, particular and important elements of the larger regime? If they are, then a volume on urban problems without a discussion of urban politics is a peculiar enterprise at best.

STEPHEN L. ELKIN

University of Maryland

Seabrook and the Nuclear Regulatory Commission: The Licensing of a Nuclear Power Plant. By Donald W. Stever, Jr. (Hanover, N.H.: University of New England Press, 1980. Pp. xv + 248. $15.00.)

As the title suggests, this book is a case study of how one of the most powerful regulatory agencies ever created carries out its responsibilities. As the site of the first serious anti-nuclear opposition to an American nuclear plant, Seabrook, unlike the Three Mile Island (TMI) facility, was controversial before it was built. Like TMI, Seabrook raises important questions about the Nuclear Regulatory Commission's (NRC) powers and procedures.

Donald Stever, formerly the assistant attorney general of New Hampshire, seeks not "so much to indict the NRC as to understand it" (p. xi). He methodically examines the commission's practical problems and legal techniques involved in site selection, financial qualification criteria, public safety, and emergency planning. As such, this treatise is one of the first attempts to analyze NRC decision making outside the polemic of the antinuclear movement. The TMI accident confirms many of Stever's findings and the president's TMI Commission corroborates his suggestions for change.

Stever's thesis is based on "the premise that siting decisions should be made in the public sector

. . . [and] that . . . siting laws . . . which leave issue development to . . . an adjudicatory hearing held long after the utility has invested many thousands of dollars to justify it, are less than adequate to the task" (p. 29). In developing this argument, he traces the controversy from the time of its inception, and it is convincingly demonstrated that the NRC routinely licenses plants on extremely thin financial, safety, and environmental evidence.

Seabrook and the Nuclear Regulatory Commission shows, for instance, that neither state nor National Environmental Policy Act review has a significant impact on the choice of sites or the range of alternates considered. Further, it is pointed out that vendor safety information is taken on faith since the agency lacks capability to make independent evaluations. It is also useful to learn from an authoritative source that the time consumed in the licensing process is the result of the NRC's inefficient way of doing business, not the product of environmentalist delay tactics. Since the Seabrook decision presents an ominous precedent for the future, Stever hopes that it will become a watershed leading to a more responsible regulatory program for the simple reason that "the NRC does not undertake its role as a regulator with adequate objectivity, . . . does not listen to intervenor arguments, and deals with license applicants at less than an arm's length" (p. 168).

This book, then, is a worthwhile contribution to the literature, at least as far as it goes. However, from the perspective of many readers of this *Review*, its strength is also its weakness. Although the author is aware of the political implications of this subject, much of the book reads like a legal brief replete with descriptive marginalia and conclusory statements. It is not until page 97 of the 169 pages of text material that the dominant social issues in the debate (e.g., the need for power) are spelled out. Civil disobedience, which transformed Seabrook from just another contested licensing case to a national symbol of the future of nuclear power, receives just four pages of discussion as an afterthought at the end of the book. A recent example of a much more satisfactory analysis of the conflict of political actors, their values, and the use of scientific evidence is found in Robert V. Bartlett, *The Reserve Mining Controversy* (Indiana, 1980). Nonetheless, Stever has presented an instructive, if limited, critique of the competence of the individuals and organizations charged with the responsibility of regulating the nuclear industry.

JAMES S. BOWMAN

University of Wyoming

Twigs for an Eagle's Nest: Government and the Arts, 1965-1978. By Michael Straight. (Berkeley, Calif.: Devon Press, 1979. Pp. 181. $5.95, paper.)

Michael Straight is a new type of government official: the cultural bureaucrat. As deputy chairman of the National Endowment for the Arts from 1969 to 1978, he helped guide that agency through a period of growth that has made it one of the great bureaucratic success stories of the past decade. But the very success of the Arts Endowment has emerged as a political issue. The advocates of public culture were once anxious to convince elected officials that supporting the arts was not an entirely frivolous use of the taxpayers' money. Now cultural bureaucrats are often at pains to assure the arts community that public funding is not a threat to aesthetic values or artistic autonomy. The Endowment has had to defend itself from charges that its effort to democratize culture is, in fact, a policy of cultural populism that would abolish aesthetic standards and artistic excellence. That some of these attacks come from within what could reasonably be considered the "cultural constituency" is particularly disturbing for the Arts Endowment.

Twigs for an Eagle's Nest is a book of personal reminiscences. While largely anecdotal, it does offer the kind of participant perspective that can provide invaluable insights into the policy-making process. As the second-in-command under the very aggressive and visible Nancy Hanks, Michael Straight was perhaps ideally situated to write a bureaucratic memoir. We also learn about the issues of cultural policy making with which the Endowment was faced in those formative years. First, there was the great fear that some Endowment-sponsored project would scandalize the public—or worse, a member of Congress. Second, there was a more positive belief in the principle of "cultural egalitarianism"—that "no church, no aristocracy, no elite of any kind was able to impose its own aesthetic standards upon the nation" (p. 54). Third, there was a commitment to government "non-intervention" in culture through the use of advisory panels—presumably composed of disinterested experts on artistic excellence.

Each of the issues indicated above were then, and continue to be, highly debatable. One can be so fearful of possible adverse public and/or congressional reaction to government-funded cultural projects as to bring about a form of self-censorship. Such a censorship can discourage the Endowment from sponsoring work that might be judged hostile to prevailing orthodoxies—aesthetic or political. Also, cultural egalitarianism can be the foundation for a broad-gauged and comprehensive policy of public culture. It can also be a justification for funding artistically mediocre if politically attractive grass-roots organizations at the expense of established standard-setting cultural organizations. Last, Straight's case for cultural non-intervention rests on a too-uncritical reliance on the panel system without analyzing the potential for bias inherent in panel members and their criteria for judgment.

But, as befits a memoir, Straight's look is longer on recollection than analysis. For the increasing number of political scientists who study cultural policy making, *Twigs for an Eagle's Nest* is invaluable as one of the only accounts written by a top NEA official. It is a good complement to Dick Netzer's economic analysis of public culture in *The Subsidized Muse* (Cambridge, 1979). For students of public policy making in general, Straight provides a good "insider's" perspective on managing a successful but controversy-prone public agency.

KEVIN V. MULCAHY

Louisiana State University

The Zero-Sum Society: Distribution and the Possibilities for Economic Change. By Lester C. Thurow. (New York: Basic Books, 1980. Pp. v + 230. $12.95.)

American society has been paralyzed by the responsiveness of government to securitarian demands. "As we deliver economic security, we undercut the implicit assumptions of capitalism, democracy, and individual initiative" (p. 24). Solutions exist for all our problems—energy, inflation, over-regulation, declining productivity and growth. But every solution imposes a cost on somebody, and all solutions face organized or potential enemies. Deregulation of energy prices would unjustly redistribute income. Unwilling or unable to meet foreign competition, both managers and workers go protectionist, as in the case of steel and autos. No general will, no cumulative consensus, any longer exists to forge policy in the public interest. Nor is our political system able to deal with the paralyzing vested interests. Hence frozen pluralism is the essence of Thurow's diagnosis of the American malaise. His prescriptions for our condition command attention, for they offer a unique amalgam of competitive and egalitarian policies.

"If we are to establish a competitive economy within a framework of international trade and international competition, it is time to recognize that the techniques of the nineteenth century are not applicable in getting ready for the twenty-first century" (p. 145). Anti-trust actions are far less effective than exposure to foreign competition as the way to enhanced efficiency and welfare. Thurow advocates that we rely on market forces as far as possible. "Basically, price controls should be avoided whenever society is addressing any long-range problems" (p. 140). He is quite willing to deregulate the energy industry so long as we compensate those whose incomes would otherwise decrease severely. Indeed, he advances as a general principle that we compensate those individuals dislocated by economic progress, but not firms that should go under. And he recommends that compensatory programs be generous, even overly generous, anything to break the ice of securitarian paralysis. He further proposes that we get rid of the corporate income tax and establish a rational and fair tax system. Rather more controversial is his notion that we need the national equivalent of a corporate investment committee to help redirect investment from "sunset" to "sunrise" industries. Here he seems to be influenced by what may well be questionable interpretations of the dynamism of "Japan, Inc."

When Thurow is thinking competitively, he comes through as thoroughly un-tenderminded. "Man is an acquisitive animal whose wants cannot be satiated. This is not a matter of advertising and conditioning, but a basic fact of existence" (p. 120). He has nothing but contempt for those who think we could live with a zero-growth economy. Above all, "We need to start over and see what a modern economy needs in order to remain a competitive economy" (p. 150).

The other Thurow proposes to galvanize the economy by an injection of egalitarian justice. We must achieve a common conception of justice if we are to say upon whom and to what extent losses should be inflicted for the sake of the common good. And for him economic justice means more equality. Income transfer is not the method by which this should be brought about. Rather, "The basic problem is to change the structure of the economy so that the entire economy generates the kinds of jobs that are now open to white males and ensures that there are enough of these job opportunities to go around" (p. 203). This requires creation of a "socialized sector" that would operate on the basis of the earnings and differentials, a five-to-one ratio of average earnings of highest-paid quintile to average earnings of lowest-paid quintile that obtain in the "primary economy," now the preserve of white, fully employed males.

As I understand the reasoning behind this dramatic proposal, it goes as follows, not all of which Thurow makes fully explicit. In his *Generating Inequality: Mechanisms of Distribution in the U.S. Economy* (Basic Books, 1975), Thurow explained how "instantaneous fortunes" are created by disequilibria in the capital markets. They are really "prizes" in an economic "lottery." He now refers to our whole economy as a "natural lottery" (pp. 200-01), a game to which white males have privileged access. Now if indeed we are running a lottery, then we should give everybody a chance by starting up another in a "socialized sector." This idea that the economy is essentially a game of chance would seem to account for Thurow's rather odd understanding of "equal opportunity." "Presumably this phrase means that each individual should have an equal chance for economic success" (p. 195). Ordinarily, equality of opportunity refers to fair competition among those equally endowed and motivated, not to a game of chance in which all participate.

Given that Thurow's "socialized sector" is not a Civilian Conservation Corps, what exactly would go on in it? He speaks of "projects" and mentions two by way of illustration: street-cleaning projects in competition with municipalities and road-bed rebuilding in competition with private railroads. Such work is to be paid at the rates that obtain in the "primary economy." This all makes sense only if the connection between pay and performance may be broken with impunity or if the connection is at best tenuous, perhaps even nonexistent. Thurow's "lottery" conception of the economy suggests that the latter is his fundamental conviction. If he is right, then of course our economy is far less a domain of rational activity than it has been portrayed by most economists, for example, Sir Henry Phelps Brown in *The Inequality of Pay* (Oxford, 1977).

Has Thurow produced a coherent theory of political economy? No doubt many will find appealing his grand strategy that egalitarianism is the key to economic dynamism. But on Thurow's own terms, does the world really work like that? On the one hand, he sees us as economic men, and he is all for competition. On the other, he asserts that we are not highly sensitive to financial incentives, willing to work hard at differentials of five to one, and this would seem to imply that we are natural egalitarians. He does not attend to recent Swedish and British experience suggesting that equalization both reduces and misallocates effort. Nor is it easy to see how Thurow's economic men could reach agreement on a substantive conception of justice. Even John Rawls shys off from that, leaving economic outcomes to be shaped mainly by just institutions and procedures.

Nevertheless, Thurow has made an imaginative contribution to the great debate over principles of political economy and justice. He agrees with Hayek that the defensively organized stand in the path of economic progress, and they both would foster competition. But Hayek thinks social justice is a mirage. To promote both efficiency and equality, Phelps Brown would improve people's ability to work; the connection between pay and performance is both rational and just. In *The Structure and Reform of Direct Taxation* (Allen & Unwin, 1978), J. E. Meade would eliminate high marginal rates of taxation at both ends of the income scale to reinvigorate the economy. All of these economists think that rationality rather than luck governs economic activity. This is the fundamental postulate that Thurow challenges.

JOHN W. CHAPMAN

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How to Limit Government Spending. By Aaron Wildavsky. (Berkeley: University of California Press, 1980. Pp. ix + 197. $8.95.)

Aaron Wildavsky's argument is straightforward: growth of government, particularly at the expense of the private sector, is not desirable. He argues that controlling the expansion of federal spending will reduce inflation, lower taxes, and enhance efficiency. An absolute limit on spending or a balanced budget is inferior to keeping the percentage growth of federal outlays to the percentage growth of the GNP. Implementation of this goal cannot be achieved by administrative regulation or statute. Instead, only a constitutional amendment is sufficiently powerful to compel all governmental or quasi-governmental actors to submit to the same discipline. Resource addition, the name of the present spending game, will be replaced by real resource allocation if a constitutional outlay limit is enacted.

This book exemplifies two facets of Wildavsky's approach to policy analysis. Naturally, there is the careful analysis of the policy being investigated: craftsmanship. But there is also salesmanship. As he says in his *Speaking Truth to Power,* "My personal preference . . . is to supplement knowledge with persuasion by actively helping policy ideas make their way in this world" (p. 10). By keeping that orientation in mind and realizing that *How to Limit Government Spending* is based on the 1979 Fels Lectures at the University of Pennsylvania, one can understand the polemic and almost conversational nature of the book. The witty, breezy, lively, forceful, and argumentative flavor dominates the analysis as well as the presentation. This does not mean the analysis is shoddy. It is good but not overwhelming. I am heavily swayed but not totally persuaded.

One of the major difficulties in analyzing proposed policies that are largely without precedent is that certitude about cause and effect is lacking. The analyst must go to analogs or relatives of the policy. Since the experiences in the states are the closest we have to the real thing at the national level, their success should weigh heavily in the analyst's mind. It is a tribute to Wildavsky's intellectual integrity that an excellent appendix by Naomi Caiden on problems in implementing expenditure limitations in the states is included. As I read it, this careful appendix is properly not nearly as optimistic about the practical matters of implementation and eventual success as Wildavsky hopes. The analogous cases make me wonder whether the proposal will proceed as the policy advocates suggest.

Wildavsky is less than forthcoming on just how the analysis will be carried out in the government as agencies compete for the marginal increases available to the government or fend off redistribution of the money already allocated to them in years past. Since policy analysis is often closer to an art than a science, it is unclear just how the analysis would be performed and used in the halls and offices of government. Wildavsky does predict that conflict within government will increase. But it is uncertain just how that will affect the delivery of services. More time and other resources may be devoted to competition within government than to actual production. His analysis is structural and macro-level. It should cover more at the micro-level.

Overall, though, this is a very provocative examination of a policy proposal that will be receiving more attention in the near future. It is well written and presents a reasonable analysis. It is worth reading. On the level of salesmanship and polemic, it merits 4½ stars. On the level of analysis, I'd give it 3 stars.

JOHN WANAT

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The Politics of Regulation. Edited by James Q. Wilson. (New York: Basic Books, 1980. Pp. xii + 468. $18.95.)

According to James Q. Wilson and the authors of the nine empirical studies of regulatory enterprises included in this volume, there is a politics of regulation. It is a politics quite unlike that based essentially upon the maximizing impulses of industries and officials featured in theories propounded by economists. It is also unlike that vision of regulatory politics most familiar to political scientists and associated with concepts such as capture, life cycles, iron triangles, and interest group liberalism. For somewhat different reasons, both of these perspectives assert that regulatory behavior is fundamentally a reflection of economic power. At least during the past decade, many developments in government regulation have been inexplicable in terms of either of them. Although this book is not the first effort to grapple with disparities between conventional theories and regulatory behavior and to examine critically the fundamental assumptions of those theories, it is an especially ambitious and successful one. The view of regulatory politics that results is of major significance and utility for students of regulation and of American politics in general.

The analyses upon which that view is developed in a concluding chapter by Wilson are of several varieties of government regulation. Although not written to formula, they are compatible in format and emphases, and most importantly in their common interest "in how goals were determined, conflict resolved or managed, standards set, and policy enforced" (p. xi).

In addition to supplying raw material for the final essay, the individual studies are well executed and instructive in their own right. Douglas D. Anderson and Bradley Behrman, in their chapters on state utility rate regulation and the Civil Aeronautics Board, assess changes in established regulatory systems that conventional theories would not have predicted. In contrast, Edward Mansfield depicts the Federal Maritime Commission as a regulatory system that does not work and in which the regulated call the tune. The analyses of the Antitrust Division of the Department of Justice and the Federal Trade Commission by Suzanne Weaver and Robert A. Katzman—taken from their books on the topics—dissect the complex factors, especially professional norms, that shape case selection decisions.

Paul J. Quirk's study of the Food and Drug Administration attacks a fascinating paradox: why drug regulation simultaneously results in "inadequate protection for the consumer" and "excessive burdens" on drug developers. In regard to the Occupational Safety and Health Administration, Steven Kelman provides a comprehensive overview of the policy dilemmas faced by the agency in implementing a regulatory program strongly resisted by its targets. He shows how the precepts of the occupational health and safety fraternity have produced an aggressive regulatory program in the face of substantial opposition. Alfred Marcus uses the case of the Environmental Protection Agency to weigh the effects of precise legislative delegations of authority, concluding that detailed specification, despite the advantages claimed for it, may impede the attainment of policy goals. Jeremy Rabkin dissects another interesting paradox, this one presented by the Office for Civil Rights of the Department of Health, Education and Welfare. The concurrence of what many consider to be overzealousness in the production of rules and regulations pertaining to the treatment of racial minorities, women and the handicapped and a woeful record of effort in enforcement is traced to the mystique of civil rights which limits possibilities for bargaining.

Altogether, the studies reveal disparate patterns which must be accounted for in the origins of regulation, in agency behavior, and in the dynamics of regulatory systems. They show clearly the naive nature of most political theories of regulation and the deficiencies of economic theories. Wilson argues that the microeconomic theory now so prominent in the study of regulation, though not without value, is lacking, because politics differs from economics in important respects. Consequential elements in the regulatory process not encompassed in economic perspectives, but shown by the cases to be of critical explanatory importance, include policy preferences not readily expressed in monetary terms, heterogeneous coalitions which drive much political action, and the continuing efforts to change preferences which are at the heart of the political process.

The analysis of regulation, then, needs to be set within a framework which takes into account the political, and a framework sophisticated enough to allow for observable complexity, variation, and change. This Wilson supplies, relying upon the case studies for suggestions and illustrations of components and relationships. Emphasis is placed on processes of coalition formation and maintenance keyed to the distribution of policy costs and benefits, the balance within agencies among the norms of careerists, politicians, and professionals, and the regulatory environment, composed of such elements as technology, price structure, the political temper of the times and associated influential ideas about policies and institutions, and the dispositions of controllers, such as legislatures, executives and the courts.

The foregoing has not done justice to the particular aspects of the framework or to the stimulative character of the book and the wealth of insights it provides. Of these, one of the most important and interesting is the assertion of the importance of ideas about policies and institutions in setting the course of regulation, independent in their effects of "interests." But the book is not in all its parts beyond criticism. Some may find fault with aspects of the case studies and of Wilson's interpretation and application of their findings. Such quibbles, however, do not detract from the essential value of the book. It persuasively invalidates much conventional wisdom about regulation and proffers an attractive and plausible alternative route to explaining regulatory phenomena. That is an admirable achievement.

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Comparative Politics

Mothers At Work: Public Policies in the United States, Sweden, and China. By Carolyn Teich Adams and Kathryn Teich Winston. (New York: Longman, 1980. Pp. vi + 312. $9.95, paper.)

Despite two decades of ever-increasing female employment, political scientists have virtually ignored questions of public policy as they affect women who work and their families. In *Mothers At Work* Carolyn Adams and Kathryn Winston analyze why government policies toward mothers who work differ in the United States, Sweden, and China. Their treatment is straightforward, sensible, and devoid of polemics.

The authors have wisely assumed that the average reader is either poorly informed or misinformed with respect to social policy for working families. The initial portion of this book, therefore, offers an inventory of government programs in family planning, maternity benefits, child care, home helps and welfare assistance to single-parent families in the three countries. The authors make clear that in the United States—at the state and federal level—social policy makes scant provision for working women. With the exception of welfare mothers, the woman who works is encouraged by those who make policy to rely upon private markets rather than the public sector for medical services, housekeeping help, and child care.

In contrast, although delivery systems and central government involvement vary considerably between the two, Sweden and China provide extensive services for working mothers. The national government in Sweden absorbs most of the cost of its well-known and extensive programs. Under its unitary form of government, provincial and municipal governments must yield ultimate control of programs to the central government. China, by contrast, has relied upon local initiative and funding for the provision of services despite the highly centralized nature of its government. As a result, in China "local service delivery has been susceptible to frequent disruption because it has relied so heavily on voluntary contributions of time, space and equipment" (p. 26). Sweden and China, the authors conclude, while not always successful in actually providing all the desired, or even necessary, services, are at least committed in principle. National policy makers in the United States even hesitate to make a commitment in principle.

Students of family policy will appreciate that Adams and Winston are not content to describe comparative policies and to establish that significant differences exist. At the outset of their study, the authors indicate that they are "concerned with explaining the presence or absence in our three nations of a particular set of social programs to facilitate working women's dual roles as earners and homemakers" (p. 15). Analysis of this kind has been long overdue in family policy research. Presumably, it has not been done because of the differing disciplinary preoccupations of the historians, sociologists, and economists who have made substantial contributions to our knowledge of the relation of the family to the state.

In chapters 3, 4 and 5, therefore, Adams and Winston weigh whether input variables—interest group and party activity—or social and economic factors of the environment account for differences found in U.S., Swedish, and Chinese policy. They first examine whether women's own efforts to promote beneficial government policy have been influential. Swedish and Chinese women are described as social feminists whose demands focus upon improved living conditions for working women even where this requires *unequal* treatment. Women activists in both countries lobby from within large, usually male-dominated organizations—generally political parties or unions. Both this integrationist organizational approach and the nature of their demands differ from those of American women who operate out of mass-based, separatist groups almost exclusively advocating equal rights.

The significance of these differences in women's political action becomes unclear, however, because the authors conclude that policy outcomes are more dependent upon the labor market and attitudes concerning the relation of the state to the family. Adams and Winston are not decisive about which of these factors exerts the stronger influence on policy initiation and implementation. Perhaps their waffling reflects the wisdom of those who know when precise conclusions are premature. *Mothers At Work* reviews a considerable amount of policy in three very different societies. The study opts for breadth rather than depth. Yet depth is required for precise analysis. I would argue, for example, that they underestimate the impact of women activists in the development of child care policy in the United States. By defining most U.S. women's political activity as equal-rights focused, they ignore the role of social feminists like Joyce Miller of the Coalition of Labor Union Women, Marion Wright Edelman of the Children's Defense Fund and, earlier, Elinor Guggenheimer and others. This leads them to deemphasize the impact of mass politics on the development of policy for working women. China specialists may argue that the authors have artificially fit that society's single-party state into an interest-group model and underplayed the debate between economic managers and revolutionary party cadres.

Despite such reservations, *Mothers At Work* serves its audience well by being broadly comparative and, in the best tradition of intellectual inquiry, generates many new questions. It presents analysis where only description existed before. Those of us who work and teach in this field must thank the authors for this important first step.

JILL NORGREN

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The Civic Culture Revisited: An Analytic Study. Edited by Gabriel A. Almond and Sidney Verba. (Boston: Little, Brown, 1980. Pp. viii + 421. No price given, paper.)

"It was the particular nature of the approach of *The Civic Culture* that made it an inspiration to some to do the same elsewhere, to others to do something similar but better, and yet others to do something different" (p. 396), writes the junior author of *The Civic Culture* (Princeton, 1963). This uncharacteristically unverifiable generalization (uncharacteristic for Sidney Verba) appears in a "personal postscript" to a volume which promises "to revisit the civic culture, to subject its philosophy, its methodology, its national and comparative findings to searching critiques by appropriate scholars" (p. v). The criteria for choosing "appropriate scholars" are not presented—only one contributor, Carole Pateman, is well known as a critic of the study—nor are most of the critiques particularly "searching." It also seems curious for Almond and Verba to edit a critique of their own work, but to their credit they generally bend over backwards to acknowledge their shortcomings. Verba's modesty, apparent from the opening quotation, is reflected in word games—"foolhardy" is chosen in preference to "bold" or "incautious" to describe the decision "to apply a new method on an unprecedented cross-national scope to a problem never studied by that method" (pp. 394-95), and in some dithering over how to describe the book's impact on the study of comparative politics. Verba gives an all-too-brief account of the continuing difficulties of developing reliable cross-national generalizations, separating stable from labile attitudes, and "connecting survey data and macropolitical outcomes" (p. 404).

An essay by Gabriel Almond begins the book and provides an illuminating account of the Civic Culture study's intellectual antecedents. He describes but does not defend the derivation from political philosophy of the "rationality-activist model of democratic citizenship" (p. 16) and its incompatibility with stable democracy despite the fact that Carole Pateman devotes her essay to an attack on "its theoretical inadequacy" and "celebration of the role of political apathy and disinterest" (p. 58). Without mentioning her specifically, Verba indirectly acknowledges Pateman's complaints about a "rather one-sided approach to stratification" (p. 407); but defends the choice as empirically sound and justified in terms of their dependent variable—democratic stability.

Comments on the methodological and philosophical underpinnings of the study are provided by Arend Lijphart, Carole Pateman, and Jerzy Wiatr followed by essays on each of the original five nations. The latter are written to a formula to include a discussion of the impact of *The Civic Culture* on scholarship within the country concerned, shortcomings in that country's original treatment, and changes since then in the portrait drawn by Almond and Verba, as well as exploration of aspects of the country's political culture not covered because of sins of omission, methodology, or changed circumstances.

Lijphart is quite gentle and in fact enters the lists on behalf of Almond and Verba to battle critics such as Brian Barry (on the operationalization of democratic political structure) and Erwin Scheuch (on the individualistic fallacy). He does criticize Almond's and Verba's failure to anticipate the consociational route to stability in antagonistic mass level circumstances and a "line of reasoning [which] links an inadequate civic culture to the poor quality of democracy instead of to democratic stability" (p. 52).

Carole Pateman subjects *The Civic Culture* to a careful scrutiny on philosophical and logical grounds and finds it wanting in a variety of respects, most of which she traces to the blinders provided by a liberal pluralist notion of democracy. Craig and Cornelius, in their chapter on Mexico, accept one of Pateman's major empirical criticisms—of the assumption that efficacious replies to questions on citizen and subject competence "accurately reflect system responsiveness" rather than "the respondent's estimates" (p. 357) or acceptance of system norms. Other points are echoed by Lijphart and Wiatr. She is otherwise ignored, despite the fact that her analysis strikes at the heart of the study as a contribution to empirical democratic theory.

The country chapters are impossible to summarize adequately. They present valuable reviews of the literature on political culture and related topics for each country, in addition to the retrospective evaluations already mentioned. In his chapter on Germany, David Conradt comes closest to a true replication by comparing changes over time in two or three of the five countries. He argues that Almond's and Verba's pessimism about that country's democratic prospect is no longer warranted. Giacomo Sani does not dispute their principal generalizations regarding Italy, and believes that, if anything, negative evaluations of the system are even more pronounced now despite (or because of) the erosion of the Catholic tradition and the legitimation of the Communist party. However he does point out that "reticence," a feature of Italian political culture which Almond and Verba noted may have affected the quality of responses, and led to an underestimate of political involvement.

Dennis Kavanagh and Alan Abramowitz take responsibility for tarnishing *The Civic Culture*'s optimistic image of the United Kingdom and the United States, respectively. Both see levels of trust as having declined although Kavanagh sees the change as general and permanent and Abramowitz as specific (related to particular issues) and temporary given "apathy, the reluctance of political elites to mobilize support for structural reforms and the ability of the two-party system to defuse and channel discontent" (p. 205). The implications of these different perspectives for the book's theory of democratic stability are not followed up.

The chapter on Mexico notes the problems for comparison and interpretation presented by a sample which ignored rural areas, translation errors, and "problems of validity or equivalence" (p. 326). More than the other authors, Craig and Cornelius are forced to correct the Almond and Verba portrayal of "their" country, especially on the measurement of system responsiveness. The concept of efficacy or competence is judged useful but better applied to the policy implementation stage and an analysis of patron-client relationships, rather than to respondent impact on "rule making."

How well does *The Civic Culture* stand up to empirical scrutiny? While taking issue with particular aspects of the study, with the exception of Craig and Cornelius, none of the country authors challenges the essential features of the descriptions originally provided. Changes are detected in the political cultures of Germany, the United States, and the United Kingdom which do not conform to Almond and Verba's expectations, but efficacy, trust, and involvement (with the significant addition of unconventional forms of participation) remain the dimensions whose measurement is attempted. All bemoan the neglect of intra-nation variation noted by Stein Rokkan in his original review (*APSR,* Vol. 58 [1974]) and would agree with Verba that the focus on system survival "did somewhat reduce the fruitfulness of the work" (p. 407).

Paradoxically, given its target, the book is insufficiently comparative. It is left to the reader to speculate about the inter-nation generalizations so prominent in *The Civic Culture,* and to synthesize a comprehensive methodological and theoretical assessment.

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Die Besiegten von 1945: Versuch einer Politologie für Deutsche samt Würdigung der Politikwissenschaft in der Bundesrepublik Deutschland. By Hans-Joachim Arndt. (Berlin: Duncker & Humblot, 1978. Pp. 462. DM 98.00, paper.)

Hans-Joachim Arndt's book performs a serious disservice to political science—not to speak of the damage it inflicts on the West German political science profession. At a time when scholars are trying to break their chains of parochialism, focus their attention on fundamental aspects of politics across time and space, and develop modes of communication among those who hold different perspectives, he chastizes German political science for failing to do precisely the opposite.

The catchy title and first subtitle—"The Conquered of 1945: Toward a Political Science for Germans"—tells the story of the first quarter of the book. Twenty-two years old when the Nazi régime collapsed in 1945, Arndt studied at a time when most West German intellectuals were trying to come to terms with their recent history, rebuild a profession devastated by Hitler's politics, and catch up with the thinking and methodologies developed by their colleagues (and frequently former compatriates) in other countries, especially the United States. Yet, ultimately, he argues, they failed to grasp a central "fact": any science of politics must begin with and proceed from the concrete basis of politics in a particular place at a particular time. For postwar Germans, he continues, that "concrete basis of political science" was their country's defeat in 1945. Their failure to understand this has led them to produce the political science of a conquered people, not of Germans.

Strong words, those! We may ignore the arrogance of such a view—and the possibility that some Germans may have seen as more central issues their nation's earlier willingness to embrace Nazism, the problems brought on by the division of Germany, or even the prevention of nuclear war. More problematic in my view are the intellectual and practical implications of a nationalistic political science, one so oriented to one's own country that Arndt can brusquely dismiss any search for regularities across systems and across time, and, for the most part, the intellectual legitimacy or even relevance of such subdisciplinary concerns as electoral behavior, international relations, and most aspects of political theory.

The remaining 362 pages, a tenth of which is bibliography, detail the postwar history of the West German political science profession. Arndt classifies full professors according to whether they tend toward historical-descriptive analysis, bourgeois democracy, social-liberal reform, systemic change, or sympathy toward communist groups, and according to the universities in which they were trained and where they found positions. He classifies articles in three major journals in a variety of ways, including the four general tendencies toward the constitutional state, "scientism," class society, and historical-descriptive analysis. And he describes the development of individual scholars and their "schools," stopping now and then to present case studies of critical turning points in the profession, such as the German Political Science Association's annual meetings of 1973 and 1975.

The description itself is not without interest—*provided* that the reader is sufficiently aware of the circumstances being described to realize that the account is a highly personal one with which doubtless no other German political scientist would agree. (That anyone would discuss in such loving detail the recent history of the American political science profession, including the stormy business meetings at the APSA annual meetings of 1968 and 1978, boggles the mind!) It is interesting to learn, for instance, that people trained in states governed by Social Democrats seem to end up with jobs in Social Democratic states, and that those trained in states governed by the Christian Union parties find jobs in the same states. We are similarly interested but not surprised to learn that the founding fathers of postwar German political science and their earliest students were by and large classical democratic theorists, whereas the younger generation, those born since 1934, are more critical, with the bulk of them even favoring changes in the system.

But ultimately it is precisely this personal characteristic that defeats Arndt's account. His classifying schema, insofar as they are explicated, seem designed to produce a particular result. Much of the descriptive portion seems to carry over into print arguments the author had with his professional colleagues. Moreover, as noted earlier, by writing off as derivative of American political science whole fields such as electoral behavior and international relations, Arndt's general position loses credibility. When Arndt concludes *inter alia* that it was the absence of a "basis in concrete reality" which led the German political science profession to orient itself first toward the approach of the American conquerors and then, after 1966, toward that of the Soviet conquerors, the reader—this one at least—remains unconvinced.

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China's Four Modernizations: The New Technological Revolution. Edited by Richard Baum. (Boulder, Colo.: Westview Press, 1980. Pp. xx + 307. $18.75, cloth; $9.50, paper.)

Not the least of the remarkable events in China since the demise of Mao Zedong in 1976 has been the leadership's outspoken commitment to a comprehensive and ambitious program of economic development. Incorporated into the original, and now revised, 10-year plan, and serving as the foundation for goals to be achieved by century's end, the "four modernizations" (agriculture, industry, science and technology, national defense) has become a theme as prominent and deeply inculcated in the China of today as "politics in command" was a generation ago.

Originally prepared for a gathering of Sinologists in early 1979, the papers contained in this volume probe a variety of aspects of this effort, explaining its origins and present status, analyzing the difficulties which confront China in attaining the stated goals, and assessing the likelihood of the scheme's realization. Appended to each piece are abbreviated "comments" by other participants in the conference.

Authors and commentators on China's current development campaign agree on three points: (1) scientific and technological advances are imperative to the country's overall economic modernization, and underpin the success of the three related "modernizations"; (2) there are serious, perhaps insurmountable, difficulties confronting the Chinese leadership in achieving even modest success in their inordinately ambitious program; (3) less obviously, but perhaps more significantly, the political climate that has most recently been favorable to "modernization" cannot be assured indefinitely. Each of the authors considers at least one of these points and several, including Richard Baum in his introductory essay, note the connections among them. In sum, the pieces offer the most explicit and thoughtful treatment of China's current economic drive available to Western readers.

Even the seasoned reader on domestic Chinese politics is struck by the tenor of these articles: the appallingly backward state of Chinese science and technology and the scale and variety of the society's needs on this score, in spite of some significant gains in the past decade. If the leadership is to succeed in expanding and refining the economy, especially the industrial sector, there is no choice but to expend all reasonable effort in acquiring, absorbing and disseminating—as rapidly and comprehensively as possible—substantive scientific and technological knowledge, skills and capability. The conference participants all agree that, short of this, there is little hope that China will be able to "catch up" with the industrialized superpowers, a goal the Chinese themselves consider critical to the country's national security.

Yet, as several of the authors explain, the problems are immense. "Science" has traditionally been a bit of a bogey in Chinese culture, a vestige of pre-modern China that lingers on in some quarters. More immediate are the obvious and practical problems of how the People's Republic can possibly integrate into the system the degree and extent of contemporary technology necessary to this objective, especially given the lack of a broad-based, complementary scientific and social infrastructure.

Politically, the problem is manifold, and in the long run, potentially the most troublesome. All of the authors in some way refer to the contrary ideological line promoted, and to a large extent implemented, by the now-purged Gang of Four in the period preceding Mao's death. Those unfamiliar with the vagaries of domestic Chinese politics would be prudent to consider carefully the consensus of most of the essays in this volume: that numerous chapters in China's twentieth-century political history remain to be written, and there is no reason to assume that just because radical politics is in eclipse currently, it will necessarily remain so indefinitely.

The *ti-yung* (essence-method) controversy prominent more than a century ago in China seems to be as pertinent as ever: can the "methods" of Western science and technology be introduced and integrated into Chinese society without detrimentally affecting the "essence" of Chinese values? Unlike the values at stake in the nineteenth century, however, those in question currently are the core of revolutionary Maoist ideology.

As many of the authors note, the implications and consequences of contemporary science and technology, which China must somehow acquire from the industrialized and capitalist West, cannot be limited solely to the laboratory or the work place. The likely political impact on Maoist values and norms is unclear. It might be, as Baum notes in his introduction, that the Gang of Four was correct in its assertion that Maoist ideological purity and commitment would fade as modernization (viz., Chinese economic development based on rational scientific and technological knowledge as pioneered in the industrialized West) proceeded.

One cannot also avoid wondering whether Mao's admonition "never to forget class struggle" might not again serve as the basis for a radical swing of the domestic political pendulum. Radical swings are not uncommon in China; it is a leveling thought to consider the possibility and potential consequences of such an event in China's future.

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The Indian Supreme Court and Politics. By Upendra Baxi. (Lucknow, India: Eastern Book Company, 1980. Pp. xvii + 272. Rupees 75.)

Public law specialists are given to describing the American Supreme Court as "a uniquely powerful political institution" (Funston, 1978). And in *Gideon's Trumpet* (1964), Anthony Lewis proclaimed the Supreme Court as "different from all other courts, past and present." If taken literally, one must conclude that the American Court is sui generis—constituting a class of one, precluding comparison with constitutional courts in other polities. Without underestimating the important role played by the Court in American politics, we ought to be able to acknowledge the presence of politically significant judicial institutions elsewhere, if indeed such exist.

For the 31 years of its existence, the Supreme Court of India has been an enormously important factor in Indian politics. The Court has declared hundreds of national and state laws unconstitutional and, since 1967, has even *successfully* claimed the power to invalidate constitutional amendments, most of which were passed to reduce the Court's jurisdiction and powers. India's first midterm election was precipitated by two Court decisions, and, in June 1975, a few hours after the Court made a preliminary ruling with the result that a lower court judgment finding Prime Minister Indira Gandhi guilty of corrupt election practices remained in effect, Ms. Gandhi proclaimed the Emergency that was to last nearly two years. Modeled on the American Court but functioning within the Westminster model of parliamentary democracy, the Indian Supreme Court, in terms of overall political significance, is probably the nearest analogue (not just the nearest non-Western analogue) to the United States' Supreme Court.

Although Indian scholars have regarded the Supreme Court as an important institution, Baxi's book is the first to view it as an important political institution, by which he means that the Court exercises significant political power, and that the judicial process "is a species of political process and that constitutional adjudication is essentially a political activity" (p. 28). While this may be conventional wisdom about the American Court, there is nothing conventional about this thesis in India. Indians are acutely uncomfortable about implied or real linkages between politics and courts, and Baxi's book, by Indian standards, represents a shocking but welcome departure from conventional writings.

Upendra Baxi is a brilliant, young Bombay- and Berkeley-educated lawyer who teaches law at the University of Delhi. He is probably the most respected of the second generation of post-independence Indian legal scholars. Unlike the vast majority of Indian students of the Court, political scientists included, whose orientation is Blackstonian and whose analysis is characterized by uncommon vapidity, Baxi is perhaps closest in his orientation to the school of sociological jurisprudence, and he presents his analysis in a lively, exciting style.

Baxi clearly prefers that he and other Indians be governed by judges rather than today's politicians, and this book is an extraordinary brief for the exercise of judicial power, e.g., "The interests of the people require the Court to *lead* and the legislature to *follow*" (p. 248, italics in original). He seems to arrive at this position because of "the nagging political realities of India" (p. 184), by which he apparently means the absence of any significant political opposition in Parliament during all but two years of the past three decades, and the declining quality of political leaders. For Baxi, these political context factors and the social and cultural contexts as well, require the Court to function "as a parallel legislature and quite often as a parallel constituent body" (p. xii).

This book is addressed to an Indian audience, and assumes prior knowledge about the Court. Moreover, the potential reader ought to be alerted to the fact that Baxi's focus in on the decisions, and the environment of those decisions, of the 1975-1979 period, i.e., the period of the Emergency and the two years of non-Congress rule. Thus the non-specialist reader would benefit from having access to a companion volume which covers more ground, a good one being Rajeev Dhavan's *The Supreme Court of India* (Bombay: Tripathi, 1977).

Perhaps it is characteristic of books of this nature to be better at provoking than proving.

Somewhat annoying is Baxi's tendency to equivocate at critical junctures. He was deeply disappointed by Supreme Court decisions that effectively endorsed the grisly features of the Emergency, but he acknowledges that had the judges done much more, the Court probably would not have survived. He manages to castigate, applaud, and forgive the Court for the infamous Emergency habeas corpus (*Shiv Kant*) decision. Even his most original argument—that the post-Emergency Court has pursued an increasingly people-oriented "frankly populistic" (p. 247) posture, is searching for new bases of legitimation of its power, and seeks to speak to the people at large—though consuming half the book, is too untidy and loosely presented to be persuasive. And one must seriously question whether *any* constitutional court, in either institutional or personnel terms, is adequate to the tasks Baxi would wish the Indian Court to perform. But if the book is at times incautious, it is also a truly seminal study, the most pioneering and most important ever written about the Supreme Court of India.

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Pakistan Under Bhutto, 1971-1977. By Shahid Javed Burki. (New York: St. Martin's Press, 1980. Pp. xi + 245. $22.50.)

Shahid Javed Burki undertakes to explain Bhutto's rise and fall in terms of conflicting class interests in Pakistan. Bhutto won the 1970 election because the urban and rural poor and the urban middle class supported him. By 1977 the latter had turned against him. His nationalization of private schools pulled standards down and alienated the urban middle class whose children attended, and whose women taught in these schools. The nationalization of "vegetable ghee" plants (despite earlier assurance that there would be no more nationalization) projected Bhutto as a covenant-breaker and scared many industrialists out of the country. The nationalization of several thousand flour and rice-husking mills needlessly alienated small business people. Bureaucratic reforms, by deemphasizing merit, hurt the middle class whose sons hoped to enter the higher civil service through competitive examinations. Bhutto's emphasis on a few grand but long-gestating projects starved many smaller development projects which would have expanded employment. Above all, the middle class began to suspect that Bhutto intended to disestablish Pakistan's parliamentary system to further augment his authoritarian rule. These points are all well made.

Most observers agree that Bhutto would have won a comfortable (though not an overwhelming) majority in 1977 even if the election had not been rigged. What then is the import of Burki's reasoning? He implies that Bhutto's reforms and authoritarian rule had so polarized the polity that his opponents would have waged mob violence against him, no matter how fair the election had been. His own supporters, the urban poor, stayed home because those who would have mobilized them—the PPP "left" led by Mubashir Hasan and Haneef Ramay—had deserted the prime minister.

It is not correct that the entire urban middle class had become hostile to Bhutto. Similarly, Burki exaggerates Mubashir's and Ramay's socialism and their hold on the urban poor. He would be on firmer ground if he were to focus on the PPP's general organizational debility for an explanation of the urban poor's passivity.

Burki does not deal with the fact, now freely acknowledged by PNA notables, that they and Bhutto had come to terms by the evening of July 4 with only a couple of minor points left to be settled the next day. Yet the generals struck later the same night. Nor does he refer to reports that while negotiations with Bhutto were in progress some PNA politicians, notably Asghar Khan, urged the generals to seize power. These reports may support the position that the polarization, referred to above, had made military intervention inevitable. But we do not know how plausible this argument is, since Burki does not choose to make it open and explicit.

Burki does not discuss any of Bhutto's positive contributions to Pakistani political culture, not even that he gave his nation's poor a measure of self-respect and awakened them to the importance of political participation. In the absence of any convincing factual basis and appropriate analytical framework, his reference to the allegedly low social origin of Bhutto's mother, as a clue to his authoritarian disposition, would seem to have no scholarly purpose or justification. How strange that the censors in Pakistan have nevertheless banned this book.

Despite the above reservations, this is an exciting book. Burki's initiative in studying social forces and their conflicts as major influencers of political development in Pakistan needs to be carried forward.

ANWAR H. SYED

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Which Way Is South Africa Going? By Gwendolen M. Carter. (Bloomington: Indiana University Press, 1980. Pp. xii + 162. $12.95.)

South Africa exhibits all the requisite conditions for revolution. Nonetheless, white dominance remains firmly entrenched. How much longer? How? Can peaceful, evolutionary change away from racially defined categories occur?

Gwendolen Carter, the most distinguished American analyst of the Republic of South Africa, takes no exception to the likelihood of basic transformation. "Change is in the air in South Africa," she opens (p. 1), consciously echoing former British Prime Minister Harold Macmillan over a score of years ago. By the end of the book, however, one is left—necessarily—with intelligent surmises rather than hard predictions, general tendencies rather than assured outcomes.

This volume draws on close to 30 years' detailed study of the Republic of South Africa. In 1958, Carter published *The Politics of Inequality* (Praeger), a detailed exigesis of apartheid legislation that remains the standard reference. In 1967, she coauthored *South Africa's Transkei: The Politics of Domestic Colonialism* (Northwestern), which sketched a gloomy prospect for what was to become the first "independent" "homeland." Between 1972 and 1977, she and three collaborators published four volumes of source materials, *From Protest to Challenge* (Hoover Institution), a useful compendium of documents from or about groups opposing minority control in South Africa. Carter's criticisms of government policies made her, in effect, persona non grata in the republic; for close to 15 years, her research was conducted from the distant reaches of Smith College, Northwestern and Indiana Universities, and other settings. Only in 1979 could she return for extended field work, and examine areas neglected in her prior publications, notably black consciousness.

Which Way Is South Africa Going? treats domestic rather than international politics, even though the external environment has changed far more than the internal. The buffers of the white redoubt have essentially disappeared since the 1974 Portuguese coup d'état. Now-independent Angola offers sanctuary to a liberation movement (SWAPO) the South African government detests; Mozambique has an avowedly Marxist government economically linked to South Africa through the port of Maputo; Zimbabwe has gained independence since the manuscript was completed; Namibia remains a bone of international dispute and guerrilla contention, the five-power initiative of 1977-78 apparently having bogged down. These are noted rather than discussed. Potential implications of a serious, effective international trade embargo remain unexplored (what *would* happen to the South African economy were its gold exports boycotted?), while the effects of long-term interruptions in petroleum shipments deserve speculation. What Carter says about the military potential of South Africa cannot be gainsaid, however: it boasts "by far the strongest and best-equipped" armed forces on the African continent (p. 114). Blacks are quietly being recruited and trained for combat against external incursions. A Security Council embargo against arms shipments has been effectively circumvented by domestic manufacturing. As a result, Carter reaches a conclusion with which some may quarrel: "South Africa's ability to defend itself against military action from outside seems assured. Its fundamental, and in the long-run possibly fatal, weaknesses lie inside its borders" (p. 126).

The interaction between Afrikaner politics and black politics thus holds the key to South Africa's future. The Afrikaners—proud descendants of the Dutch who wrested most of the Republic from the Africans—give scant indication of being willing to share power, even under extreme pressure. The blacks have been divided by historic antagonisms, dramatically sharpened by the "separate development" strategy of the government. Ultimately, one is forced to conclude that the present South African situation is hopeless, but not serious, terminal in its present state but buoyant at the moment. The eruption of black consciousness, the Soweto riots, and continuing external pressure notwithstanding, the Afrikaner government remains firmly entrenched, having further sharpened its legal and coercive instruments for excising dissidence. The winds of change, though more than zephyrs, remain far from hurricanes.

There are problems with the book, as the author herself best knows. Carter walks an academic tightrope 'twixt contemporary journalism and historical scholarship, 'twixt valued brevity and necessary detail. This volume, published in July 1980, includes references to events barely six months earlier, a record of updating few university presses can manage. Compression means that nuances are mentioned rather than explored; events are outlined instead of analyzed in depth; the recounting of facts is at times breathless rather than reflective. *Which Way Is South Africa Going?* will not answer its focal question to the satisfaction of all, but it gives the reader a clearer idea of the complexities of South African politics than any other book now on the market.

CLAUDE E. WELCH, JR.

State University of New York, Buffalo

Asian Women in Transition. Edited by Sylvia A. Chipp and Justin J. Green. (University Park: Pennsylvania State University Press, 1980. Pp. x + 262. $16.00, cloth; $8.95, paper.)

This edited book contains country-by-country chapters dealing with the role and status of women in Asia, both historically and at present. No methodology or rigid guidelines were imposed on the contributors, but most chapters include sections on general background, socio-cultural (especially religious) norms, and economic and political constraints. Some selections contain case studies while others include attitudinal surveys. A couple of the selections are updated versions of earlier work.

The thesis presented is that the role and status of women in Asia is not static, but in a state of flux. Change of all kinds in recent decades is affecting the position of women in a variety of ways. Some of the changes in Asia have had a positive effect on the situation of women, others have been more superficial than genuine, and still others have been challenged by a reactionary backlash. The book makes clear that the ultimate problem of women in Asia generally is that they have been, and still are, excluded from the mainstream of economic and political power. Indeed, in some of the countries examined, the concern must be focused at a much lower level of aspiration: the problem of women who are denied basic choices concerning the conduct of their own daily lives. The key analytical questions posed are (1) what are the economic, social, political, ideological, and religious factors affecting the process of role and status changes? and (2) will changes in women's roles really alter their status very much?

The selections in the book are well balanced between East, Southeast, and South Asia, and some clear area contrasts emerge. The position of women in Southeast Asia is considerably higher than women's position in either East or South Asia. This is apparently the result of the bilateral (rather than patriarchal) kinship system, and of traditional customs which accord the female high status. The position of women is highest in the Philippines and Thailand, and somewhat lower in the Muslim countries of Malaysia and Indonesia (with the exception of Java, where there is the potential for high status).

The position of women in East Asia is not as high, but the paradox there is that the real status of women is higher than the ideal. In the two chapters on China, it emerges that, while the Communists are ideologically committed to equality for women, whenever this goal conflicts with economic aims (for example with unemployment problems or child care costs), equity is shelved.

However, it is in South Asia that the status and role of women is the most depressing. Religious and cultural norms, Muslim and Hindu, place women at the bottom of the social ladder in a situation that offers virtually no mitigating factors.

This book would serve as a good text for women's studies courses and an interesting supplementary book for Asia courses of various disciplines. It fills a gap in the literature, and it is well presented. The selections on Indonesia, Malaysia, China, and the Muslim areas of South Asia are particularly enlightening. The main faults of the book are faults of omission. A number of interesting places are not covered, for instance, Singapore and Vietnam. Also, some of the contributions (e.g., on the Philippines and India) are too narrow in scope to provide an overall picture. Most disappointing is the limited treatment accorded to Hindu India. It consists of only one chapter which concentrates almost entirely on the political skills of Mrs. Gandhi. While Mrs. Gandhi is an interesting subject, the absence of additional, more general material on the position of Hindu women seriously short-changes a large and important area of consideration. Nevertheless, as it stands, the book is certainly to be recommended.

DIANE K. MAUZY

University of British Columbia

China's Global Role: An Analysis of Peking's National Power Capabilities in the Context of an Evolving International System. By John Franklin Copper. (Stanford, Calif.: Hoover Institution Press, 1980. Pp. xv + 181. $7.95, paper.)

Many observations have been made about China's status as a second-ranking power, but not so much work has been done to evaluate systematically China's power capabilities. John Copper's book fills this gap considerably. It is a welcome addition to Chinese studies.

Using numerous statistics and comparative figures, the author examines carefully several elements that determine China's power status. These elements include geography, population, natural resources, economic strength, military power, political system, and science and technology.

In the geography and population factors, Copper discusses the advantages and disadvantages of China's geographic situation and population size in comparison with several other nations. Although China's geographical condition is difficult for an enemy's invasion and occupation, her population size may not be "an asset" in germ, chemical, or conventional warfare on Chinese soil (pp. 22, 28).

Copper examines China's natural resources, ranging from basic grain production to up-to-date energy capabilities. One of China's important resources, uranium, which is essential to nuclear development for both peaceful use and military purposes, is not discussed. Uranium deposits in China are reportedly ranked seventh in the world, behind the U.S.A., the USSR, South Africa, Canada, Zaire, and Czechoslovakia.

Copper's statistics show clearly that, in terms of economic strength, China is still a backward country. In military power, the author finds that the number of military personnel (over 4 million, p. 75) and conventional weapons is of only relative significance. The lack of modern and sophisticated weapons, as all are aware, is the main weakness of the Chinese military power. But, if we take China's "defensive military policy" into consideration, we will see that China's second-ranking military power is also determined by its defense policy.

Copper discusses the well-organized and efficient Chinese political system but also asks whether the Chinese government is a totalitarian regime (p. 95). When we look at the turmoil during the Cultural Revolution that forced Beijing to recall all of its ambassadors, except one (Huang Hua in Egypt), for "consultation," we will have to acknowledge the fact that the Chinese government is still a centralized regime with not necessarily a stable leadership. Power struggles among factions are interpreted by the Chinese leaders as only the party and anti-party struggles, regardless of the fact that the previously purged "anti-party" faction has turned out to be the party leadership today.

Copper rightly points out China's backwardness in the field of science and technology when compared with other powers. Undoubtedly, political campaigns and ideology present a serious obstacle to progress in the field (p. 136). Finally, Copper concludes that China can only be regarded as a second-ranking power and that she will not become the dominant power in Asia (p. 150).

Two points can be raised here. First, the problem of overpopulation (pp. 23, 134) merits some elaboration. Recently, visiting scholars from China to the United States strongly indicate that overpopulation in China has entailed serious problems that certainly have slowed the economic growth. These problems include food, housing, employment, education, and even social unrest. They still appreciate economist Ma Yi-ch'u's "lost" warnings of overpopulation in the early 1950s. This dimension of evaluation can hardly be achieved by statistics.

Second, it is hard to predict that China in the future "will not become the dominant power in Asia." If the definition of "dominance" means the Soviet *domination of* the Eastern European countries, China will not obtain such a power status. But if it means the U.S. *influence over* the American states in the West Hemisphere, China may reach it in Asia in the future provided that China enjoys a few decades of peace. The magnitude of China's relative power can be more accurately measured at that time.

This book, nevertheless, is a fine and scholarly work. It is quite useful to students in the field.

King C. Chen

Rutgers University

Ideology, Public Opinion, and Welfare Policy: Attitudes Toward Taxes and Spending in Industrialized Societies. By Richard M. Coughlin. (Berkeley, Calif.: University of California Institute of International Studies, 1980. Pp. xvi + 195. $4.95, paper.)

This book presents a variety of survey data on public attitudes toward welfare policy for eight Western democratic nations—Australia, Canada, Denmark, France, Sweden, the United Kingdom, the United States, and West Germany. Survey responses are examined which pertain to broad ideological support for "welfare state" activities, for specific welfare programs, and for recent "tax revolt" initiatives. Unfortunately, the book offers little beyond a descriptive presentation of a rather mixed collection of survey data. Two principal shortcomings justify this conclusion.

First, the book suffers because it lacks a theoretical rationale. Coughlin offers neither hypotheses nor theoretical puzzles to be analyzed. Obviously, several theoretical postures are possible here and each would link the data to be examined to some particular explanatory focus. Yet Coughlin adopts none. He claims a concern with explaining "welfare state development," but he backs away from postulating any particular relationship between mass attitudes and welfare policy. And he has no data suitable for testing developmental hypotheses. He attempts to compare levels of public support for welfare policies with levels of program content. Yet, his conclusions about these comparisons are purely descriptive.

Ultimately, this absence of theory is the most important shortcoming of the book. Without a clearly conceived focus, the book breaks down into three descriptive presentations of survey data, themselves only weakly interrelated, with little effort to tie their results to any centralizing issues of more than topical interest.

Related to the absence of a theoretical focus is Coughlin's failure to take account of several bodies of research of notable relevance to his topic. One of these concerns the appropriate character, if any, of causal linkages between mass preferences and public policy. Research on this topic has, of course, been particularly extensive on the American case. A second body of literature is the more recent spate of "comparative policy" studies, many of which focus on some of the same issues of interest to Coughlin. A third body of research, largely focused again on the American case, is the controversy about whether there exist consistent ideological postures or, for that matter, specific policy-relevant attitudes at the mass level. The literature in all these areas is extensive, and each is of direct concern to Coughlin's work. Yet, none of the relevant literature is addressed in this study and the topics themselves are ignored or, in the case of the first one mentioned above, shrugged off as beyond the scope of the present study.

Second, the book is a severe disappointment methodologically. Coughlin offers an "analysis" of his various survey data which is, at best, a generation out of date for systematic social science. What passes for empirical analysis is the presentation of side-by-side frequency distributions for responses to somewhat similar (but sometimes rather different) survey items for eight nations at various times over the last 30 years. The data are at points very problematic as to comparability. As one might expect, the range of time points, question wordings, and response formats is exceedingly wide. Even descriptive generalizations are hazardous in these circumstances, yet Coughlin proceeds to make them. In his analysis of support for specific kinds of programs, Coughlin only has even roughly comparable data for two program types (old-age pensions and nationalized health care). Yet, for the three remaining program types, when survey data were by his own admission unavailable or extremely fragmentary for over 60 percent of the comparison points, Coughlin proceeds anyway to use the data to support descriptive generalizations.

Beyond these frequency distributions, no more sophisticated analysis is offered—even at points where possible additional analyses virtually thrust themselves on the reader. For example, Coughlin "analyzes" several bivariate rank-order associations by visual inspection. My own visual inspection of some of these situations suggested much more moderate relationships than judged by Coughlin. If one thought the data good enough to use in the first place, rank-order correlations would, of course, not be subject to such subjective disagreement regarding their implications. Ultimately, then, for failings of both theory and method, this book offers little to recommend it.

Kim Quaile Hill

University of Houston, Clear Lake City

Poverty and Problem-Solving under Military Rule: The Urban Poor in Lima, Peru. By Henry A. Dietz. (Austin: University of Texas Press, 1980. Pp. xiv + 286. $19.95.)

This is a thoughtfully done study of the urban poor in Lima under Peru's "revolutionary" military government. Drawing on survey data, participant observation, and systematic comparison of six neighborhoods, Dietz contributes to both the Latin American literature on urban workers and the more general literature on political participation.

The survey data, approximately 550 interviews conducted in 1971, generally confirm earlier findings regarding the urban poor. A high proportion are migrants, but most hold non-marginal jobs. Most have achieved some upward social mobility, though Dietz emphasizes the shift from lower-level to more skilled manual occupations *after* arrival in the city. Most are relatively satisfied with their material progress and cautiously optimistic about the future. Very few blamed the government or existing social structures for their problems; most felt that "hard work" was the best way to improve their lot. These data obviously contradict the once-popular characterization of urban workers as an anomic, dislocated, and undifferentiated mass.

The second major theme is political participation by the urban poor. Whereas previous works have treated organizational membership as an independent variable, Dietz suggests that involvement in community self-help activities, membership in community organizations, and involvement in local and/or national demand-making are all distinct forms of participation. The first generally involves collective action at the neighborhood level to provide public goods, e.g., a neighborhood recreation center. The second, membership in the local organization, increases the probability of involvement with local or national authorities, but also includes other activities which are political but not demand-making per se. The argument for treating organizational membership as a separate mode of participation relies on factor analyses and causal modeling, about which I have reservations, but Dietz adds another element to the debate over the adequacy of the Verba and Nie typology of participation.

To me the more important contribution is the finding again that the dominant forms of participation are conventional, non-disruptive methods of petitioning and lobbying for assistance in the solution of local problems. The demands of the poor were generally limited to goods which the system is designed to produce. Often those demands were consciously tailored to the requirements of particular agencies.

The study also confirms the existence of a variety of participatory activities despite military rule, although this is an unusual case. The critique of SINAMOS and other mechanisms for participation created by the Velasco government adds to a growing literature on the political limitations of military radicalism in Peru.

The major weakness of this book is a certain diffuseness, which results from its attempt to be truly comprehensive in its approach to the urban poor, locating individual acts of participation within a specific community context as well as the larger national and international context in order to explore how the poor affect and are affected by the nature and structure of power within society. This is a herculean task, which necessarily involves a large number of related but nevertheless separate questions. Trying to deal with all of these—from the causes of urban migration to the limits of military radicalism—across all the levels of analysis involved—from individual differences among migrants to the structure of world capitalism—involves the author in many issues which go far beyond his data. A less ambitious study more closely tied to the original purposes of the field research would have produced much the same conclusions without diminishing Dietz's contributions to our understanding of the politics of the urban poor.

JOHN SAMUEL FITCH

University of Colorado

Political Development and Social Change in Libya. By Omar I. El Fathaly and Monte Palmer. (Lexington, Mass.: D.C. Heath, Lexington Books, 1980. Pp. xiii + 230. $13.00.)

Since they seized power in 1969, the overriding domestic preoccupation of Colonel Muammar Qadafi and Libya's other revolutionary leaders has been the search for a satisfactory political and economic system. The regime has had considerable revenue from oil at its disposal. Nevertheless, it has gone from one political experiment to another with only limited success. El Fathaly and Palmer describe and assess these efforts to achieve development objectives, most notably the mobilization and resocialization of the country's citizens and the transformation of Libya's rentier state economy. The book is thus about the nature and consequences of strategies of nation-building, fitting nicely into the emerging policy analysis perspective in the study of political development.

Libya's development efforts went through three phases between 1969 and 1979. The first was marked by the replacement of the traditional elite with modernizing administrators, by the creation of a mass-oriented political organization, patterned and named after Nasser's Arab Socialist Union, and by the use of schools and mass media as agents of socialization. However, Qadafi acknowledged the failure of these efforts in 1973 and offered new proposals to guide the revolution. Similarly, he stated in 1975 that progress was still "too random" and articulated in the *Green Book* the philosophy of an even more radical transformation. Part I, published in 1976, deals with political organization and Part II, appearing in 1978, concerns economic change.

El Fathaly and Palmer summarize the evolution of Libya's political strategy during the revolution's first decade and give critical assessments of specific programs. They show, for example, that modernizing administrators, appointed to bring the revolution to the masses, were in fact quite timid on matters of change. Other efforts assessed include the national plans of 1973 and 1975, which were hampered by a dearth of competent middle managers and which had the unintended consequence of reinforcing the rentier state pattern. The evaluation of early programs relies heavily on survey data and overlaps to some extent with *Political Development and Bureaucracy in Libya* (Heath, 1976), which the authors published with Richard Chackerian (reviewed in the *APSR*, September 1979). Other information sources include aggregate statistics, government documents, newspaper reports and interviews with local and national elites.

Beyond its theoretical interest in strategies of development, the book provides much useful information about contemporary Libya. The characteristics of the political elite are presented, for example. Also notable are translations from the *Green Book* and a detailed summary of the five-day General People's Congress of 1978.

A weakness of the volume is that the data with which it evaluates regime policies are sometimes thin. The accuracy of the authors' general conclusions is not at issue; what is needed is more specific factual information about the functioning and impact of government programs. On the other hand, given the difficulty of collecting reliable information on such sensitive matters, it is more reasonable to commend El Fathaly and Palmer for the data they do provide than to complain about what is missing. A related concern is the authors' apparent acceptance of the regime's view that a lack of popular motivation and revolutionary consciousness are among the major obstacles to progress in Libya. Thus while some Libyans may find the book overly critical, other observers may consider it too scrupulous in judging the revolution on its own terms, thereby ignoring the possibility that Libya would be better served by a different conception of development. But this is a subject for another investigation. El

Fathaly and Palmer assess policy by referring to the revolution's own goals and, in the process, offer a useful and thoughtful account of the nature and consequences of Libya's development strategy.

MARK TESSLER

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Ethnic Soldiers: State Security in Divided Societies. By Cynthia H. Enloe. (Athens: University of Georgia Press, 1980. Pp. xii + 276. $19.00).

The thud on the desk of a new book on either ethnicity or the military might evoke only a yawn. It would find many recent companions on the bookshelf. However, the reader's curiosity is at once aroused when the two themes are discovered interwoven. *Ethnic Soldiers* will well repay all those attracted by its novel topic.

By joining ethnic questions with military ones, Cynthia Enloe makes a breakthrough on both fronts. She provides important new insights concerning the politics of ethnicity by examining its operation in the security core of the state. At the same time, conventional views about the military are confounded by her careful inquest into the ethnic composition of military forces, concealed from view by conventional assumptions about the quintessentially national and integrative character of the armed forces. On both scores, the book will stand as a major contribution.

Enloe fired one of the first salvos in what became a barrage of ethnicity studies with her 1973 book, *Ethnic Conflict and Political Development.* The present volume moves beyond the earlier work in its theoretical sophistication and in the scope of materials used, both in time and space. Even the most skeptical reader cannot fail to be impressed by the range of her examples.

The most evocative and stimulating concept introduced by Enloe is the notion of a "state security map." Most contemporary (and for that matter historical) states are multi-cultural. While the unitary doctrine of nationhood is usually coupled with state creeds, in reality most ruling elites must quietly take cognizance of varying patterns of polity relationship among various ethnic communities. Cultural groups vary greatly in their degree of politization, in their affective attachments to the national entity, in the socioeconomic advantage (or deprivation) they derive from it, and in their proximity to the core institutions. Further, each of these factors is situational, fluid, and changeable over time.

Military establishments became an integral part of the institutional means by which the state copes with external adversaries, and also assures the hegemony of the state over the population. Because force is placed in the hands of the custodians of state security, the fidelity and reliability of those custodians has troubled rulers from time immemorial. Enloe brilliantly demonstrates how crucial ethnic calculus has been in controlling strategies for the security forces. What social composition must a constabulary have to repress uprisings among those groups where unrest is most likely?

From such ruminations comes what Enloe terms the "Gurkha syndrome," which was to have such a widespread effect on the composition of colonial armies. Particular communities, usually socially marginal, acquired the reputation of forming a "martial race." As colonial security forces became dominated by such groups, a military tradition could well arise in these recruiting areas, where—in the absence of appealing alternatives—army service became a way of life. More particularly, Enloe documents the impact of ethnic strategies in military personnel policies in countries as diverse as the United States, Canada, Peru, Russia, Britain, Malaysia (and many more).

Most studies of the military really focus on armies. Other components to the security establishment—police, navy, air force—are overlooked: but not by Enloe. Indeed, ethnic calculus operates quite differently in each of these domains, in function of the role played and status enjoyed. Police are generally of lower status, less well armed, and not a potential threat to the state itself; because of their more direct interactions with the populace, there are more advantages and fewer risks in representativeness. Navies and air forces, by contrast, are much higher-status pursuits, and for the most part lack the capacity to directly intervene internally; much more than armies, they are truly external security branches. Thus social class has been much more important than ethnic calculus in shaping the composition of these forces, especially their officer cadres.

Any undertaking of such scope inevitably invites the reviewer to carp at some omission. The volume loses some of its punch in the final portion, as the examples begin to become repetitious. Enloe asserts the importance of renascent theoretical concern with the state, but then fails to make use of it. She too abruptly dismisses the significance of regional shifts in regime dominance brought about by military coups in Benin and Congo-Brazzaville (pp. 143-58). And Lunda citizens (men as well as women) will take justifiable offense at being categorized as "tribesmen" (p. 231).

However, these are inconsequential quibbles. Enloe may take pride in the "ethnic soldiers" she marches past the reviewing stand. They are a well turned out unit, and merit our salute.

M. CRAWFORD YOUNG

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Poverty and Inequality in Common Market Countries. Edited by Vic George and Roger Lawson. (Boston: Routledge and Kegan Paul, 1980. Pp. xi + 253. $27.50, cloth; $15.00, paper.)

This collection of individual country studies seeks to answer three important questions: How is poverty defined in the various EEC countries, and what policies have been adopted to deal with the problem? How extensive is inequality in income and wealth among different social and economic groups? And what trends can be discerned in the understanding, treatment and prevalence of poverty and inequality? The countries dealt with are the United Kingdom, Belgium, France, Italy, Ireland and West Germany.

This book demonstrates most clearly the numerous difficulties of exploring, let alone answering, questions about poverty and inequality. Problems of data hamper not only comparative analysis, but also investigation of these issues in a single country. Nevertheless, all contributors make a valiant effort to use what reliable data are available, and to arrive at conclusions concerning both the extent of poverty and inequality and the effect of governmental policies on them. The data for all of the countries surveyed point to similar conclusions. Despite unprecedented high rates of growth both in the economy and in government spending over the past few decades, considerable income inequality persists. Social welfare and tax policies have done little to reduce the extent of socioeconomic stratification. If anything, they have tended to reinforce income inequality. Few inroads, moreover, have been made in lessening the amount of poverty in many of the Common Market countries. In most instances, income maintenance programs have not been aimed at low-income groups; consequently, they have not benefited from such programs. Finally, almost all contributors suggest that the problems of inequality and poverty are interrelated, and that if there is no effort to deal effectively with the former, attempts to reduce the latter will meet with only partial success.

In the studies of individual countries, however, the extent to which the question of inequality has entered into the political arena is paid scant attention. Indeed, with the exception of the discussions of France by Adrian Sinfield and on Italy by David Moss and Ernesta Rogers, there is little systematic analysis of either the ideological or the political context in which poverty and inequality are imbedded and maintained. A discussion of various functional and structural theories of poverty and inequality is found in the introduction to the volume by Vic George. But it is too general to provide testable hypotheses concerning the extent and nature of inequality and poverty in the EEC countries or differences and similarities among them.

Equally lacking is a systematic comparative analysis of these differences and similarities. Roger Lawson and Vic George, in the concluding chapter, offer an overview of the findings from the single-country studies. They attempt to rank the countries according to level of income inequality and, more problematically, according to degree of poverty. They also suggest that, both across and within countries, "The relative level and changes in income maintenance expenditures do not bear much relationship to the extent or change in relative poverty" (p. 240). They attribute this to the existence of other more powerful structural and economic determeinants of inequality and poverty, as well as to the fact that the beneficiaries of such expenditures have not been the most disadvantaged. Yet a comparison of these determinants and of the impact of social welfare policies is not made. Also not addressed in this volume are the subjective dimensions of inequality and poverty; that is, people's awareness and evaluations of existing patterns of stratification. This, Sinfield argues, "might help to account for different patterns of poverty and the extent or lack of policy concern for the deprivation of particular groups" (p. 117).

Despite these omissions, this work does contribute to our knowledge of the extent and nature of inequality and poverty in certain European countries, and does lay the groundwork for further more systematic comparative analyses of these phenomena and the reasons for their persistence.

GOLDIE SHABAD

Ohio State University

Race and State in Capitalist Development: Comparative Perspectives. By Stanley B. Greenberg. (New Haven: Yale University Press, 1980. Pp. xiii + 489. $40.00, cloth; $11.50, paper.)

This is a densely rich and provocative book. Stanley Greenberg traces the emergence of capitalist agriculture and industry in the U.S. South (specifically Alabama) and in South Africa to answer the important political question: does the rise of capitalism necessarily produce a free labor market, one in which racial and ethnic categories and barriers fall away? Using the cases of Alabama and South Africa—and, to a lesser extent, Northern Ireland and Israel—Greenberg challenges the facile assumption that capitalists and their state allies need and, where they are powerful, create a free labor market and, in so doing, make racism more and more anachronistic. Such an assumption misses the very overlaps and lags that create political contradictions and the structures intended to resolve or at least band-aid over those contradictions.

Much of the analysis of the transition from feudalism to capitalism within societies and as prompted by international pressure is peculiarly apolitical. The macro-analysis that portrays certain transformations as virtually irresistible essentially denies the existence of choices, of selective use of power. That is, in many of even the best studies of the history of political economies, the *politics* of the evolutionary pattern is left out. It is as if no one ever made choices, as if there were no choices to be made. This stands out as especially paradoxical in that some of the best work, by analysts such as Emmanual Wallerstein, Perry Anderson, and Samir Amin, is intended to be apolitical critique of the current international power configuration. Greenberg makes strides toward reintroducing politics by pin-pointing those choices, actors and legal contrivances that characterized the usually contradiction-filled transition period. It is important for political scientists to take a hard look at Greenberg's book, to incorporate into their own research both the macro-historical sensitivity (too often lacking in much of the work in our discipline) and the specifics of power utilization.

Greenberg makes clear that in both South Africa and in the U.S. South, as well as in Ulster and Israel, it is impossible to comprehend the peculiar character of the contradictions in the feudalism-to-capitalism tradition without taking explicit account of two politically charged phenomena: ethnicity and the state. Actually, Greenberg discusses racism more than ethnicity, and when he compares South Africa's and Alabama's capitalization of agriculture and industrialization with those of Ulster and Israel it would have been helpful to have drawn finer distinctions between racism and ethnic communalism. Nonetheless, he does demonstrate that, in reality, the alleged capitalist imperative of "free," mobile labor is tempered by the utility of racial/ethnic categories for even the emergent capitalists. Furthermore, his comparative study exposes the state's crucial part in not just preserving such categorizations, but also in adjusting them so that the South African mining companies will get the African laborers they need and the Birmingham steel mills will get the racially structured work forces they need—at the wage rates and under contract conditions that will assure not only profits but also labor stability. The state, according to Greenberg, is not a passive tool in the hands of the capitalist class, but rather is often an active resolver of tensions between capitalists in different sectors with different labor/ethnic needs and between the emergent capitalists and the older feudal agrarian elites. Rarely, of course, do the various sectors of the labor force—white or black, Protestant or Catholic, Arab or Jew—control that state apparatus.

This book makes a contribution to political science by pointing to the ways in which the state evolves during the early capitalist period in ethnically and racially segmented societies. It compels us to take legal structures seriously, to analyze them as means (not always successful means) by which certain classes attempt to alter the mode of production to suit their own interests without giving up those racially and ethnically defined relationships that have bolstered their influence in the past.

At the end, the reader is left a bit uncertain as to whether Greenberg believes that "ultimately" the transition will pass; if (a big, i.e., politically problematic, "if") the state is astute enough to first patch over and then resolve the contradictions during the lag period, then indeed the imperatives of capitalism for a "free" labor market will rule, and racial and ethnic barriers will wither away. Because of his interest in both U.S. southern politics and South African politics, Greenberg has a real stake in projecting their respective trends. He certainly is not a sanguine political observer of either of these systems. But his conclusion could be read as coming down, even if tentatively, on the side of projections which assume that more genuine capitalism will finally produce an awareness on the parts of both the strongest business interests, and the state elites who have become their allies, that racial discrimination, even racial stratification (the two are not synonymous), are anomalous. One looks forward to Greenberg's continuing monitoring of both these political economies, for he has the comparative skills and historical sensitivity to make us smarter about the ongoing processes of change and contradiction in capitalist states.

CYNTHIA H. ENLOE

Clark University

Politics and Policy Implementation in the Third World. Edited by Merilee S. Grindle. (Princeton, N.J.: Princeton University Press, 1980. Pp. xv + 310. $20.00, cloth; $7.95, paper.)

Implementation, the relative success or failure of public policies, has emerged as a focus for administrative studies in the past decade. Implementation analysis might supercede in popularity the grander schemes of managerial science for the selection of policy alternatives such as cost-benefit and systems analysis. The emergence of implementation stems from the failures of public policy on a global scale—from New York City to terror-stricken Uganda. Aspirations and ambitions are in retreat. Now the most important criterion for a policy is its feasibility. Can the policy alternative adopted be implemented, achieving the goals in view?

This conceptual approach is applied in this series of nine case studies in Zambia, Peru, India, Colombia, Brazil, and Kenya. The context (the political environment of the policy) and the content (types of benefits—e.g., housing, nutrition—interests involved, resource commitments) are examined for their consequences for implementation. Merilee Grindle states that interest in implementation has been stimulated by the realization that the translation of policy statements into goals, whether the implementation is successful or not, involves "fundamental questions about conflict, decision-making, and 'who gets what in a society' " (p. 3). Implementation, then, is ultimately a political process.

This book undertakes a difficult task. The conceptual framework is an effort to reduce an irrational process having greatly disparate goals, e.g., squatter removal, infant nutrition, rural electrification, agricultural cooperatives pursued in different historical and cultural contexts, to a model which relates complex and often ephemeral real-world events to specific variables. Accomplishing this, given the present state of the art, is for political scientists the equivalent of the mathematicians' squaring of the circle.

This volume deserves consideration from two perspectives: (1) To what extent does the conceptual apparatus of implementation contribute to the outcomes in the case studies? Does implementation provide a basis for classifying the events and relationships which account for success or failure of the programs as designed? (2) Do the case studies contribute to the understanding of administration of social and economic change in the Third World?

In the concluding chapter, "Implementation amidst Scarcity and Apathy: Political Power and Policy Design," Peter S. Cleaves appropriately answers the first question. He places these materials, after observing that the success or failure of program "depends upon the perspective of the observer" (p. 281), into a framework of closed, intermediate, and open systems and "Coordinates Affecting Policy Implementation." These coordinates are policy actors that have more or fewer resources dealing with policies that are more or less problematical. Cleaves tells us that whether or not a policy can be implemented depends upon whether the regime is relatively authoritarian and coercive, or democratic and persuasive, and whether the implementers of policy are rich or poor in resources to apply to policies that are tough or easy to implement.

Considering the book as an instance of implementation, the application of a theoretical design to complex data, I find that these conclusions, after elephantine labors, bring forth a mouse. This is not to denigrate the intellectual effort or the validity of the conclusions. However, an experienced student of government administration could have arrived at these conclusions about the obstacles to implementation without the aid of the model.

The second question involves the informative value of the case studies. All of them, but most notably David F. Pyle's study of a pilot infant-nutrition project, left the impression that the authors were writing with inordinate restraint. They appear to be telling less than they know. There are several plausible reasons for this: conventional scholarly caution, the fact that five of the studies had their origin in dissertations with one in progress, or constraints introduced by the conceptual framework.

The case studies are adequate and informative. Of signal interest are Stephen A. Quick's report on ideological molding of implementation and its consequences in Zambia, and Frederick T. Temple's and Nelle W. Temple's report on how a public housing program in Kenya became the source of spoils for a favored few. Merilee S. Grindle, in her analysis of the role of "the implementor" in Mexico, gives the most comprehensive and disciplined application of the theoretical framework. She also provides an extensive bibliography of the literature of implementation.

In her introductory chapter, Grindle observes: "Most Third World countries have extensive and active public sectors involved in many aspects of economic and social life, and state bureaucracies may be among the strongest institutions of society" (p. 17). Later in the chapter she says: "The functions of public administration, and thus the capacity to carry out program activities, are characteristically not well institutionalized in Third World countries" (pp. 25-26). Pondering these two propositions can support the thought that implementation is a revival of the much-abused politics/administration dichotomy, with the impiety of profane politics as always destroying the prospects of salvation.

HERBERT G. WILCOX

West Virginia University

Public Policy in a No-Party State: Spanish Planning and Budgeting in the Twilight of the Franquist Era. By Richard Gunther. (Berkeley: University of California Press, 1980. Pp. xiv + 361. $17.50.)

The study of corporate authoritarianism has yielded a variety of fresh ideas and insights that bear on our theories of the state, political economy and interest-group representation. Thus, while the regime of Francisco Franco has passed into history, important questions about its implications and significance linger on.

Richard Gunther's book is a valuable effort to give a more precise and informed answer to a question that guided several earlier studies of public policy making in late Franco Spain. How different in fact are the processes of economic policy making in authoritarian and democratic systems? In the last generation of Franco's rule, Spain adopted a program of economic management that closely paralleled the strategies of other Western capitalist nations. Examining the record of this period, can we determine how important democratic institutions are in steering an industrial economy? Was Spain hampered—or advantaged—in its pursuit of the goals of modern economic policy because of its peculiar political arrangements?

Gunther's case is that authoritarianism did make a difference. Budgetary policy in late Franco Spain was made in something of a power vacuum. The Caudillo provided little systematic guidance in economic and social policy. His concerns were mainly with regime structure and maintenance. The putative dominant party had become a hollow shell, served in no sense as a vehicle for interest aggregation, provided no agenda for public policy. Neither legislative process nor interest-group activity had much bearing on the policy process. Consequently, the budgetary process was highly legalistic, hierarchical and personalistic. The politics of the budget were the politics of the administrative state.

Within a "zone of indifference," Franco's economic advisers had substantial discretion to set the economic goals and policies of the regime. Nevertheless, they operated in a space delimited by certain identifiable political constraints set by the Franquist elites. Thou shalt not increase taxes. Thou shalt not issue public debt. Thou shalt not generate revenues by means of the printing press. This meant that the economic managers were precluded from using certain Kenyesian measures fully to steer the economy, and relied more heavily on a policy of deregulation, indicative planning and selective public investment.

In the end, Gunther argues, the structural characteristics of the regime did delimit the agenda for change. Proponents of more substantial reform were prohibited from organizing public support behind their proposals. Unless the Caudillo perceived a relation between economic programs and his own goals of regime stability, policy innovation could not be effectively pursued within the channels of the administrative state. The result, for Gunther, was an agenda and a policy product quite different from what might have been possible in a more democratic context.

In making this case, Gunther seems to be arguing against certain propositions—perhaps, more generally, the tone—of my earlier study of economic policy making in Franco Spain. It is true that I was impressed by the range of proposals and initiatives that were admissible into Spanish political economic discourse in the 1960s. I still believe that it is necessary to understand the dynamics of the economic policy debate in this period to appreciate certain continuities in the policy-making processes of late Franco and post-Franco Spain.

Nonetheless, I think Gunther offers a needed critique and corrective to my earlier views. He is right in emphasizing the constraints and rigidities against my interest in the latitude that was available for what Albert Hirschman called "reformongering." At times, however, I think Gunther unnecssarily sharpens the differences in our perspectives. (For example, at p. 92, he takes exception to a "characterization" of Spanish policy making in my book which is actually a statement about Western policy making generally.) In fact, our interpretations differ mainly in nuance and detail. We are not arguing totally different theses.

This is a carefully researched and well-written book. It is in several ways the most successful study of economic policy making in Franco Spain. Medhurst's work was too descriptive and preoccupied with administrative detail, my own too narrow in focus and too much guided by a somewhat idiosyncratic theoretical framework. Gunther has the advantage of perspective and he captures the overview. This is a straightforward and comprehensive portrait of the way in which the distinctive politics, institutional framework and norms of a corporate authoritarian state affected its decision-making processes.

CHARLES W. ANDERSON

University of Wisconsin, Madison

Pluralism and Corporatism: The Political Evolution of Modern Democracies. By Reginald J. Harrison. (Winchester, Mass.: Allen and Unwin, 1980. Pp. 198. $22.50, cloth; $9.95, paper.)

Reginald Harrison is concerned with the present and future state of modern democracies. He argues that democratic politics in modern industrial societies more closely resembles a corporatist model than a pluralistic one.

In this corporatist model authority in different decision areas was delegated to non-competitive, governmentally linked, consultative bodies "in which managerial, professional and union technocrats met with the appropriate government bureaucracies as the recognized representatives of groups within their sector and had an important, often decisive, influence on policies affecting their sector" (p. 29). Corporatism is a "collaborative but functionally segmented process of policy formation" (p. 188) in which monopolistic corporate interests control access as well as policy.

Harrison seeks to explain the reasons for the growing importance of corporatist modes of decision making and to indicate the implications for contemporary democracies. The book takes the form of an extended essay, with illustrative references to structures and events in a number of Western European countries and the United States.

This analysis finds the origins of corporatist predominance in the ideological consensus and economic prosperity of the 1950s and 1960s. With widespread agreement on social goals and confidence in general economic policy directions, the policy-making process seemed largely one of technical application, appropriately left to governmental bureaucracies. The bureaucracies in turn were encouraged to secure the agreement and cooperation of elites in various social sectors by permitting their participation—and even predominance over—decision making in their sector.

Harrison views the consequences of corporatism in contemporary democracies as doubly negative. One consequence is a decline in participation, representation and accountability in politics. A second implication is poor economic performance. Both the radical protests of the late 1960s and the sluggish industrial performances of the 1970s are presented as primarily the results of corporatist predominance in decision making.

Drawing on the work of Hayek and Galbraith, Harrison depicts the contemporary "mixed" economies of the industrial West as inherently flawed. "The absence of either purposeful central government coordination or of any automatic regulator like competitive market pricing help account for economic problems. . . . The absence of a coordinating mechanism means that the corporations, seeking to regulate their environment, are quite likely to be regulating their own failure" (p. 187). Unfortunately, the analysis does not attempt to assess the simultaneous impact of other domestic and international factors on economic performance. Nor does it explain the relative economic success of some of the countries with highly developed corporatist tendencies (such as Austria and Norway), in comparison to less corporatist systems such as the U.S.A. Rather, British economic problems are placed at the doorstep of corporatism, and ad hoc examples from other countries are used to assign general corporatist responsibility for poor economic performance. The problems of "pure" market and "pure" centralized systems are not discussed.

Harrison is aware of the diverse historical factors shaping the adoption of corporatist decision making in different societies. However, despite the book's subtitle, the historical processes are not emphasized here. The reader is well recommended to turn to the relevant essays of Lehmbruch and Schmitter. Harrison does examine the role of parties and constitutions in limiting corporatist tendencies. In his view parties provide some potential counterweight to corporatist closure in decision making. He points out the dilemma involved in asking party systems to provide both completely open access to power for new groups and an accountable decision process through which established elites can be collectively controlled. Harrison's reflections on parties and constitutions lead him to favor the French and American systems, with loose parties and strong presidencies (p. 112). This preference is not, however, grounded in serious empirical investigation of multiple causes of corporatism or even its pervasiveness in the respective societies. For control of the bureaucracy, the Swedish model is held up for emulation.

In short, *Pluralism and Corporatism* provides a reasonable introduction to the corporatist analytic approach, whose influence has been growing in recent years, and tries to place it in the context of more traditional thought about parties and constitutions. It can thus render a useful service for those who have not yet been exposed to the stimulating insights of the corporatist literature. However, as presented here, its generalizations often seem simplistic in tracing both causes and, especially, consequences of corporatist decision making. Suggestions about linkages are usually plausible, but are not buttressed by rigorous consideration of multiple factors or by systematic empirical comparison. Nor is serious account taken of alternative or complementary explanations of economic and political discontent in contemporary democracies.

G. BINGHAM POWELL, JR.

University of Rochester

Soviet Politics, Political Science and Reform. By Ronald J. Hill. (White Plains, N.Y.: M.E. Sharpe, 1980. Pp. x + 221. $27.50.)

Over the last 15 years, Soviet scholars and commentators have somewhat hesitantly begun to use the term "political science" to describe a valid field of inquiry. Indeed, Soviet participation in the International Political Science Association began in the mid-1950s, a relationship sustained through the Soviet Political Science Association. And yet, there are still no departments of political science in the Soviet Union, or research institutions that bear this label.

Does political science as the West knows it exist in any form in the Soviet Union, however limited? Or is political inquiry limited to fairly narrow legal and institutional research, the exegesis of unchallengeable Marxist-Leninist principles, and bits and pieces of sociological inquiry? Ronald J. Hill has sifted through a mass of Soviet scholarship and documents in pursuit of at least a tentative answer.

One cannot, of course, deny that Soviet research in political institutions is valid political science, since it was not so many decades ago that Western political science was using analogous institutional research as a primary focus. Hill directed his search toward the Soviet use of elementary theoretical concepts, including information, opinion, function, and role. His text is not entirely structured around these concepts, however, which would have made a difficult task at best. His chapter headings divide the inquiry between institutions and concepts: the electoral system, the soviet deputy, and local government reform, a chapter on "Interest, Information and Public Opinion," followed by "Intra-Party Democratization." The findings are summarized in two concluding chapters, one on the process of political reform, and one on the larger problem of political change.

What did the author find? Under each topic, something of interest did indeed turn up. In no area did Hill's search turn up wholly negative results. Regarding the electoral process, for example, several Soviet scholars have suggested procedures for insuring genuine secrecy of the ballot, in place of the long-standing practice of offering curtained polling booths only for the purpose of striking names from the one-candidate slate. On the other hand, there have been only a couple of hints of interest in multiple candidacies, and this in spite of the fact that multiple candidacy has long existed in parts of Eastern Europe. For the most part, Soviet scholars' proposals for reforming local government deal with such topics as how to assure meaningful reporting from local soviet deputies to their constituents, surveys of how deputies apportion their time, how to involve the public in the sessions of local soviets, and what to do about the virtually moribund village soviets. Few of the proposed changes would do anything more than streamline the existing system. The most radical proposal, which actually dates back to the 1950s, would put local administrative departments under the direct supervision of commissions of deputies, thereby erasing the formal distinction between legislative and executive functions, and realizing a feature of the 1871 Paris Commune pointed out by Karl Marx.

What of the Communist party? The one really new development since the mid-1960s is the fact that investigation of the relationship between party and state is now both accepted and promoted. The proposals that have emerged from research in this area, however, focus on delimiting the party's functions by setting forth in detail the roles that party officials and members should and should not play. Discussion of this problem is by itself nothing new; the point is merely that published research has advanced beyond the old generalities. Policy making, leadership and overall supervision remain the realm of the party, which leaves legislation and policy application to the organs of government. The chapter heading "Intra-Party Democratization," is a misnomer, since the text yields almost nothing about democratization in the sense of increasing the scope of electoral and representative procedures. Instead, as Hill himself concludes, the entire emphasis is on greater efficiency and rationalization for the existing system.

Only in the chapter "Interest, Information and Public Opinion" has the author discovered lines of Soviet inquiry which, if they ever take root in the Soviet Union, point to significant change. The concept of "interest" is now well established, certain types of interest conflicts are accepted and studied, and some forms of interest articulation are a subject for research. At least one bold writer (Shakhnazarov) has made a case for greatly liberalized circulation of information.

The book ends on a note of cautious hope for expansion of these trends in political research. From their present involvement in local reform and experiments, Soviet scholars may well move into comparative research among communist nations. Beyond this, the change of political generations may open up further doors. However, Soviet scholarship in this area (as Hill himself concedes) has little impact on either the public or the top leadership. Hill's hopeful conclusions about the future of the young political science profession are somewhat dimmed by the realization that if political change occurs in the coming decades, the present generation of scholars seems fated to follow rather than help to generate it.

ROBERT J. OSBORN

Temple University

Soviet Leadership in Transition. By Jerry F. Hough. (Washington, D.C.: Brookings Institution, 1980. Pp. xi + 175. $11.95, cloth; $4.95, paper.)

In his *Soviet Leadership in Transition,* Jerry Hough has presented a fascinating picture of the accomplishments and attitudes of the leadership of the Soviet Union. During World War I, Russia was one of the more backward countries; its population only mediocre in health, its economic efficiency poor and its standard of living low. In World War I, a much smaller Germany and Austria were able to occupy the richest parts of Russia, and had it not been for the U.S. entrance into the war, might have won and occupied parts of Russia permanently or converted them into client states. Yet in that same period, Russia could boast novels, poetry and music that had won world acclaim. Now, by comparison with the most advanced countries of the world, the Soviet Union's public health is good; it is a great, although hardly the most efficient economic power; the standard of living is much improved; and it is one of the two greatest military powers in the world. How this great change took place is the subject of Hough's thoughtful study.

As with most of humanity, our judgments are rooted in our own experience. We tend to see the industrialization and modernization of Western Europe and some of its colonies as the only possible model, which is in fact far from true. The rapid and extensive progress of Japan yesterday and China today and tomorrow illustrates how special the Western European experience is. The Russian experience has its own unique qualities.

Russia was, and to some extent, still is different from Western Europe and its advanced colonial outposts. The Russian standard of living, and the literacy and technical skills of the bulk of the population were considerably lower than those of advanced countries. But the differences were in degree rather than kind. Even with many of the best-educated and most highly skilled people either leaving the country during the civil war or expelled or imprisoned by the Soviet leadership, in a relatively short time the Soviet Union advanced enormously, with dramatic improvements in living standards and skills of the population. Although the amenities of refined existence are still in short supply, the Soviet Union is not a backward country. The educational level has risen enormously. The scientific establishment, unlike its Tsarist predecessor, concentrates on practical problems rather than pure science. The industrial and political machinery of the Soviet Union, for all its lack of polish and refinement, works well. Its population, whatever their grievances, made loyal and effective troops against a superior foe in World War II. Foreigners from backward countries find much to admire in the Soviet Union, though they may find some of the luxuries of London and New York lacking.

Hough deals with the important aspects of the question of how Russia, which approached the task of modernization belatedly, managed to catch up so quickly and extensively. The work examines mass standards, the administrative elite, the military and foreign policy establishment, economic activities, and the changes in experience and attitudes from one generation to the next. The Soviet Union has gone through and is still undergoing the crisis of industrial and educational modernization in a political regime that has scant regard for the rights of individual expression and thought. Much remains to be done, and from the perspective of the wealthy in advanced Western nations, life in the Soviet Union is not very appealing. Yet in its self-conscious modernization, the Soviet Union has accomplished much, and Hough's book adds greatly to our understanding of that development.

HERBERT S. DINERSTEIN

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Beyond Ujamaa in Tanzania: Underdevelopment and an Uncaptured Peasantry. By Goran Hyden. (Berkeley: University of California Press, 1980. Pp. x + 270. $18.50, cloth; $7.95, paper.)

The Tanzanian experience has held a special fascination for the past 15 years. For many it has been an experiment in creating socialism without totalitarianism; for others it has been important as a development model focusing on rural agriculture, rather than unavailable, capital-intensive, industrial technologies. Politically, Tanzania is a nation where corruption and individual greed have not destroyed the lofty ideals of the independence struggle. Seeking self-reliance and autonomy from the fortunes of the international capitalist system and great power struggles, Nyerere's Tanzania sought to organize development in terms of locally based village development (*ujamaa*), relying on labor-intensive practices and political organization to create rural production units to transform the society. Foreign capital, either in the form of capitalistic investment or foreign aid, was not to be the core of Tanzania's plan and was to be accepted only under certain limited conditions.

Evaluating this course of action is not as easy as some would like it to be. Part of the difficulty is that we first need to figure out what happened in Tanzania since the Arusha Declaration. From this point of view, it is clear that Tanzania neither developed as its leaders wanted, nor collapsed as its critics claimed it would. In both cases there were only errors in predicting macro-outcomes, but more interestingly from the point of view of social science, these are not just due to outside forces such as natural disasters, war, or the rise in the cost of petroleum products. They are also due to lack of understanding about how social, political and economic activities are related inside Tanzania.

Hyden's book is a fascinating answer to this question. He says the Marxists are wrong because they overestimated the impact of dependency in Tanzania; it is neither international capitalism nor petty-bourgeois capitalism at the local level which has impeded Tanzania's effort. Nor is the problem bureaucratic incompetence and nepotism. Rather, Hyden argues that the peasant mode of production in Tanzania is essentially pre-capitalist and sufficiently strong to resist the building of capitalism or socialism, at least for the moment. Their mode of production gives rise to an "economy of affection" which provides the basis for social and political actions which are quite different from those sought by the national leaders. Peasant independence comes about because the government has too little to give the peasant as an incentive for doing what it wants and because what the peasants want is not what the government wants.

In making his argument, Hyden surveys conflict between the government and the peasantry in Tanzania since the onset of the colonial period. First under the Germans, then under the British and finally after independence, peasants have been seen as uncooperative and lazy. They don't do what they are told, and what is more, they don't do what is good for themselves. But Hyden argues that they are not so irrational. Rather, they have managed to maintain their autonomy and continue to promote what means most to them—the values embodied in the economy of affection. Not only does the pre-capitalist economy of affection make mobilization of peasants difficult, if not impossible, but furthermore this mode of organization incorporates many urban workers and members of the petty bourgeoisie who maintain extensive ties to their families in the countryside and continue to own land there as well. These people are "capable of organizing alternative means of sustaining their livelihood. . . . Thus not only are the peasants in Tanzania capable of ignoring state demands but so are the state's own employees" (p. 170).

The small is not so beautiful in Africa, Hyden writes, and for development to take place, contradiction and conflict between the peasantry and those who control the state are required. The rise of modern socialism in Africa means that present means of production which are both pre-capitalist and pre-socialist will have to be destroyed. "As long as the rural producers are absorbed by the social logic of the peasant mode, there is little room for modern socialism" (p. 221). Furthermore, accomplishing the task will require the same use of force that capitalist modernization did. Thus "development in a direction towards greater national self-reliance is improbable without a successful subordination of the peasantry to the demands of the ruling class, whether they are identified as 'petty bourgeois' or 'revolutionary.' . . . Such is the development logic in contemporary Africa. Nobody ought to be surprised if the peasants object to the logic of the process" (p. 232).

I find the argument carefully developed and well illustrated. While some of the strands are difficult to discern at first, they are well woven together by the end. The conclusion, which is certainly not the only one which can be drawn from Hyden's argument, is controversial but provocative as well. He reminds us that good intentions are not enough and that structural conditions cannot be ignored.

MARC HOWARD ROSS

Bryn Mawr College

Labour and Scottish Nationalism. By Michael Keating and David Bleiman. (Atlantic Highlands, N.J.: Humanities Press, 1980. Pp. xii + 215. $37.50.)

Labour and Scottish Nationalism by Michael Keating and David Bleiman is an important addition to the literature on nationalism. Not only does it present a well-documented discussion of the historical connections between trade unions and nationalism in Scotland, but also a detailed case study of a socialist party and its reconciliation with nationalism. Thus the book makes a significant twofold contribution.

Keating and Bleiman are primarily concerned with developing these topiics, not in delineating the history of the Scottish nationalist movement.

They focus on how trade unions and the Labour party have dealt with the issue of home rule and its later derivative, devolution. In detailing those relationships in the early chapters, however, they present a substantial amount of information not readily available elsewhere, particularly for the period 1880-1930. For example, the Scottish Home Rule Society and the various phases of its existence are given extensive consideration. Also, attention is devoted to conflicts within the labour movement itself as different unions and individuals have competed for leadership positions. Their stances on nationalism have frequently been a factor in such bids for power. Much of the information on which these sections are based is drawn from contemporary union records and personal accounts, which contribute greatly to the substance of the work.

Keating and Bleiman found that the commitment to home rule within the unions and the Labour party has undergone circuitous shifts over the years. Initially, there was substantial support for home rule among trade unionists in Scotland and, consequently, among Labour party supporters. Then, as economic conditions worsened after the First World War, making industry-wide unions more desirable than smaller regional ones, and as the viability of a Labour government became more realistic, the depth of the commitment was reconsidered. The focus of support became one of securing more administrative discretion for Scotland than of a separate parliament or dominion status.

In the authors' opinion, it would be wrong to regard these changes as having been made for simplistic electoral or economic advantage. The Labour party and the trade union movement have never had the means for forging a completely consistent ideology. This has meant accepting policies favoring centralized economic planning, while also nominally supporting more localized decision making for Scotland. Any contradictions between these two goals was moderated greatly by the undefined nature of home rule/devolution, the assumption that Labour would govern both Scotland and the rest of the United Kingdom simultaneously, and unpublicized acceptance of the Scottish Office's right to act as Scotland's lobbyist at Whitehall. It is when these factors were altered by the electoral success of the Scottish Nationalist party in the 1960s and 1970s that the Labour party was forced to reevaluate its position on devolution. This matter of reevaluation of support for goals is an important aspect of the book, having theoretical implications beyond this topic. As Keating and Bleiman clearly show, the choice of which goal is to receive current emphasis depends upon many factors, only one of which is concern for electoral consequences. Personalities, economic conditions, historical commitments, and strategies of action, as well as events and conditions external to domestic political control, may have direct impacts on a party or a unions decisions. By illustrating this fact, the authors have notably expanded our understanding of how and why the Labour party has responded to the Scottish nationalists.

As I have stated, this book makes a significant contribution to our understanding of the development of Scottish nationalism, the history of Scottish trade unions, the Labour party, and the theoretical relationship between socialism and nationalism. It is not always easy reading, nor always even in its coverage. The highly detailed early chapters are often heavy going and stand in contrast to chapters dealing with contemporary events. Nevertheless, the book is both substantively and theoretically important. Readers interested in Scotland, British politics, or nationalism should be familiar with this work.

ROBERT J. THOMPSON

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Political Participation in Korea: Democracy, Mobilization and Stability. Edited by Chong Lim Kim. (Santa Barbara, Calif.: CLIO Books, 1980. Pp. xiv + 238. $26.50.)

This book, according to its editor, Chong Lim Kim, intends to contribute to the emerging field of political participation in Korea and to provide a systematic introduction to comparative political participation studies for the case of Korean development (p. xiii). In fulfilling both of these purposes, the book makes a significant contribution.

Broadly defined, political participation as a key political concept means "the participation of individuals and groups in the governing process, supportive and subversive, voluntary and involuntary, involving real choice and no choice" (p. 5). Political participation studies should include inquiries on the socioeconomic and personality dynamics of participants, on the various forms of participation in which they engage, and on the consequences of their participation. The book's wide range, therefore, includes topics as diverse as "Korean Political Participation as Collective Action" (Roger W. Benjamin); "The Modes of Citizen Political Participation" (Chong Lim Kim, Young Whan Kihl and Seong-tong Pai); "The Dynamics of Electoral Politics: Social Development, Political Participation and Manipulation of Electoral Laws" (Jae-on Kim and B. C. Koh); "Political Roles and Participation of Community Notables" (Young Whan Kihl); "Political Participation and Mobilized Voting" (Chong Lim

Kim); "Student Activism: A Comparison between the 1960 Uprising and the 1971 Protest Movement" (Sungjoo Han); "Environment, Policy, and Electoral Participation: A Comparison of Urban and Rural Areas" (Sung-il Choi and Chae-jin Lee); and "Constituency Service among Korean National Assemblymen: A Study of Leadership Responsiveness" (Chong Lim Kim and Seong-tong Pai). Some of these papers were initially presented at a conference held at the University of Washington in February 1975 that was supported by a grant from the Joint Committee on Korean Studies of the Social Science Research Council and the American Council of Learned Societies. Most of the other papers were contributed by the editor.

All of the papers show evidence of careful research as well as conceptual and methodological sophistication. The breadth and scope of the book, however, tends to make each paper stand somewhat by itself without a high degree of integration. Very often, furthermore, the studies work from variable data bases and time frames and cannot avoid some amount of repetition. Because empirical studies are limited by the restricted availability of data, the papers leave many questions unanswered about the nature and scope of political participation in Korea.

Some of the findings are, however, worth summarizing. They include: (1) Higher voter turnouts as shown in Korea are not necessarily a measure of democratic participation (Kim); (2) Rural voter turnout is always higher than urban voter turnout (Kim and Koh); (3) Urbanization correlates with lower voter turnout and a general turning away from support of the government party (Kim and Koh); (4) Regionalism modifies to some extent urban/rural differences in voting and party support (Kim and Koh); (5) Governmental manipulation of the less sophisticated rural voters is extensive (Kim); (6) The mobilized voting bloc is characteristically elderly, female, poorly educated, rural, and low in status (Kim); (7) Participation is hierarchical, depending on how easy it is (Kim, Kihl and Pai); (8) The regime relies on mobilized participation for legitimacy (Kim); (9) Electoral laws are often designed to neutralize anti-government forces (Kim and Koh); (10) Community notables or *yuji* serve as a link between citizens and politicians (Kihl); (11) Students are the only check on Korea's authoritarian regime, and student dissent has become better organized and more radical in goals (Han); (12) Public policies have more influence on urban than on rural electoral participation (Choi and Lee); (13) Korean voting behavior may still reflect the voter's rational choice (Benjamin); and (14) The greater the existing volume of demands on National Assembly members, the greater will be the service provided (Kim and Pai).

These findings may sound familiar to students of Korean and comparative Third-World politics. Perhaps a thorough investigation of the literature in the field could have helped delineate such findings in this volume. The papers have, however, placed these findings in a systematic and empirical framework through the judicious editorial work of Chong Lim Kim, and in this particular regard, the book's contribution is more than sustainable.

This book also challenges us to ask many questions regarding political participation in particular and political development in general. Some unanswered questions concern the use of money and the role of media in electoral politics. Other questions should include why some candidates for National Assembly are successful and some are not. The turnover rate of Assembly members has been relatively high in Korea. Future studies may also include not only mass electoral participation but also more extra-constitutional and functional group participation in decision making in an authoritarian, developing political system as represented by the Republic of Korea.

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The Foreign Policy of West Germany: Formation and Contents. Edited by Ekkehart Krippendorf and Volker Rittberger. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. 372. $20.00, cloth; $9.95, paper.)

This volume, number 4 in the series "German Political Studies" sponsored by the German Political Science Association, might prove of interest to the conservative foreign policy makers clustered around the new Reagan administration in Washington. Although their ideological views will not be in tune with the radical left views of many—but not all—of the young German political scientists who wrote these original essays, the American policy makers may have to contend with such views should the left wing of the Social Democratic party again put its mark on the foreign policy design of Chancellor Helmut Schmidt.

Actually, the authors are less interested in providing policy prescriptions than in seeking an explanation of how West Germany's national interests have been integrated into broader transnational security and economic interests. In the essay on the country's management of security relations, Helga Haftendorn provides a most useful account of the complex decision-making process, noting that domestic political forces, except for organized industry, have had limited interest in or influence on the country's security policies. As a consequence, the bureaucracies and experts have had considerable freedom to recommend or shape policies.

In another essay, Joachim Krause and Lothar Wilker deal more specifically with the bureaucracy's role in shaping foreign policy. They concur with Haftendorf's thesis, but also point out that low-level civil servants can pre-select political alternatives, impede political changes, and prevent the development of comprehensive concepts—even when political leaders draft guidelines and manage crises. Yet these civil servants also must work within the parameter of international economic developments and the interests of Germany's alliance partners.

One major essay, by Frieder Schlupp, concerns Germany's key role in the international political economy. Schlupp argues that the country's economic strength is a mixed blessing at best, because it forces other West European countries into a pattern of instability and dependence, thereby making West Germany an "international villain" in their eyes. These countries also express concern with the key position of the Federal Republic in United States-European relations and in the trilateral world, and resent West Germany's implicit claim that its policy of stabilization is the model that they ought to emulate.

The author also emphasizes the role the Federal Republic plays in the inter-capitalist economy: in international trade and foreign investment, and in the internationalization of its capital. Giant German high-technology firms have captured an increasing share of the world's market, although foreign multinational companies have made inroads into the domestic market. He calls for a "structural transformation of the very economic base, and political superstructure of the "Modell Deutschland' into a societal emancipation and political autonomy" based on a democratic-socialist system "beyond the systemic deformations of both late capitalism and 'real socialism' " (pp. 91-92).

Three essays deal with the Federal Republic's policies toward the German Democratic Republic, the East, and the Third World. Gebhard Schweigler argues that in both Germanys a strong national consciousness is developing, replacing an all-German state consciousness, and killing the hope for an eventual reunification. Michael Kreile briefly traces the foreign and domestic implications of West Germany's Ostpolitik. He notes that improved relations with the Easst Bloc can only be achieved by a detente policy vigorously supported by the United States and Western Europe. Ulrich Albert argues that Bonn has not been sensitive enough to the legitimate aspirations of Third World countries because industrial penetration and Western-style tutelage continue the pattern of former colonial relations, especially in Africa where Germany's interests have not flagged over the decades.

Ekkehart Krippendorff has written a brief introduction to this volume, which, however, as so many other multi-authored works, suffers from a lack of comprehensive and balanced treatment. For instance, the subtitle promises a coverage of the contents of foreign policy, but the reader will find little on Germany's relations with Western Europe, the Atlantic Community and China.

While there are gaps, a lengthy chapter is included on the development of international relations studies in West Germany (Gisela Gantzel-Kress and Klaus Juergen Gantzel) that only has tangential relation to the theme of the volume, although it will be of interest to political scientists. As useful is a bibliography of West German political science literature for 1977-1979 (compiled by Volker Rittberger). Despite these problems of coverage, the radical critique of West German foreign policy found in most essays is path-breaking and a useful corrective to the more establishment-oriented volumes on the same topic.

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Governing Trade Unions in Sweden. By Leif Lewin. Translated by Kjersti Board. (Cambridge, Mass.: Harvard University Press, 1980. Pp. v + 180. $20.00.)

In this book, Leif Lewin reports research designed to test whether the Swedish trade union movement (LO) is governed through "interactive democracy." The data are 1974 mail survey results from a sample of approximately 2300 members and 650 officers drawn from 50 locals, plus 250 central officers. The research was funded by the Bank of Sweden Tercentenary Fund.

Lewin devises "interactive democracy" as a way out of Michels' dilemma. Lewin writes that "oligarchy within democratic organizations can be avoided if the interaction between leadership and membership functions in such a way as to develop the public spirit of the individual" (p. 16; *viz.* J. S. Mill). On page 54 Lewin reveals that public-spiritedness is membership support for LO's official wage policy of solidarity.

Interactive democracy requires both political participation by the members and "creation of opinion by the leadership" (p. 20). It is not enough, or even desirable, that leaders perfectly reflect their constituents' preferences. Elites should "in some cases be required to go against public opinion" (p. 19). Further, "competition [among elites] cannot be viewed as a sufficient condition for democracy, and may not even constitute a necessary condition. . ." (p. 25).

Since Lewin sampled first the 50 locals and then members and officers within each local, he is able to score each local on the three dimensions he says are critical to interactive democracy: *opinion formation* (member and leader activity, competence of members and leaders, and "system orientation"), *consensus* of members and leaders on union priorities and values, and *authority* of leaders (ability to take action without protest).

Locals which satisfy criterion values for opinion formation, consensus, and authority are "democratic" (26 percent of the sample). Locals below each of the three criterion values are "passive" (30 percent). "Impotent" locals (14 percent) lack authority, "manipulative" locals (14 percent) lack opinion formation, and "therapeutic" locals (16 percent) lack consensus.

Public spirit of each local is scored by the members' responses to three questions supposedly probing the members' internalization of the wage policy of solidarity. "Democratic" locals score either high or medium on public spirit whereas about half of the "passive" locals scores low. (It is also true that half of the "passive" locals scores medium or high.) The conclusion is that "democratic" locals generate public spirit.

This study is so badly flawed that its conclusions must be rejected.

Cross-sectional data are inappropriate for testing a model of member-leader interaction. Lewin asks members and leaders about their generalized activity levels in 1974 and relates this activity to support for a norm that has been official LO policy for 20 years. Case studies providing process data, rather than cross-sectional survey data, are necessary to evaluate Lewin's model of interactive democracy.

Lewin relies on reported activity even where behavioral data are available. Lewin reports that " 'voting in union elections' is not indicated as a measure of activity, because . . . the claim of voting participation was so exaggerated that the measure proved useless. . ." (p. 173). Lewin's peculiar solutuion is to ignore the actual voting data and rely instead on member reports on how often they attend meetings and whether they believe in worker solidarity. In another case, however, Lewin has it both ways. He reports that about 5 percent of LO membership participates in study circles (p. 48) but accepts without comment that 11 percent of his representative sample reports participation in study circles (p. 78). Lewin opts for unreliable survey data when hard bureaucratic data are available.

The operational meaning of the dependent variable, public-spiritedness, is garbled. Responses to three different interview questions are added together to give each local a public spirit score. Seventy-eight percent of the members agree with the normative statement that higher-paid groups should support low-income earners even if this means that their own demands are set aside. Only 47 percent, however, say that they personally would forego a raise to benefit low-income workers in other unions. The third question is whether "it is wrong of LO to make such wage demands that the least efficient industries are forced out of business" (p. 76). Forty percent disagree. It is not obvious that these questions are equivalent. No intercorrelations are provided to allow the reader to assess whether the questions tap different dimensions, in which case an averaged score is meaningless.

There is no independent measure of public spirit. Consensus, an independent variable, is constructed from, *inter alia,* the same questins used to score public-spiritedness. It is not surprising that 63 percent of the "therapeutic" locals (which lack only consensus—"therapeutic" locals have both opinion formation and authority) have low public spirit. On the other hand, of the 27 locals "having" at least consensus, only 2 have low public spirit.

Authority, one of the three constructed variables explaining public spirit, is conceptually vague and operationally useless. We are told on p. 27 that "union leadership enjoys authority if it is able to act upon the decisions made without encountering protests from the members," which is quite different from the statement on the following page, "Authority signifies that union policy is approved because the leadership has convincingly argued its case."

For Lewin, the *local* leadership's authority is the percentage of its members which both does not agree that "the union managed to derive maximum benefits from the latest [centrally] negotiated agreement" and claimed *not to have noticed"* wildcat strikes or protests at their workplace (p. 108, emphasis added). This leads to the peculiar observation that in one local, 8 percent of the workers noticed wildcat activity while the remaining 92 percent, presumably at the same workplace, did not (p. 124). Lewin accepts the 8 percent as the actual extent of wildcat activity (pp. 109, 124).

Calculations of local scores for the other two independent variables, opinion formation and consensus, are mindless number crunching. Opinion formation, for example, is constructed from 31 separate questions, each assigned a different numerical value. The most elementary rules for index construction are ignored. The analysis is replete with double counting, averaging apples, oranges and potatoes, and mistaking horses for zebras. Whatever opinion formation may be conceptually, it cannot emerge with any validity from this construction. If, for example, opinion formation in a model of interactive democracy is supposed to reflect membership activity, it must be conceded that membership activity items account for only about one-ninth of the summary score (pp. 116, 122). Having "attended a union meeting three times or more," "contributed to discussion," and "discussed union matters with a committee member" together are worth as many points as being able to name the union president (p. 116). Also, the sample calculation of the opinion formation score on page 122 is incorrect.

Lewin on page 95 concludes that "competition does not further legitimacy" after finding that 75 percent of the members of locals with competitive elections are critical of union elections (versus 72 percent of the members in locals without competitive electins). Nearly all of the members' criticisms of union elections can as easily be dissatisfaction with the election results as challenges to their legitimacy (p. 92). It may be that the problem with competitive elections is that someone loses.

This book should not have been translated. Specialists are already familiar with the book in Swedish. It is a pity that other readers will learn very little about the Swedish labor movement from the book, and much of that will be misleading. Still, Kjersti Board deserves credit for a true and readable translation.

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Arabs in the Jewish State: Israel's Control of a National Minority. By Ian Lustick. (Austin: University of Texas Press, 1980. Pp. xii + 385. $19.95, cloth; $10.95, paper.)

Arabs in Israel have been a vastly neglected group in the large body of literature on Israel and its political life. Ian Lustick's study is the first major, systematic analysis by a political scientist of Israel's policies and practices toward the 15 percent of Israeli citizens who are Arabs. It brilliantly focuses on the causes behind the low level of political activity on the part of Arab citizens of a country surrounded by hostile Arab neighbors, with important internal political divisions, with substantial grievances on the part of its indigenous Arab population on issues such as land expropriation, and unequal educational and occupational opportunities. Though formally possessing full civil rights, Arabs in Israel neither have an independent political base nor have they been able to establish a mass-based civil rights movement. They control no independent newspaper and in effect possess no significant leverage in the political process.

Lustick argues convincingly that this political quiescence does not derive from the failure of an inept traditional leadership to use effectively the resources of the political system to extract concessions and derive greater benefits. Quite the contrary, Lustick's thesis is that "the failure of Israel's Arab minority to 'organize itself' and the minimal significance, to date, of the communal segmentation of Israeli society of the operation and stability of the Israeli political system are due to the presence of a highly effective system of control which, since 1948, has operated over Israeli Arabs" (p. 25). The purpose of the book is not to decry this system but to describe how it has worked to serve the goals of a regime which serves the interests of the dominant Jewish population; to explain why the strategies used have been so successful, notwithstanding the considerable resentment and potential resistance of the Arab minority, and to consider the possibilities of changes in that system in the years ahead.

Lustick presents a history of the Zionist movement prior to 1948 in terms of how it dealt with the question of the role of the non-Jewish minority in the posited Jewish state, and how the new government responded to the problem of the Arab community that remained within Israel's borders after independence. The imperatives of that new relationship centered on five major concerns for the Jewish state: security, territorial consolidation, immigrant absorption, economic growth, and political stability. In each of these areas Israeli policy was fueled by the primary goal of controlling the Arab minority rather than integrating, absorbing, developing or eliminating it.

The analytic framework for the comprehensive examination of the control system focuses on three levels of analysis—structural, institutional and programmatic. Three components of control —segmentation, dependence and cooptation are identified and described in separate chapters.

Segmentation was implemented through the encouragement of structural fragmentation of the Arab minority along religious and clan lines, through demographic isolation in population enclaves, and via the nearly 20-year military government over the Arab sector, through a policy of unequal access to the resources of the country. Lustick concludes that segmentation served to deprive Arabs in Israel of "facilities for united political action, whether involving alliances among Arabs on a country-wide basis or between groups of Arabs and politically significant groups of Jews" (p. 149).

Dependence as a component of control is analyzed in terms of the sustained, massive and unavoidable reliance of Israeli Arabs on the Jewish sector for education, work permits, status, and all the essentials of economic, social and political resources. The Arab population totally lacks a viable, autonomous basis of economic power, not fortuitously, but by regime intent. This has made them, not surprisingly, quite vulnerable to noncoercive forms of economic pressure and reprisal.

Lustick suggests that while segmentation and dependence have become institutionalized, *cooptation* of Arab elites has not. Rather, those who are subject to cooptation—sheiks, school administrators, scoutmasters, and others—have been used in a highly instrumental and summary fashion. Cooptation is based on material inducements, threats of material deprivation, and the claims of self-interest, rather than any effort to socialize coopted elements into the dominant ideology or bring them into the mainstream of Israeli life. It is quite clear that for the Arab population in Israel, the doors of advancement are structurally limited throughout the society. Cooptation serves mainly to capture the existing Arab elite and to weaken the incipient forms of leadership that might otherwise spearhead articulation of needs and interests of their potential constituency.

In the concluding chapter, Lustick interprets the synergistic relationships among the three factors. He finds that the system of control has been very successful in achieving the regime's goals at minimal costs of resources and effort. He predicts that although there are not likely to be any massive changes in the near future, the regime is likely to have to put more effort into the control system in order to maintain its position. He believes that Israel's governing elements will eventually have to decide between continuing the prevailing control system (the Toledano-Histadrut line) or opting for the "tough," repressive policy position (the Linn-Sharon line). In either case, he thinks, costs are going to escalate and in the long run the quiescent state of the Arab community will not continue. Internal developments and changes in the external environment will bring about a significant shift in Arab political activism.

One of the great virtues of this work is that it avoids the academic shoals of apologia on the one hand, and of radical polemics on the other. Lustick's study weakens the argument of the majority of writers on the subject, that the Arabs in Israel are a modernizing and adjustable minority in a Western society; that the new state has significantly improved the overall position of its Arab citizens, and made an honest effort to make them equal before the law. At the same time, it eschews the simplicities of those who see Israeli Arabs as part of an oppressed Palestinian national minority in an exploitative and "racist" Zionist state. Lustick uses a conflict perspective in his analysis and presents Arabs in Israel as a vulnerable, controlled, nonassimilating and underdeveloped national minority in the Western, Jewish, Zionist state.

This comprehensive study is meticulously researched, cogently argued and lucidly presented. Lustick builds his argument on the extensive use of government documents, Hebrew newspapers, Zionist archives, and interviews with prominent Arabs and Jewish officials in government, political parties, labor unions, and non-governmental Jewish agencies. He is commendably careful not to sit in judgment. The book, though very well written, possesses an element of redundancy that may grate on those familiar with the subject. Another minor flaw is the inadequate attention paid to some of the more "hidden costs" of the Jewish state's control policies for both Arab and Jewish citizens in terms of morale, personal and national identity, social consciousness and sense of justice.

This study will not please those who see Israel as the unblemished bastion of democracy in the Middle East, nor those who are persuaded that it has treated its Arab minority in an exemplary fashion. Though this book may provide ammunition for the critics of Israel, at some point it must be said that there is considerable value in having this thorny terrain examined in a calm, scholarly and dispassionate fashion.

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The Making of a Stormtrooper. By Peter H. Merkl. (Princeton, N.J.: Princeton University Press, 1980. Pp. 328. $16.50.)

The Hitler Movement: A Modern Millenarian Revolution. By James M. Rhodes. (Stanford, Calif.: Hoover Institution Press, 1980. Pp. 253. $14.95.)

The two books reviewed here are very different in both theme and methodology, but they are part of a by-now voluminous yet still growing genre: the literature on why and how the Nazis and Nazism. Both Merkl and Rhodes accept implicitly that the Third Reich remains a historical phenomenon that is, if not *sui generis,* of far greater import than the mere substitution of one group of politicians for another, as Lord Rothermere once put it. In addition, the two works also share parallel documentary bases. Both are largely synthetic accounts, relying heavily on reinterpreting known literature and sources, but Peter Merkl and James Rhodes do make use of one major archival collection, the so-called Abel collection at the Hoover Institute at Stanford University. As a historical source, this group of documents has some problems (about which more in a moment), but this is not to deny its usefulness for both books.

Rhodes takes what might be called a theo-philosophical approach. He seeks to find a proper ideological categorization of Nazism and the Nazis, and is doing so to explain both motivation and socio-political success of the Nazis. These questions, as Rhodes readily admits, are hardly new, but he feels that all previous causal explanations of the phenomenon (the societal breakdown effect of World War I, the lower-middle-class thesis, the German national character fault, etc.) have been inadequate or contradictory. Merkl's aim is somewhat more modest. He is concerned with the motivational forces and internal categorizations of the largest and most violent segment of the NSDAP during its movement phase, the storm troopers (SA). These politicized paramilitary units played a major part in bringing Hitler to power, though they were quickly eclipsed in the power struggle with Himmler's SS after 1933. Like Rhodes, Merkl is not satisfied with the previous explanations of the reasons that led to membership and activism in the SA, and he attempts by a variety of sophisticated quantitative and qualitative methods to delineate different chronological segments in the SA's membership and to correlate these age groups with prevalent types of activity (from primarily poster-pasting to a near-exclusive concern with political brawling).

Methodologically, both authors attempt to rest their thesis upon a combination of macro-documentary bases and the use of direct, personal testimony of rank-and-file Nazis. For the former, Rhodes uses philosophical and theological sources reaching back to the visions of John in the *Book of Revelations* of the New Testament; Merkl makes abundant use of the literature on the Weimar Republic and on political motivation in mass movements. For their micro-sources, both authors rely heavily on the Abel collection. At this point it might be useful to express a few caveats about this collection of documents. It consists of 581 testimonials (originally 681, but 100 were removed from the collection by the FBI in the early 1950s and have been lost since) of NSDAP members regarding their reasons for becoming Nazis and their activities as party or SA members before 1933. The statements were solicited (under the pretext of a contest for the best essay) shortly after the Nazis came to power by the sociologist Theodore Abel, who first used part of the information in his book, *Why Hitler Came to Power* (1938), reprinted under the title, *The Nazi Movement* (Atherton, 1965). Merkl has subsequently made much more extensive and sohisticated use of the materials in his work *Political Violence under the Swastika* (Princeton, 1975). The Abel documents have the advantage of being a primary source written shortly after the events described, and they are the expressions of genuine rank-and-file Nazis, rather than pronouncements by the leadership. (Few of the "Abel-Nazis" achieved any prominence in the Third Reich.) On the negative side, they are heavily weighted in favor of pre-1930 members, geographically unevenly distributed, written in the immediate flush of political victory before the *Katzenjammer* that set in by 1935, and there is of course no means of verifying the factual statements. As I have noted previously (*Societies*, 8 [1978], 71-73) there is considerable evidence of factual and motivational embroidering. Incidentally, it would be most interesting to compare the statements submitted to Abel with the same individuals' account of their lives given in the course of their de-Nazification proceedings after the war.

Both authors present easily comprehensible, vigorously defended theses. Rhodes contends that the Nazis were essentially a secular millenarian movement, and, except for the adjective "secular," comparable to any number of previous attempts to create utopian societies on earth. Rhodes takes a somewhat unique approach in that he sees millenarian movements not merely as socio-historical phenomenon, but as forms of sin and heresy directed against a divinely ordained, Christian-oriented societal order and morality. In supporting his thesis, Rhodes cites numerous millenarian authors beginning with John of *Revelation* to establish the characteristics of millenarianism. He then uses statement from Nazi propaganda and the Abel collection to illustrate the specific Nazi forms of this disorder.

It is difficult to "review" Rhodes' thesis, since he argues outside the established conceptual framework of the social sciences. He proceeds from an essentially theological and teleological standpoint which holds that not only factual statements are verifiable, but moral and political beliefs are objectively right or wrong as well, because within their sphere divine law sets absolute standards. Thus as millenarians the Nazis were wrong because they should have known that all millenarianisms are Christian heresies. Rhodes points out, for example, that those Germans who expected the pre-World War I prosperity to continue were not just naive, but objectively wrong in their belief: genuine fulfillment can never be achieved in this life, but only after death in the presence of God. Any other expectation is a hope for false salvation and therefore heretical.

While Rhodes' reasons for condemning millenarians are largely matters of faith and thus not subject to the usual tests of social scientific verification, it is possible to examine the methodological path he travels to reach conclusions, and here a number of major questions seem appropriate. Rhodes returns to viewing Nazis and Nazism as a monolithic bloc undifferentiated by either factions or chronological segments. This view, it seems to me, empirical research has long since disproved. Moreover, in order to cover all observed characteristics of Nazism with the millenarian umbrella, Rhodes is forced to enlarge the concept almost beyond recognition. If, for example, calling for an end to corruption in public life or worship of military heroes are typically millenarian sentiments, then clearly all politicians in a modern mass context are millenarians. In essence, Rhodes' book, while laudable in its aim to condemn Nazism as immoral and sinful, suffers from too many questionable conceptual and methodological assumptions to become a major contribution to the historical literature on Nazism.

Merkl's work, on the other hand, fits into the mainstream of social science methodology and conceptualization. The author shows that far from being a monolithic body of ideologically committed men, the Nazi storm troopers were composed of three quite distinct age groupings: those old enough to have served in World War I, a second group too young to have fought though they did experience the war consciously ("the victory watchers"), and a third cohort composed of those who were essentially still children before 1918. In addition, Merkl is able to correlate political attitudes and activities with each of the age groupings. This does not mean, however, that the SA members as a whole do not show some distinguishing characteristics: as a group they can be differentiated from the party membership in terms of both activity and motivation, and the primary motivational force for joining the storm troopers was not economic hardship or ideological conviction, but a sense of alienation resulting partly from the *Zeitgeist* of the Weimar Republic but more significantly from personality problems of the individuals. The SA members, in other words, were misfits, and Nazism gave their asocial tendencies the sanction of political sacrifice and national service. The extraordinarily large number of such misfits among that generation of Germans Merkl attributes to a series of national shocks that confronted this generation of Germans; the defeat in World War I, the post-war Freikorps operations the chauvinistic reaction to the Ruhr occupation—all this led to a very high degree of violence and para-militarization of Weimar politics.

It is difficult to quarrel with Merkl's overall conclusions; his thesis is empirically well supported. The difficulties with using the Abel collection remain, but unlike Rhodes, Merkl is clearly aware of the caveats and tries to compensate for them. I do think Merkl may be painting the paramilitary background of Weimar politics in too stark colors. It might be argued that viewing all 15 years of the republic as a unit is historically unwarranted as is lumping the SA and the Reichsbanner Schwarz-Rot-Gold into the same category of para-military organizations. Membership and motivation of the two groups was really quite dissimilar. A few minor items of style and formulation are jarring. The description "Prussian-ruled Poznan" for the province of Posen in 1914 seems to give the social scientist the job of retroactive political rectification. After all, Posen at that time had been part of Prussia since the 1780s, and by that standard South Dakota could still be styled "American-ruled Sioux territory."

But these are minor items of criticism in an otherwise important addition to our knowledge of Nazism. Not least among Merkl's contributions is to disprove Rhodes' thesis. The storm troopers were clearly not a bloc of millenarians, but variegated social misfits who joined the SA for a number of reasons, among which the desire to engage in physical violence was prominent. Merkl's book demonstrates yet again that attempts to explain Nazism and Fascism in terms of mono-causality, including that of Rhodes, remain unconvincing.

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Palestinian Society and Politics. Edited by Joel S. Migdal. (Princeton, N.J.: Princeton University Press, 1980. Pp. xv + 290. $20.00, cloth; $6.95, paper.)

Multi-authored volumes tend to suffer from substantial gaps, even when the editor provides an overview and commentary to bridge the individual essays. Nevertheless, such volumes can usefully collect related writings that would otherwise be scattered across different journals or available only in dissertation form. This is true of the study under review, since many of the essays are based on primary documents from relatively inaccessible Israeli archives and several are excerpts from unpublished Ph.D. dissertations.

Palestinian Society and Politics contains an extended essay by Joel S. Migdal and eight chapters by Israeli and American scholars. The volume seeks to cover a broad range of issues, but actually deals with a limited number of themes and a constricted geographical context. The analyses of the pre-1948 period take the entire mandatory territory as the geographical unit, but those covering the post-1948 period concentrate virtually exclusively on the West Bank. They thereby omit the majority of the Palestinians, including those who experienced the most severe disruption in 1948 and who now lead the Palestinian political movement.

Moreover, the essays are primarily concerned with village society. While serving as a useful corrective to studies that emphasize elite politics and ignore the lives of the peasants, they tend to overstate the importance of villagers in Palestinian political life. The study thus contains several interesting but esoteric micro-analyses on the village mukhtar (Gabriel Baer), "The Impact of British Norms on Arab Community Life" (Ylana N. Miller), efforts to protect cultivators in Mandatory Palestine (Kenneth W. Stein), internal labor migration in the 1940s (Rachelle Taqqu), and a comparison by Migdal of social cleavages in 10 West Bank villages in 1973.

Although the impact of British politics on the peasantry is delineated, the essays tend to understate the impact of the Jewish presence on the Arab community and the inability of the Arab sector to compete with the Zionist movement economically as well as politically. Nonetheless, Migdal does comment that the presence of the British rulers and the Jewish community impeded the unfettered development of a large, strong Arab middle class. Miller contrasts the impoverished Arab villages of the 1930s with the effectively organized Jewish community, and Taqqu notes the wage disparities between Jewish and Arab laborers in the 1940s. Moreover, Stein presents an intriguing but incomplete analysis of the symbiosis between the Jewish land-purchaser and the Arab land-seller who frequently colluded to circumvent British legal measures intended to protect tenants against eviction.

Several essays point out the contradiction between the aims of ruling regimes and the actual outcome of their policies. Migdal argues that the British allied with the urban, landowning elite but pursued policies which weakened that elite's bases of power and accelerated the fragmentation of village society. Jordan was more successful in coopting the traditional elite into the state apparatus. While Israeli military rule sought to weaken both the traditional elite and the radical intellectual counter-elite and to preserve only local administrative structures, its economic policies inadvertently caused substantial social change. Baer supports this argument by describing the contradictory effects of increasing administrative and security demands on the village headman. Taqqu also depicts the unpredicted social impact of mobilizing village workers during World War II, and Shaul Mishal details Jordan's methods of simultaneously co-opting and fragmenting the Palestinians on the West Bank. Mark Heller traces Israeli policy since 1967: by denying a mediating role to the Jordanian-backed notables and refusing to let any representative framework be established on the West Bank, that policy resulted in not only the radical counter-elite but also the traditional elite actively opposing the occupation.

In analyzing current Palestinian politics, Heller asserts that neither Israel nor Jordan can authoritatively allocate political values on the West Bank, and only the Palestine Liberation Organization can effectively control the Palestinians' political and moral resources. This argument is reinforced in the essays by Shimon Shamir and Donna Robinson Divine. Shamir, interviewing refugees in a West Bank camp in 1968, found Palestinian identity to be the dominant value and Jordanian identity the weakest. Divine, observing more broadly the problems created by the absence of political independence, argues that subjugated societies contain no paramount political power center around which the system pivots. The PLO represents a significant departure from previous Palestinian political organizations, she maintains, in that it has reconstituted a national elite and a political center, even though social cleavages and geographical dispersion still persist and contribute to limiting the PLO's effectiveness.

Migdal has assembled a provocative collection of essays which can be used effectively by the researcher who already has an overview of Palestinian history and knows how to interpret and integrate these diverse selections.

ANN M. LESCH

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Values, Identities, and National Integration: Empirical Research in Africa. Edited by John N. Paden. (Evanston, Ill.: Northwestern University Press, 1980. Pp. viii + 397. $37.95.)

This book is a reflection of things past and, perhaps if we are lucky, a harbinger of things to come. Ably edited by John Paden, this collection relates research undertaken on national integration in Ghana, Ivory Coast, Kenya, The Gambia, Nigeria, Cameroon, Liberia, and Zaire. A product of the National Unity Project of Northwestern University, this volume contains, for the most part, essays drawn from the doctoral dissertations of students in sociology and political science. It is thus very much an "in-house" product.

Paden's introductory chapter both summarizes the findings of each contributor and surveys the relevant theoretical underpinnings (systems theory) which inform the work. Values and identities are identified as key components of national integration and defined. Values are seen as "modal patterns of preference which characterize broad segments of a population" and identities as "the ingroup and outgroup ascriptions of 'labels' or 'names' to aggregations of people which may have social, political, or economic relevance" (p.

5). The key question, asked by each of the researchers in his or her own style, is "what are the patterns of values and identities, especially at a 'mass' level, which affect orientations toward authority, community, conflict resolution, and innovation?" (pp. 3-4). With this as a common starting point, the authors propose to examine the relationships between values, identities, and national integration; to explore the notion of ethnicity through its empirical manifestations; and to provide data for other cross-national studies which often omit Africa from their purview (p. 5). Although there was no formal, common research design, the contributors had extensive contact with each other at all phases of the project and the final product reflects this. Unlike many edited collections, this one does hang together and the authors successfully address a common theme.

This is not to say, however, that all of the contributions are of equal quality. Joseph Kaufert's work on situational ethnicity among university students in Ghana is particularly noteworthy. He specifies population density, mobility, heterogeneity, disproportional representation, and economic differentiation (p. 56) as key variables which interact, in different circumstances and contexts, to produce identities based on kinship, town of origin, language groupings, regional origins, and nationality. His research is both subtle and informative and is one of the few attempts to provide a systematic categorization of possible ethnic identity outcomes. Kaufert recognizes, correctly, that much more effort needs to be put into an inventory of situations. His study is merely a starting point, but a very good one.

The chapters on ethnicity in Cameroon by Ndiva Kofele-Kale are also worthy of sustained attention. In a compelling exploration of political culture in English-speaking Cameroon, the author creatively weaves quantitative data with extracts from unstructured interviews. Confident in his use of both methodologies, he is a prisoner of neither. (Regrettably, not all of the contributors to this volume are as skillful.) Kofele-Kale attempts to measure patterns of authority in family and ethnic group and his results deserve a wide hearing. He finds for example, that "rural ethnic man and urban ethnic man are essentially the same with regard to exposure and attachments to ethnic group values" (p. 149) and that loyalty to nation, on the one hand, and ethnic group on the other, are not necessarily mutually exclusive (p. 170). These essays on Cameroon also stand out because they consider certain social class considerations and note the differences in perceptions of government among urban, semi-urban, and rural people. Those in the countryside have a much more negative view of government largely because they have been the victims of an increasing gap between rich and poor (p. 154). Kofele-Kale has furnished us with two methodologically creative, intellectually rigorous, and theoretically relevant essays.

Amos Sawyer's chapters on Liberia are also recommended for they are the only ones in this volume to deal explicitly with social stratification. They are all the more important in light of the recent military coup and overthrow of the dominant Americo-Liberians. His conclusion that "the effective utilization of the institutions of the political system in the interests of the people of the system, and the fair and equitable distribution of the goods and services of the system play no small part in instilling in the population the value predispositions essential for national development" (p. 320) might once have been read profitably by President Tolbert.

Despite the generally high quality of these essays and the occasional outstanding research reports cited above, I concur with Ronald Cohen's epilogue to this volume. Unfortunately, the "inductive behavioral procedures did not lead to major breakthroughs in macrotheory" (p. 326). The question then becomes one of future research orientation. Paden's conclusion argues that replication is necessary not only to check reliability, but more importantly to provide the time-series data so crucial for the comprehension of any social change (p. 359). While this position makes sense, it is simply not enough. Future work must not only replicate what has gone before, but should also expand into new realms. Cohen indicates that forthcoming studies of internal processes must also consider the external factors pinpointed by theorists of dependency. He further argues that while ethnicity and regionalism are likely to remain salient axes of political conflict in Africa, we should also pay attention to the rural-urban continuum and social class. It is in the failure of most of these authors to treat social class as a component of national integration that I find myself most at odds with this volume. Their work, with some exceptions, is important and well done. It is, however, too narrow. Cohen's thoughtful essay on the future of socio-political research in Africa should be read by all working in this field.

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Democristiani. Edited by Arturo Parisi. (Bologna: Societa editrice il Mulino, 1979. Pp. 231. Lire 6,000.)

This volume gathers efforts of scholars well-versed on the Italian Christian Democrats. In the aggregate, their essays bring us abreast of important changes, especially at the level of orientations, occurring within the DC community as it attempts to cope with new, oftentimes unfavorable, changes in its socio-political environment. Among the matters considered are DC member attitudes towards their own party, the Catholic world, and their main adversary, the Italian Communist party. The essays offer the reader a rich body of current data which is insightfully interpreted. Overall, the DC is portrayed as undergoing, somewhat reluctantly, an evolutionary process that is altering its character as a party and its role in the Italian political system. A careful reading of the *Democristiani* will provide ample rewards. Brief glimpses of its contents follow.

The essay by Maurizio Rossi charts the rise, fluctuations and partial decline of the membership of the Christian Democratic party. In response to the organizational challenge of the Italian Communists, and anxious to reduce its political dependency on the associations of the Catholic Church, the DC leadership recruited more than 1½ million members during the late 1940s and early 1950s. Thus, and in spite of the perennial inflation of the DC's official figures, there can be little doubt about the mass-based character of Italy's dominant party. At the same time, in the view of Rossi, the DC never fully achieved its aspiration to be an "inter-classist" party: it remains basically a middle-class party—notwithstanding its efforts (partially successful) at broadening its social base of member recruitment.

The overall success of the DC in mobilizing a mass membership has not, unfortunately, been accompanied by similar success in meaningfully involving its members in the internal life of the party. Indeed, in the considered judgment of Douglas Wertman, the attitudes and comportment of the DC's leadership have tended to discourage the party's rank-and-file from assuming an effective and meaningful role in the party structure. The lack of responsiveness of national leaders of the party to the activity of members at the local levels has undermined its value, leading ordinary members to conclude that their participation in the party is inefficacious. This negative perception then blunts their interest in and commitment to the party. When other factors are added—the indirect recruitment and hence weak ties of women, the retarding effect of clientelism, and cultural norms which discourage female political activism—the level of participation in the DC is further reduced.

After mapping out the size, social composition and participatory profile of the DC's membership, the inquiry turns to the relationship between party members and organized Catholicism. Using attitudinal and behavioral indicators, Arturo Parisi concludes that the DC is still a "party of Catholics"; e.g., 53 percent of the members are "regular practitioners," a quota triple that of the general population, greater than that of any of the other parties, and far in excess of the quota for PCI members. Moreover, Catholic militants who are politically active and belong to a party are overwhelmingly Christian Democrat. Parisi notes, however, that important changes are occurring among younger generations: they are becoming less "Catholic" and less accepting of the DC. To survive politically, Parisi concludes, the DC is relying less upon its appeal to Catholics and more on its "catch-all" character.

Some corroboration of this trend is found in the essay by Piero Ignazi and Angelo Panebianco. Their data indicate that the confessional perspective among DC members is definitely on the wane. The clerical right ("Traditionals") and clerical left ("Integralists") account for only one-fifth of the party's membership. The lay or secular perspective is now shared by a preponderant majority of the party, half of whom are characterized by Ignazi and Panebianco as "left" (prefer political formulas which include the PCI; feel "close" to parties of the left), and the other half as "right" (favor PCI exclusion and feel antipathy towards the left). The members of the two groups—the "Democratics" (lay-left) and "Conservatives" (lay-right)—are mainly well-educated, enjoy high professional status, do not observe religious practice, and are younger than the clericals of the party. Furthermore, their numerical strength and personal and social resources assure them a place of power in both the party and in the elective/administrative arena, from which the clericals have been largely displaced. But although the religious ties of the bulk of Christian Democrats have been weakened, so long as the Conservatives remain strong, the posture of the DC towards Catholic interests, as well as towards the PCI, will change only slowly.

But change it will—especially with respect to the Italian Communists, as Gianfranco Pasquino ably demonstrates. In fact, sizeable minorities now favor (41 percent) and/or predict (46 percent) DC coalitions with the PCI (with the latter preferably in a subordinate role). This does not imply that Christian Democrats have found new sympathies for their old adversary; it is, rather, a manifestation of the political realism and operational pragmatism which has for so long characterized Christian Democrats and has contributed to their success, over the course of more than three decades, in maintaining a position of dominance in the Italian political system.

FRANK BELLONI

Virginia Commonwealth University

Mobilità senza movimento: Le elezioni del 3 guigno 1979. Edited by Arturo Parisi. (Bologna: Societa editrice il Mulino, 1980. Pp. 160. Lire 8,000, paper.)

The title of this book accurately captures the dilemma its contributors face. After five years of what appeared to be considerable movement on the part of the Italian electorate, the 1979 general elections suggested, at first glance, a shift away from the fluidity of the seventies. In spite of this apparent change, and despite numerous differences of interpretation within the present collection, all five authors agree that we are not witnessing a return to older patterns. Something changed in 1974, and the absence of dramatic movement in this particular election does not indicate an immobile or static electorate in Italy.

This central proposition is easy to accept, especially since the contributors offer qualified arguments and numerous caveats concerning the various measures they employ to analyze recent voting patterns. Moreover, aside from its own considerable merits, this volume is welcome as part of a much larger and longer-term undertaking, supported by the Istituto Carlo Cattaneo, which should provide a comprehensive overview of recent Italian electoral behavior.

Arturo Parisi's introductory essay gives coherence to the collection. He furnishes a useful synthesis of the proliferating interpretations (many of them mutually contradictory) of recent electoral fluctuations, and offers a number of highly persuasive arguments to support the core thesis spelled out above. He admits that no definitive conclusions can yet be drawn, and emphasizes the need to integrate different types of analysis into studies of voting behavior. Especially important, in light of recent Italian political history, is the emphasis he puts on analyzing the parties' activities in a period of increased voter mobility.

In view of Parisi's admonition, it is a pity that only Gianfranco Pasquino's analysis of the Socialist party fully integrates explicitly strategic and political issues into the broader electoral analysis. Like the others, Pasquino shows how the vote has a distinctive logic in different areas of the country, a logic often washed out by correlation coefficients applied only to the national level. But, in an essay rich in suggestions and interpretations, Pasquino is also able to demonstrate the interaction between the variegated vote and the behavior and intentions of the Socialist leadership.

The big loser in 1979 was the Communist party, and Renato Mannheimer's territorial analysis of its losses helps put them into perspective. Here again, the importance of different voting patterns in different areas of Italy is very effectively underlined. In some instances, national patterns are clearly discernible—e.g., the PCI lost most in 1979 where, in 1978, the referendum on the public financing of parties showed the greatest resentment against the party system. But even more frequently, different patterns emerge: the drop in voter turnout hurt the PCI in southern Italy, but not elsewhere; the Radicals were able to capture disaffected Communist voters in central Italy, and especially in the north, but not elsewhere.

The other contributors also emphasize the regional specificity of the vote in most instances. Piergiorgio Corbetta's study of three north-central cities is the most restricted in scope, but it has the advantage of being able to focus on changes at the level of the "ward" (*senzione elettorale*) over time. Corbetta also is most explicit in arguing that changes initiated in the mid-seventies continue apace, demonstrating that what appears as stability is often revealed on closer scrutiny as extensive self-canceling movement between the parties. Maurizio Rossi has the unenviable task of trying to summarize the fortunes of the entire center-right of the political spectrum, including of course the dominant Christian Democratic party. The result, imposed by the subject matter, is diffuse, while confirming the multi-directional movement of the 1979 vote.

As a study which claims to be no more than a contribution to an ongoing analysis and debate, this book is quite successful. It is necessarily tentative in view of the evidence thus far at hand, but it raises the important questions and points to the problems that will have to receive more attention in the future.

STEPHEN HELLMAN

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The Apartheid Regime: Political Power and Racial Domination. Edited by Robert M. Price and Carl G. Rosberg. (Berkeley: Institute of International Studies, University of California, 1980. Pp. xii + 376. $8.95, paper.)

Publication of this collection of papers, originally prepared for a conference held in Berkeley in early 1978, provides students of political and social change in South Africa with a solid academic description of recent trends and a few theoretically based predictions about the future. The book, amazingly inexpensive by current standards, is well written for classroom use, and most of the authors have updated their chapters to cover events of 1979. Thus the book makes a substantial contribution by summarizing and updating knowledge and making it available to students. It is unfortunate, however, that greater editorial control was not exercised to eliminate excessive duplication in the material presented in various chapters.

Only a few chapters go beyond summary description to present new data or original analysis. In a chapter entitled "Change in South Africa: Opportunities and Constraints," Lawrence Schlemmer discusses the implications of recent survey research conducted among both whites (a broad sample) and blacks (more specifically, Soweto Africans with at least Standard 8 education) for the possibility of change. He finds that the attitudes of whites, and especially of Afrikaners, have changed on some issues while remaining firm on others. "Soft" issues on which change has taken place include occupational advancement, improvement in the socioeconomic conditions of blacks, integration in sport and other "impersonal spheres of social interactions," and the creation of "overarching linkages between separate political institutions." "Hard" issues on which attitudes have remained constant include preservation of white economic benefits, separation in intimate spheres of interaction, and political integration within common organizations resulting in the elimination of ethnic corporate autonomy.

Schlemmer concludes that this pattern of attitudes leaves "great scope for the type of innovation which will counter the well-nigh total political alienation of urban Blacks. . . ," and that "the time is clearly auspicious for bolder moves than hitherto in the direction of a constitutional arrangement which will embrace all South Africans and will allow for steady evolutionary changes toward a stable, just, and open society with constitutional protection of group interests" (p. 274). He cites in support of these conclusions the finding that 57 percent of the Soweto African sample, although conscious of great deprivation, highly politicized, and almost unanimously supportive of radical political organizations, were "willing to accept and happy to live with" a government having equal numbers of blacks and whites in the cabinet, while only 35 percent said the same thing about a government in which a black majority would rule the whites (p. 277). Furthermore, most African respondents were worried that black rule would create inter-ethnic or economic problems or white alienation and emigration.

While Schlemmer readily acknowledges that inclusion of non-racial rule as an alternative to a balanced cabinet or black dominance would have produced a substantially different response pattern, he does not discuss the relationship of attitudes to possible changes in the power situation in South Africa. While a racially balanced cabinet may be an acceptable alternative to many Soweto Africans today, it is not acceptable to the parties they would elect to the African half of such a cabinet if they were given the vote. And if these parties boycotted the elections leading to a racially balanced cabinet, or were prohibited from participating in them, this constitutional arrangement would surely be deprived of much of its support.

Robert M. Price's concluding chapter, "Apartheid and White Supremacy: The Meaning of Government-Led Reform in the South African Context," presents a more pessimistic view. Distinguishing between those aspects of apartheid which have consummatory significance as ultimate values and those which have instrumental or utilitarian significance as tools for the maintenance of white supremacy, Price suggests that events of the last 30 years, both within South Africa and abroad, have made abandonment of the former aspects both possible and neceessary for the preservation of the latter aspects. Consummatory apartheid has not succeeded in preserving internal security or in gaining international legitimacy. The Botha government has responded to these failures with a pattern of reforms designed to "eliminate those aspects of apartheid which have drawn the most international attention and criticism, while at the same time tightening state control in those areas where apartheid has proved less than fully effective" (p. 316).

Price demonstrates that recent changes in personal apartheid, African trade union organization, influx control and pass laws, rights and living conditions of those Africans who are entitled to remain in urban areas permanently, inclusion of such Africans the emerging pluralistic constitutional formula, and control over mass media all fit into the pattern specified above. He concludes that this pattern of reform will probably not succeed in attaining internal stability and international legitimacy because it does not go far enough to prevent internationally visible, militant black opposition. Such opposition can be eliminated, he believes, only by policies which would substantially diminish white privileges. While the future is always uncertain, this conclusion is probably more realistic than Schlemmer's hope that a stable, just society can be achieved through an acceleration of present trends in political change.

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Resource Mobilization in Poor Countries: Implementing Tax Policies. By Alex Radian. (New Brunswick: Transaction Books, 1980. Pp. xxiv + 266. $19.95.)

The first impression of this book is that Dr. Radian is the Dr. Reuben of LDC public finance, telling us all we might ever want to know about the problems of tax assessment and enforcement in developing countries, but further reading and reflection suggests more eminent literary comparisons. As one considers the journey Radian takes us on through Jamaica, Trinidad, the Philippines and Thailand, with references to experience elsewhere in Latin America, Asia and occasionally Africa, the analogy with Jonathan Swift seems more apt. We are shown how people in these other countries, like the Lilliputians, are outwardly doing things quite similar to what we do, but with quite different results, creating a topsy-turvy world out of familiar objects. They can't seem to do anything right. Efforts to eliminate a backlog of cases create more of the same (pp. 102, 116); collection campaigns don't work (pp. 156-57); the courts let off tax evaders with light penalties even when cases are "air-tight" (p. 189), and so on.

Radian's own description of his first encounter with the tax departments of Jamaica and Trinidad in the summer of 1974 suggests that he started as a latter-day Gulliver. (These departments' looked "remarkably like the U.S. Internal Revenue Service. Everything the book says should be there was in its proper place. . . ; [however,] things began to lose much of their glitter when officials were asked to describe how the new system actually works.") Still, more careful reading reveals that his observations are more like those of Yossarian in *Catch-22.*

Time and again the personnel in tax departments are caught in situations where to follow the "book" produces the wrong results. In his explanations, Radian is more like Joseph Heller, chronicling the irrationalities of bureaucratic performance. Responding to the government's call for producing more revenue leads diligent collectors into conflict with government when they press its supporters to pay up arrears. Tightening the assessment noose seems to produce still new opportunities for evasion. Actually, most of the behavior described fits the standards of "rationality," given people's range of alternatives and career incentives. Unfortunately, what pays off for the individual civil servant does not for the government as a whole.

The book usefully elucidates many of the decision rules which account for this behavior, and also suggests decision rules that can produce some positive results (e.g., pp. 124-34). In its methodology and exposition, it most resembles the book by Caiden and Wildavsky, *Planning and Budgeting in Poor Countries* (Transaction, 1980). If you liked that book, you will probably find this one to your liking, if less lively. The rule of "redundancy" figures prominently in the analysis, as might be expected, since Radian and Wildavsky have previously been collaborators (*Budgeting: A Comparative Theory of the Budgetary Process* [Little, Brown, 1975]).

Political scientists as a group are likely to be less pleased with the book than public administrationists because it gives politics shorter shrift than warranted. One of the main themes is that poor tax collection is more a matter of lack of skill than lack of will (e.g., p. 97). While the first lack is easily documented, it is not necessarily a "first cause." While Radian identifies places where political influence and power impinge on the tax-collection process, he shows little curiosity about in whose interest it is to have tax offices understaffed, undertrained, underpaid, not backed up in the courts, etc. While tax auditors told to go after the bigger suspects in the Philippines may indeed find the returns of smaller potential evaders "simpler" to deal with (p. 118), this is not the only reason they shy away from the bigger ones. Only passing attention is given to the political environment which shapes the administration of resource mobilization.

To his credit, Radian does not dwell on "political will," the favored *deus ex machina* of many political scientists, as a solution. Rather, he advocates various practical, incremental approaches to building up compliance (pp. 245-57).

The shape of the larger political system and its bearing on resource mobilization is hardly examined. The most common pattern of government interaction with its various publics in LDCs can be characterized as the politics of largesse. Political leaders, military as well as civilian, autocratic as well as democratic, have been disposed to present themselves to their publics as "giving" without "taking" (though they do "take" for themselves). As long as this is the strategy for justifying continued incumbency, introducing more stringent enforcement of tax laws makes no sense.

Governments do need, of course, ever-increasing resources to be able to play the role of Lady Bountiful, and here they are cross-pressured to lend an ear to suggestions for improving tax collection, if it can be done without much political cost. Radian's suggestions are pitched to such an ear. The logic of LDC political environments helps explain why such a large proportion of revenue comes from *indirect* rather than direct taxation, and it is a shortcoming in Radian's analysis that he devotes most of his attention to defects in the latter.

Perhaps more important is the lack of attention to the expenditure side of public finance in LDCs, the extent to which budgets are prepared with hardly any broad public input. Very special interest groups have substantial influence, and a narrow political elite makes all basic allocative decisions, even when there is a functioning legislative structure. The tax collection procedures and results of the U.S. and U.K. are held up as models of effectiveness (probably too much so, in light of increasing evidence of systematic evasion). But what is not considered is the extent to which American and British improvements in tax collection were made concurrently with democratization of their polities in the latter nineteenth and early twentieth centuries. As the public generally gained more control over patterns of expenditure, so did its willingness to pay taxes. Enforcement mechanisms were strengthened with public acquiescence, as the objects of expenditure more and more met their needs and expectations. Thus, it is more than a matter of political "culture," which Radian does admit into his model of comparative explanation (p. 194).

In the final analysis, resource mobilization in poor countries cannot be treated as a means for their development divorced from the ends to which these resources, once mobilized, would be put. Who benefits (most), and who decides on this, are questions to be explored more extensively. The supply-side and demand-side of public finance cannot be so readily separated. In his treatment, Radian has illuminated many of the factors operating within the structures of administration, and has come up with some well-thought out suggestions for working essentially within those structures. It is left to someone else to show how resource mobilization is a matter requiring much more than implementing tax policies. It comes squarely into the realm of politics—political interests, political leadership, and political mobilization.

NORMAN UPHOFF

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Leaders of Revolution. By Mostafa Rejai, with Kay Phillips. (Beverly Hills, Calif.: Sage Publications, 1979. Pp. 245. $15.00; cloth; $7.95, paper.)

It is unfortunate that theory construction and testing have assumed such priority in political science that some scholars who make other and useful contributions to political knowledge feel compelled to present them under the guise of theory. This work, ostensibly a quantitatively derived or tested (the intention is not clear) theory of the emergence of revolutionary leadership, is really a comprehensive mapping operation of the attributes of a large sample of revolutionary leaders, perhaps the most extensive one since Lasswell's and Lerner's *World Revolutionary Elites.*

In the initial chapters, Rejai summarizes in turn the psychological approach of Lasswell and Wolfenstein, the psychohistorical approach of Erikson and Mazlish, and the sociological approach, equated here with charismatic leadership. Finding each limited in some respects, he then proposes a synthetic theory combining the strong points of all. Revolutionary leadership is asserted to emerge from the interplay or coalescence of three elements: situation, leader motivation and leader skills. The situational element, characterized as crisis, is given primacy; the relevant leader skills are stipulated as verbal and organizational.

That would appear to conclude Rejai's obligations to theory. There is no specification of relationships among or between these elements. That of crisis makes no further appearance. Nor do the components of leadership skills. The rest of this study is devoted to analyses of other data on 64 revolutionary leaders coded for 60 variables hypothesized as relevant.

The 12 revolutions from which the sample is drawn range from the English revolution of Cromwell in the 1640s to the French one led by Cohn-Bendit in 1968. The variables cover: social background; family life; experience, including education, occupation, prison and travel; ideology and attitudes; psychology. Included are such suggestive ones as the number of siblings, and personalities admired by the leaders. For completeness of coverage, one might wish only more reference to sources of the hypotheses and some indication of those with some prior confirmation in the literature.

Frequency distributions of the characteristics yield some interesting results. Well over half the sample of leaders were from mainstream ethnic and religious groups in their societies and had tranquil family lives. Moreover, they were prolific writers, mainly of revolutionary works. Rejai and Philips also provide additional confirmation of findings elsewhere that revolutionary leaders have tended to be intellectuals, relatively young, of middle-class origin. Through factor analysis of the data, five configurations or types of leaders are discerned: founders, agitators, generals, scholars and professional revolutionaries. Half the sample were surveyed for the presence/absence of six clusters of psychological variables held to account for motivations of revolutionary leaders. Findings that over two-thirds were characterized by vanity/egotism/narcissism and by asceticism/puritanism while oedipal conflict was infrequent are fleshed out by illustrative material.

I have reservations about the chapters in which are compared the "classic" revolutionary leaders of the seventeenth and eighteenth centuries and the contemporary twentieth-century ones. Findings to the effect that the former were less travelled than the latter and fewer of the former adopted foreign ideologies tell us more about the independent variable of time period than about revolutionary leaders. I find a similar problem with correlations using level of development of the country as an independent variable. Admittedly, some findings are more interesting than those which tell us that more leaders from undeveloped than developed countries come from rural areas or that all Muslim and Buddhist leaders are found in undeveloped ones. Still, these chapters illustrate the limitations of quantification without a solid theoretical base. Rejai's projected comparison between his sample of revolutionary leaders and one of non-revolutionary leaders should produce more significant results.

Yet even though the current exploration does not add up to the why and how of revolutionary leadership, it does tell us a great deal about the who. Moreover, it provides a data base and interesting observations from which other efforts can be launched.

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Challenge to Governance: Studies in Overloaded Polities. Edited by Richard Rose. (Beverly Hills: Sage Publications, 1980. Pp. 238. Sage research series in European politics, Vol. I. $20.00, cloth; $9.95, paper.)

This book belongs among the growing number of studies that worry about the health of democratic governments of the First World. The list of "the ill" waxes and wanes, but usually includes Italy (for recent examples, see G. DiPalma, *Surviving Without Government* [California, 1977] and A. S. Zuckerman, *The Politics of Faction* [Yale, 1979]); and notably, since the late 1960s, Great Britain (for a relatively early example see Trevor Smith, *Anti-Politics: Consensus, Reform, and Protest in Britain* [Knight, 1972] and for a recent one, S. E. Finer, *The Changing British Party System, 1945-1979* [AEI, 1980]). No Western European government seems immune from such treatment—and if the U.S. is included in the First World, the bibliography of its ailments has become a growth industry by itself. In various ways these studies mark the "crisis" engendered by the gulf between an "ideal" (postulated variously: e.g., according to classic theorists such as Locke, Rousseau, J. S. Mill, Marx; or according to broad concepts such as "participatory democracy," "consensus," "non-alienation"; or bread-and-butter standards, such as "price stability," "full employment," "economic growth"; or public expectations as measured by civic culture types of survey research) and "the reality" (always dismal and described in many combinations of terms: political, economic, psychological, sociological, cultural, ecological, etc.). Conclusions drawn from these "crisis" studies have an equally broad range: From the "need for revolution," to the "need to 'lower expectations,' " (which, in particular applications, may be radical in itself), to the "need for reforms" (of constitutions, parliaments, parties, nominations, elections, policies, policy-making mechanisms, economic priorities, administrative procedures, etc.), to calls for "realism" (or conservatism?) in accepting the way of the world.

Challenge to Governance promises more than it delivers, but because of its topic, it is a worthwhile addition to the literature. The book evolved from a series of conferences and seminars held at various European universities in the late 1970s. Its purpose is "to analyze . . . pervasive challenges to the political authority of [Western] European governments today" (p. 2). All Western European polities are "overloaded," according to this book. Their governments are facing increasing citizens' demands but command inadequate resources (mainly economic, but also political) to satisfy the demands. Hence their publics increasingly perceive them as ineffective. As a result there is a "crisis" in government credibility and this state of affairs threatens the legitimacy (political authority) of every Western European polity.

The book consists of an introductory chapter by Rose on "The Nature of the Challenge" (of overloaded polities) and another by him on survey responses towards respondents' governments, plus chapters by Peter Self on administrative overload in British attempts at planning; by Jacques Leruez on France's indicative planning; by Don S. Schwerin on Norway's experience in comprehensive tripartite bargained incomes policy; by Rudolph Klein on the costs and benefits of administrative complexity to the British National Health Service; by A. F. P. Wassenberg and Jan Kooiman on the fact that while survey research indicates that "overload" is a perceived problem there is no correspondingly drastic decline in confidence over democratic regimes in Western Europe; by Paolo A. Farneti on the Italian response (or lack thereof) to overload; and by Dennis Kavanagh who argues that old-fashioned, heroic leadership (à la Lloyd George, Churchill, DeGaulle) could not solve current problems, nor will increased centralization of decision making.

Although most of the chapters address specific cases, the overall aim is to address general patterns of governments trying to cope with increased demands in the face of declining national economies, inflation, unemployment, increased governmental complexity, and a fall-off of national consensus. The questions raised are: (1) how have governments responded to overload in ways that try to preserve both their effectiveness and popular consent? (2) how do citizens react when they feel there is a decline in their government's effectiveness? and (3) what has happened to governmental authority or legitimacy?

None of the authors reports examples of, or proposes methods for increased governmental effectiveness. The case studies of formal planning in Britain, France, and Norway report inabilities to manage adverse economic conditions. (Yet governments persist in planning efforts because they are *expected* to have some general strategy.) Nor does any of the authors propose structural solutions or normative policies to improve effectiveness; and none advocates an Eastern European "socialist" method of maintaining effectiveness and compliance by adopting coercion.

What then of the future for consent? The authors do not forecast any real revolutionary threat to First-World polities. Where terrorists are active (Italy, France) the public has responded with heightened support for the existing polity (though not necessarily its political leaders). Among proposals to maintain or foster consent, the articles by Klein, Wassenberg and Kooiman, Kaase, and Kavanagh endorse structural reforms towards more widespread participation in decision making to cope with a fall-off of party support and the diffusion of power in modern societies. But admittedly, participatory schemes trade off efficiency in favor of consent, for they increase the cost and complexity of decision making —and may even heighten overload from those who champion contradictory goals.

Further, with regard to consent, Rose reminds us that citizens' choices represent more than a stark polarity between compliance (full consent) and revolution. People can and do exercise the option of "indifference." Tax evasion, famous in Italy, is a mark of civic indifference, a distrust and doubt that government can or will make things better. Refusal by unions and employers to be bound by wage-price guidelines is another manifestation of "indifference"; and Rose points to several other examples (pp. 22-26).

But all this further erodes government authority, for as individuals and groups ignore governmental policies declared to be for the good of the whole, governmental efficiency declines because Western polities have always relied heavily on voluntary cooperation by the public.

My greatest caveats about this work come from the feeling that there are too many generalizations from too few cases, and, more basically, that the notion of "overload" is pretty overloaded itself. Is it *cause* or *effect* of governmental *action* or *inaction*? Perhaps Rose could have added a concluding chapter clarifying the term.

THOMAS MCENROE

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Peasants of Costa Rica and the Development of Agrarian Capitalism. By Mitchell A. Seligson. (Madison: University of Wisconsin Press, 1980. Pp. xxxii + 220. $21.50.)

Mitchell Seligson's book is about the impact of agrarian capitalism on Costa Rican peasants during the nineteenth and twentieth centuries. Using a variety of techniques including survey research, Seligson traces the evolution of peasant living conditions and attitudes as they were influenced by changes in the nature of the rural economy. He argues that the development of coffee into an export commodity during the nineteenth century gradually transformed the self-sufficient class of yeoman farmers inhabiting the central highlands into a fragmented collection of landed peasants (with and without title), landless laborers, and squatters. Growth of the rural population, lack of adequate land in the central highlands, and the limited number of urban jobs gradually marginalized the yeoman class, forcing many to migrate to less-productive lowland regions.

The major question Seligson attempts to answer is why Costa Rican peasants seldom rebelled, given such downward mobility. He argues that changes in the agrarian sector came slowly, allowing for adaptation. Some peasants were able to find work on the expanding lowland banana plantations. Using survey data, Seligson argues that the primary concern of agrarian workers is not maintaining ties to the land but rather personal security. This being the case, wage employment on the banana plantations was viewed as a relatively attractive alternative. He also asserts that the ethnic homogeneity of the population prevented the kind of cruel and systematic exploitation of the peasant class that occurred in countries such as Guatemala.

While the central hypothesis concerning the relationship between security and rebellion is well argued, several points bear further investigation. First, it is not clear that a self-sufficient yeomanry existed in Costa Rica over a long enough period for a new class mentality to have formed. Seligson notes that the central highlands were settled during colonial times by Spaniards from poor areas but with strong aristocratic aspirations. These poverty-stricken settlers had to wait awhile for the development of an economic base from which to finance these aspirations, but the opportunity finally came with the nineteenth-century coffee boom. Although Seligson entitles his first chapter "The Rise of the Yeoman," it might more appropriately have been called "Limbo for the Aspiring Aristocracy." The Costa Rican claim of cultural and political uniqueness may contribute to a tendency to romanticize rural class attitudes and relationships as they existed during the seventeenth and eighteenth centuries.

Secondly, Seligson does not clearly specify the nature of the elite that emerged during the nineteenth and twentieth centuries. Was this elite a product of the temporarily marginalized groups mentioned above? Were large numbers of nineteenth-century immigrants involved? What role did intermediaries in the coffee export trade play? While the coffee oligarchy is at one point described as having been coterminous with the government elite, this oligarchy's growing impotence with regard to tax policy is also noted. Although lack of more detailed attention to the nature of the Costa Rican elite is understandable in a book on peasants, further knowledge concerning the nature of this elite might have contributed to an understanding of its relationship to rural groups.

Finally, several observations can be made concerning the attitudinal data presented. While the case for peasant concern with security is convincing, Seligson further argues that peasant organizing activities are a response to insecure conditions. If the search for security plays a primary role in such activity, it is difficult to explain the high levels of organization on the "secure" banana plantations, and low levels elsewhere. Seligson also argues from survey data that the agrarian reform programs sponsored by the government agency ITCO have increased peasant feelings of efficacy. However, he further observes that ITCO adopted policies in the 1970s which stressed self-selection of peasants for reform programs. One would expect such self-selecting individuals to express feelings of efficacy when surveyed.

Seligson generally succeeds in presenting a comprehensive view of the evolution of Costa Rican peasant society. His study represents one of the few attempts to combine historical analysis of the impact of agrarian capitalism on peasants with detailed survey research. This book will be read not only by those interested in agrarian problems but by individuals wishing to understand contemporary Central America. It contains insights that should be particularly useful during the tumultuous decade of the 1980s.

STEVE C. ROPP

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Politics and Purges in China: Rectification and the Decline of Party Norms, 1950-1965. By Frederick C. Teiwes. (New York: M. E. Sharpe, 1979. Pp. xiii + 729. $35.00.)

In the absence of a set of formalized rules, procedures, and institutions, management of elite conflict in communist societies has proved to be a difficult problem. In this volume, Frederick Teiwes examines how the Chinese regime has attempted to use the rectification approach that was developed during the Chinese revolution to manage elite conflict during the early post-revolutionary period. In his examination, Teiwes develops detailed balance sheets of the political costs and benefits of the major political campaigns during this period.

Teiwes' major thesis is that a decline in policy consensus among the Chinese elite led to widespread violation of the norms of rectification, thereby setting the stage for the Cultural Revolution. Rectification as a conflict resolution process assumed openness in policy making, obedience in the implementation of policy decisions, and a reasonable degree of leniency in treating violations of elite discipline. As disputes over the direction of policy developed, dissent from the chosen policy direction was treated harshly, with substantial damage to the integrity of the decision-making process and the morale of the cadre as a whole. Teiwes draws particular attention to the contradictory role of Mao Tse-tung as both a strong supporter of the norms of rectification and as a sometime violator of those norms, particularly during the late 1950s and early 1960s.

The book begins with an overview of the doctrine, methods, and historical development of the rectification process. It then proceeds to analyze the major campaigns of the period, starting with the consolidation campaigns of the early 1950s (e.g., the "Three Anti" and "Five Anti" campaigns) and ending with Socialist Education Movement of 1962-1965. A concluding chapter draws together the arguments advanced in the individual chapters and makes some suggestions regarding the implications of the analysis for current Chinese politics.

The strength of Teiwes' study is his painstaking analyses of the major political campaigns of this period. Drawing heavily upon previous research, Chinese publications of the time, and Chinese material revealed during the Cultural Revolution, he meticulously traces the timing, direction, and effects of the campaigns as well as the particular roles of individual Chinese leaders. Despite the often substantial problems of missing data, he manages to develop carefully reasoned judgments on the issues involved. It is worth noting that some of these judgments are contrary to those previously offered by other analyses of these events (e.g., Richard Baum's analysis of the Socialist Education Movement in *Prelude to Revolution* [Columbia, 1975]).

Beyond his balanced analyses of specific political campaigns, Teiwes' study could have benefited from a more systematic attempt to consider China's rectification process in a comparative perspective. The only comparative reference Teiwes makes is to the Soviet Union of Stalin's time. Clearly, this is not the only possible point of comparison in the study of Communist systems. One suspects that using Stalin's regime as the only benchmark biases evaluation of Chinese rectification practices in a favorable direction. The lack of comparative perspective is also reflected in an absence of reference to relevant studies in psychology, sociology, and political science on norms and organizational behavior. Given the stress on rectification as a set of organizational norms for the Chinese Communist party, this is somewhat surprising. The general approach is to treat Chinese politics as *sui generis*. This deficiency is particularly apparent in the discussion of the rectification process in chapter 2. Current literature on the processes of attitude change could have added much to Teiwes' discussion of the likely effectiveness of the rectification process.

The absence of a comparative perspective poses particular problems in trying to assess the efficacy of the rectification approach as a long-term solution to problems of elite conflict in the People's Republic of China. While cognizant of the ambiguity and fragility of rectification norms, Teiwes suggests that in the future these norms can again provide a framework for managing elite conflict, albeit with some irreducible element of unpredictability (p. 633). This suggestion is particularly surprising, given the ample evidence offered in *Politics and Purges in China* that Chinese cadre at all levels have been frequently at a loss in trying to divine what policy means and have often been caught pursuing previous policies after new ones have been established, with disastrous personal consequences. As long as there exist few meaningful "exit" options for dissident or mistaken cadres and there is no institutionalized procedure for insuring Chinese leadership succession, rectification does not seem to provide a particularly viable long-term solution.

The absence of a comparative perspective also makes it difficult to assess the significance of Teiwes' major finding that lack of policy consensus led to a decline of rectification norms. That extreme disagreement about goals should lead to violations of the "rules of the game" on how to handle conflict over those goals seems unsurprising. It would be useful to know whether this pattern is indeed common across different political circumstances, it indicates a relatively low level of institutionalization of the Chinese Communist party, or, as the author suggests, it is a reflection of the unique role of Mao Tse-tung.

In sum, *Politics and Purges in China* is a book that should be read by students of Chinese politics for its analysis of the political campaigns of the 1950s and 1960s. For comparative scholars interested in understanding the Chinese experience in a larger context, it has less to offer.

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Ethnic Politics in Malaysia. By R. K. Vasil. (Atlantic Highlands, N.J.: Humanities Press, 1980. Pp. xii + 234. $12.00.)

This work had its origins in the two well-received earlier publications by the same author: *Politics in a Plural Society* (Oxford, 1972) and *The Malaysian General Election of 1969* (Oxford, 1971). Portions of chapter 3 and most of chapter 4 of *Ethnic Politics* replicate or summarize material from Vasil's first book and pp. 145-78, chapter 5, of *Ethnic Politics* replicate or summarize material from his second book.

Ethnic Politics analyzes developments to 1973, though there is a footnote reference (p. 205) to the 1978 elections. There is an incisive overview of the *Barisan Nasional* (National Front) which emerged in 1972 and controls 131 of 154 seats in the House of Representatives. Vasil's evaluation of the Front, though overly pessimistic in its assumption of permanent and heavy-handed Malay domination, is quite useful. However, there is little substantive information provided on the day-to-day workings or policies of the Front.

Vasil argues the 1957 independence constitution was to lead toward political equality for all races, the special position of the Malays—Article 153—was an interim measure, and that the Malays were not to remain an economically depressed plurality in Malaya, now Malaysia. An underlying theme is that the Alliance comprised of UMNO (United Malays National Organization), the MCA (Malaysian Chinese Association) and the MIC (Malaysian Indian Congress) which evolved between 1951 and 1955 was "a freak of history" based on expediency (pp. 62-63). UMNO accommodation gradually became domination because UMNO Malay leaders always were committed to the principle that Malaysia could not be governed by a mixture of races. On all crucial issues, the Malay position must dominate. A paradox alluded to but never explained, if indeed it can be, is this: from 1957 to the communal riots of May 1969, non-Malay communal parties kept pushing for more political parity. But, if the more moderate UMNO leaders were to compromise, where was the *quid pro quo,* i.e., significant improvement in the Malay economic position? A diminution of Malay political domination had to mean yielding up economic benefits by the Chinese, but this did not occur.

In selected areas, I disagree with Vasil. He is too critical of moderate UMNO leaders. When Tun Razak became prime minister and Mahathir deputy prime minister, their moderation became apparent. Not all MCA leaders were prosperous businessmen. T. H. Tan, for example, became a *towkay* after he achieved leadership status. The "starting-point of the communal holocaust of 13 May 1969" was not so much the victory celebration of the Selangor state UMNO in Kuala Lumpur (p. 182), but the provocative parades by non-Malay partisans who thought they had achieved victory, as well as the 1964 communal riots in Singapore and West Malaysia. The suspension of parliamentary government and the establishment of a National Operations Council on May 14, 1969 resulted not only from pressure by UMNO extremists (p. 184), but also on the advice of most cabinet ministers—Malay and Chinese. Finally, one should not assume the era of accommodation is past and non-Malay parties in the National Front are subservient appendages. The non-Malay parties, no longer limited to the MCA and MIC, represent non-Malay influence and there is room for compromise. For example, Chinese control approximately 29 percent of Malaysia's economic assets today, up from 22-23 percent in 1970. The commitment that Malays will control 30 percent by 1990 will be achieved by an expanding economy and redistribution of foreign-controlled assets rather than at the expense of Malaysian Chinese and Indians.

A fundamental problem insufficiently discussed is the fact that no strong, widely supported Chinese leadership has emerged in the postwar period. Many Chinese businessmen today lack confidence in the MCA; witness the fact all MCA candidates who recently contested the Selangor Chamber of Commerce election were defeated.

Vasil's book provides a good foundation for analyzing contemporary Malaysia. It also will be useful for those interested in Third-World political parties and/or the effort to maintain a modicum of democracy in a communal society.

THOMAS J. BELLOWS

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The Centralist Tradition of Latin America. By Claudio Véliz. (Princeton, N.J.: Princeton University Press, 1980. Pp. xii + 355. $22.50, cloth; $9.75, paper.)

Claudio Véliz is familiar to Latin Americanists as the editor of two collections of essays on Latin American politics and of the useful *Latin America and the Caribbean: A Handbook* (Praeger, 1968). Now he has undertaken the large task of reexamining the history of Latin America since the Conquest with a view to isolating and analyzing its "centralist tradition." The resulting book is somewhat curious. Its two concluding non-historical chapters, on "Latin American Industrialization" and "Authoritarian Recentralization," are the most successful. In the eleven that precede them he identifies historical *antecedents* for the present-day socio-political phenomena that interest him, but I do not believe that he establishes chains of *causation.*

His working definition is somewhat meagre: "the centralist disposition of Latin Americans ought not to be confused with an ideology. Rather, it can be taken to be a . . . description of the ways in which Latin Americans have gone about attending to their economic, social, and political arrangements. . . . Until now [it] has been latitudinarian, legalistic, and emphatically civilian" (p. 302). So lightly armed, he nevertheless ventures into the historical literature in search of evidence; and not surprisingly he finds it. Though he resorts to the occasional eccentric source or interpretation—the White Legend according to Hanke and others simply will not support the weight he places upon it—most of his readings fall within the conventional wisdom. He places the beginnings of Latin American centralism in the Conquest: in the extraordinary efforts through which the Castilian crown eliminated all rivals, real and imagined, ecclesiastical, civil, and military, to its supremacy in the Indies and imposed there the most advanced colonial bureaucracy of the age. Véliz stresses the continuities that lasted through four and a half centuries—through Hapsburgs and Bourbons, bourgeois republicans and backwoods caudillos, urban populist demagogues and the most recent efflorescence of military saviours. Aware that Brazil's beginnings had little in common with those of Spanish America, he accounts for present-day similarities in political culture by hypothesizing a confluence of traditions in the eighteenth century at the time of the Pombaline reforms on behalf of the Portuguese Crown and the Caroline reforms on behalf of Madrid. The durability of the centralist tradition he attributes chiefly to elements *not* within the Latin American experience: feudalism and its breakup; religious non-conformity; the industrial revolution of the eighteenth and nineteenth centuries; the ideological, social, and political concomitants of the French Revolution.

In general terms, these arguments are familiar enough; in Véliz' hands, however, they lack important nuances. The most serious problems arise from his neglect of the environmental-economic and demographic matrix. He thereby fails to see *dis*continuities created by wildly gyrating boom-and-bust cycles, by protracted contraction of imperial trade and communications in the seventeenth century and the consequent emergence of local and regional networks, by periodic retreats into ruralism and self-sufficiency, by stagnation of social and economic life in the first half of the nineteenth century and its reorientation in the second half by virtue of imperialist penetration, above all by the secular contraction and subsequent explosion of population. Historically there has been ample scope for the exercise of initiative and power at the hacienda, municipio, regional, or interest-group level. Although the ideological and juridical culture has been hostile to legitimization of a sort of *political* pluralism, it does contain strong safeguards for entrenched *social* and *economic* privilege or simply for workable ways of life. Latin America is encrusted with such survivals: on them are reared the structures of corporatism—more properly, *natural* corporatism. It is with these structures that any centralizing regime must interact; the *interaction* gives the distinctive Latin American style. Remarkably, Véliz pays only perfunctory attention to Iberian parallels. A more systematic treatment might have proved illuminating, not least a treatment of the nineteenth-century Spanish experience of economic contraction, centralist/regionalist and modernist/traditionalist struggles, predatory militarism, and penetration by European capital. It is not coincidental that Juan Linz' observations on Spanish authoritarianism offer one of the most useful conceptual guides to Latin American politics.

Véliz is more convincing on modern Latin American industrialization and its consequences. "Industrialization did not come to Latin America as the result of an indigenous process of growth," he writes, "but as a complex response to an external stimulus" (p. 252). He describes in broad strokes the growth of import-substitution from 1929 through the Second World War and discusses the reasons why the state rather than small private entrepreneurs assumed the more dynamic role. He comments perceptively on industrialization's almost total lack of impact on either the popular or artistic imagination of Latin Americans. His observations on the reasons for the survival and prosperity of Latin America's landed oligarchs in changing times are also worth pondering. But Véliz' concluding political analysis is despairing. Magisterially he dismisses nineteenth-century liberalism and twentieth-century Marxism as "exotic" doctrines unsuited to Latin America's unique circumstances. The populism of the 1930-1970 era was unsuccessful for its disastrous insistence on distributing slender and unreliable state revenues rather than aggregating and investing them. He banishes military developmentalism—principally, it would appear, because he detests militarists. Unlike other writers who seek the "unique tradition," Véliz has no sympathy for quasi-fascist or "corporatist" solutions. He is left with bureaucratic centralism, which he describes as "a pragmatic tradition devoid of ideology . . . as much the consequence of accidental external circumstances as of deliberate choice" (p. 301). Like some islet in the Outer Hebrides, it is terrain as narrow as it is bleak.

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Models of Political Economy. Edited by Paul Whiteley. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. 388. $20.00, cloth; $9.95, paper.)

The term "political economy" has lately become fashionable, and a variety of scholars seem determined to describe their work using this label. Since a book entitled *Models of Political Economy* could focus on a variety of topics, it might be well to describe the contents of this volume. The major subsections include such topics as "Modelling Politico-Economic Interrelationships," "The Political Business Cycle," "Politics, Inflation and Economic Policy," "Formal Approaches to the Analysis of Political Systems," and "Strategic Behaviour at the National and International Levels." The foreword to the book, by Richard E. Wagner, portrays the contents more simply by noting that most of the papers examine the relationship between "democracy and prosperity" (p. 5), which would probably have been a more suitable title for the book.

In general, the conception of "political economy" that underlies most of the papers in this book coincides with what has variously been called "public choice theory," "positive political theory," or "formal political analysis." There is nothing wrong with this, but it should be noted for the record that a number of scholars who think of themselves as "political economists" are excluded by this definition. See, for example, various articles in *International Organization* during the last decade.

Two strong points of this collection of essays deserve mention. First, the models of politico-economic relations are carefully drawn and potentially useful to other scholars. Second, empirical data on prosperity and democracy is presented for a number of countries, including Norway, Denmark, Sweden, West Germany, Britain, France, and Japan. Those studying the relationship between political support and economic variables will find much that is useful in this volume.

The weak points of the book concern scope, organization, and timing. The misleading title has already been mentioned. The organizational difficulties probably stem from the book's origin as a series of papers presented at a professional meeting. On the whole, these papers form about as much of a coherent whole as most books with similar origins. The addition of an index would have made it easier to find treatments of the same topic by various authors. The papers were presented at a conference held in the spring of 1978. It is unfortunate that Edward Tufte's *Political Control of the Economy* (Princeton, 1978) was published too late for consideration by most of the authors. There is something disappointing about reading a book published in 1980 on papers written two years earlier when a major relevant book has appeared in the interim.

Overall, *Models of Political Economy* is a useful addition to the rapidly growing literature on public choice theory. The discussion of democracy and prosperity in countries other than the United States should be helpful in broadening the outlook of American students of this subject.

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International Politics

Reform and Resistance in the International Order. By Ian Clark. (New York: Cambridge University Press, 1980. Pp. v + 202. $29.50, cloth; $8.95, paper.)

The "reform" in the title of this book is of rather modest ambition. It stands for "attempts to have the Great Powers subscribe to limited 'group norms' in terms of which the powers might conduct their management of the international system"; attempts that have been exemplified, for Ian Clark, in the nineteenth century, by the Concert of Europe and, in the twentieth, by the League of Nations. "Resistance," on the other hand, stands for balance-of-power politics. Such a minimal conception of the practical improvement of which the international system is capable is illustrated in the bulk of this short book by four chapters that deal with the international history between 1815 and 1980 as the unfolding and then retreat of these reformatory attempts: (1) 1815-1854, basically the era of the Concert; (2) 1856-1914, a period of power politics; (3) 1919-1939, "Concert without balance" and (4) 1945-1980, return to balance-of-power practices pure and simple, exemplified by deterrence and now tri-polarity.

For Ian Clark, the "reform" in the international order has been neither evolutionary nor cumulative; such progress as might have been achieved in period 1 was given up in period 2; and the brief rally in period 3 did not suffice to make of period 4 anything more promising than the return to balance-of-power conditions characteristic of Europe late in the eighteenth century. He sees the to-and-fro between "reform" and "resistance" producing something like a cyclical pattern of international politics wherein the five-power world of the 1970s, at least as envisaged in the Nixon-Kissinger conception of a few years ago, begins to resemble the five-great-power balance of Vienna (1815). Thus, he claims, we have come full circle.

This account of international history as fluctuating between "concert" and "balance" is seen to illustrate the debate, so familiar to students of international relations, between "idealism" and "realism," one that Clark holds to be roughly equivalent to a difference between optimism and pessimism in international affairs and even to Herbert Butterfield's "whig" and "tory" interpretations of history (1931). Kant's views are offered as an example of the "tradition of optimism" (most recently exemplified, Clark argues, by world-order model projects), and Rousseau's views, as exemplars of the "tradition of despair." The discussion of these debates and traditions is even-handed and brings out the strong points of both positions; it runs smoothly and at a brisk pace, even though the reader is taken through much familiar terrain. However, the historical accounts could do with clearer definitions of both "concert" and "balance" and firmer rules about what it is that makes one period one of "concert" and another of "balance." (As formulated, e.g., by Vattel, 1758, common interests and common principles—hence the group norms of concert—were certainly an inherent part of the balance of power.)

The treatment of evidence tends toward selectivity: missing from the account of period 2 are the important Berlin meetings of 1878 and 1884-85 which would tend to weaken the argument; the review of period 4 is curiously telescoped from the perspective of the late sixties and early seventies and not readily acceptable to one who has lived through all of it. Nor do global wars form any part of this story. The long descriptive chapters have no room for either whigs or tories, whose debates were so crucial to the definition of Britain's world position and its conceptions of the balance of power ever since 1688. And finally, the account suffers because it invites drawing conclusions about world politics from what is, for the most part, a story of Europe alone.

Clark's attempt to combine discussions of international theory with a review of international history is certainly to be welcomed, even though little is said about the interaction of these two "dialectics," it being implied that theory affects history hardly at all. According to the book jacket, "the approach of the book reflects a traditionalist persuasion." That is fine, too, but one does wish that the "traditions" of students of IR theory, of whatever persuasion, went deeper than the Congress of Vienna, and nineteenth-century European diplomatic history. For how is it possible to make judgments about evolution and progress in world politics when one's experience begins in 1815? By the time he reaches the stage of conclusions, Clark admits that his time frame might be unduly constrictive and he wistfully refers to the broad vistas of Adda Bozeman's *Politics and Culture in International History.* Undaunted, he proceeds to declare that international politics has not changed in its operative principles in the past 165 years. As a bald statement, that must surely be incorrect. Global politics in the modern period shows both recurrent patterns (i.e., long cycles of world leadership) as well as sustained evolutionary tendencies, likely based on innovation and learning. But, in any event, Clark does not make it clear why such a large and intricate system as international relations should have undergone such dramatic change in such a relatively short period. Maybe the time has come when an adequate theory of world politics needs to be based upon an analysis of the essentials of the entire human experience with world systems.

The book concludes on a decidedly pessimistic ("doubly dismal") note: "If the present system is no more than a balance one, then it would appear that the search over the past one-and-a-half century for a more highly developed form of international order has, at least temporarily, come to a halt. . . . What is more provocative is that, in the meantime, other variants of international order have been attempted and found wanting. What the present age lacks is any widely-shared vision of the direction future reform should take. The question lingers, should the balance system prove defective for our future needs, where do we go from here?" Hence, whatever the shape of this needed vision of the future, it seems unlikely to emerge from an inquiry organized around the respective merits of optimism or pessimism in international politics.

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Problems of Contemporary Militarism. Edited by Asbjørn Eide and Marek Thee. (New York: St. Martin's Press, 1980. Pp. 415. $32.50.)

No one can deny that the rising menace of militarism should be a serious concern of us all. This concern is reflected in this collection of essays written by a diverse group of scholars. Most of the contributors are united in their conviction that the nature of military influence has now become much more pervasive in the post-colonial period. Consequently, they stress the need for a proper understanding of the international implications of "contemporary militarism."

Two major concepts which appear again and again for explanation, clarification, or definition are "militarism" and "militarization." Marek Thee defines "militarism" as a set of "such symptoms as rush to armaments, the growing role of our military (understood as the military establishment) in national and international affairs, the use of force as an instrument of supremacy and political power, and the increasing influence to civilian affairs"; and "militarization" as "an extension of military influence to civilian spheres, including economic and socio-political life" (p. 15). These definitions are echoed, with variations, in a number of essays. Michael T. Kaler's idea of militarism is "the notion of excess of the growing encroachment of the military over civilian institutions with a concomitant decline in individual freedoms and democratic forms of decision-making" (p. 36), while Jan Øberg's idea of militarism is a "merging of major civil and military interests" (p. 49). However, the principal theme remains the same, namely, that military values are increasingly dominating national culture, education, the media, religion, politics, and the economy at the expense of civilian institutions.

The "contemporary" character of this phenomenon, according to most of the contributors, derives from its global reach and especially its extension to the Third World. A number of contributors view it as essentially a post-World War II development and have tried to present analyses of the various socioeconomic, political, and cultural factors responsible for this development. Although these analyses overlap a good deal, the editors maintain a balance by including a few articles like those of Julian Lider and William Eckhardt who believe that militarism is the inevitable consequence of imperialism and that the abolition of the former depends upon the abolition of the latter.

Much as any peace-loving person is disturbed by the global expansion of weaponry, my problem with most of the essays is that they tend to suggest that the Third World is the only devil and that the developed world shares no blame for the rise of modern militarism. Also, it is not correct to say that contemporary militarism is "contemporary" with respect to the Third World alone. Indeed, it is unfortunate that, despite the inclusion of a couple of articles on the transfer of arms, this phenomenon has not been integrated into the various definitions of contemporary militarism. At least one or two papers should have dealt with how the highly militarized nations use transfer of arms as an instrument of colluding with the political elites of the Third World and thereby contribute not only to the suppression of the rights of the people by those elites, but also to "contemporary" militarism.

Nevertheless, the editors of this volume serve a timely warning to the academic community that unless the alarming danger of new militarism is studied and understood, the situation may well slip out of human control. But the problem of research is only one of the two fundamental problems of contemporary militarism. The other problem, posed in the essays by contributors like Richard A. Falk, Dieter Senghaas and Asbjørn Eide (although their basic orientations differ), is how to control the phenomenon of new militarism in a world in which the dichotomy between national sovereignty and individual freedom still exists, in which the world's common people are still denied the bare necessities of life, and in which, as Ejub Kučuk observes in his article, military organization still seems to be the only way to transcend militaristic values.

MAHENDRA AGRAWAL

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Global Food Interdependence: Challenge to American Foreign Policy. By Raymond F. Hopkins and Donald J. Puchala. (New York: Columbia University Press, 1980. Pp. xvi + 214. $20.00, cloth; $7.50, paper.)

This lucid book's title states its insistent thesis: that nations' fates are linked on the food problem. Troubles will proliferate from the indifference of rich nations, from the autarky of poor nations, from neglect and abuse of multinational institutions. The authors review, one by one, the commercial system and the major national and international programs, concluding that all should be used positively and complementarily. It is the maximizing relationship, modified to achieve political feasibility, which the authors continually seek to specify. The United States, as an initiator of international agendas and as a major exporter of grain and technology, has a large but by no means predominating role. The book apparently is an outgrowth of an analysis prepared for the U.S. Department of State.

Mainly from synthesizing other analyses, Raymond Hopkins and Donald Puchala find that "the global food system has not been performing satisfactorily to solve outstanding problems in recent years" (p. 30). Five problems are posed for world attention: chronic shortages in South Asia, Africa, and elsewhere; disruptive instability in supply and prices; insecurity of crucial food imports; low agricultural productivity in less-developed countries; and chronic malnutrition among the underprivileged in many countries.

The international commercial system, though it embraces only 2 percent of world food and most governments resist its effects, nevertheless imposes the principle of comparative advantage without which, the authors assert, world food production would be too costly and too small. However, governmental intervention is inevitable and often necessary to achieve stability and for other goals. Developing nations' commodity agreements, for example, are defensible in promoting their domestic production though these are by no means beneficial to all. The authors waver in judging the self-sufficiency policy of EEC, whose quotas and dumping affront the international spirit, but whose subsidized production reduces international strains during times of world food shortage.

Hopkins and Puchala grant that developing countries have an increasingly disadvantaged posture in international markets, in part because of discriminatory structures such as tariffs that discourage them from processing the products they export. Concessional sales and grants by exporters, like those in the P.L. 480 program, serve producer interests—developing markets abroad, disposing of surpluses—but such programs are sometimes terminated at the moment world supplies fail, frustrating humanitarian and foreign policy goals. Still, the authors value the "heterogeneous coalition" which makes aid feasible, and they argue the need for much more research to evaluate concessional programs.

The management of U.S. food policy is divided between the Departments of Agriculture and

State, understandably, because both furnish knowledge and represent particular interests; but many improvements are suggested in coordinating the two, in quality of personnel, in congressional oversight, and in less reliance upon private interests for technical knowledge and other resources.

Multinational agencies have many shortcomings—redundancy, inefficacy, inhospitability—yet these agencies do provide representation, information, and legitimacy, and their impulse coincides with that of U.S. policy. Therefore, the U.S. should return to its earlier, thoroughly positive attitude toward multinational effort.

Hopkins and Puchala pose three scenarios: continuation of the current pattern of tandem expansion in production and demand of food; tightening of supplies, which would lead to an unraveling of cooperation and, in turn, production; "surprise bounty" in which the world's considerable potential is brought to fruition. Obviously, the second scenario is a disaster to be avoided at virtually any cost in commitment to world food management; the other two scenarios also pose serious problems of stability and equity which demand strong management. The authors conclude that America's heterogeneous political interests lie wholly on the side of positive food goals: rural modernization, adequate diets, food aid as a right, freedom of information and research, market stability, and, in the last analysis, global food interdependence.

DON F. HADWIGER

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World Labour Rights and their Protection. By James Avery Joyce. (New York: St. Martin's Press, 1980. Pp. 190. $22.50.)

James Avery Joyce has written a short earnest book about the capacity of international institutions to establish and protect workers' rights throughout the world. The study focuses principally on ILO procedures and efforts, but the activities of other UN bodies and non-governmental organizations are also included. Referring interested readers to his other, more specialized works in this field, Joyce intends this volume for a general audience (there is only incidental use of footnotes, and a set of six appendices containing basic texts and other materials is provided). However, the book suffers from the limited depth of treatment Joyce adopts in examining this contemporary human rights problem.

In the first several chapters, Joyce outlines the international legal and institutional bases for the protection of workers' rights. He describes provisions of the Universal Declaration of Human Rights, the UN Covenants, and other conventions as they pertain to such fundamental entitlements as the free choice of employment, just and favorable conditions of work, and freedom of association. Joyce also details the operation of the ILO system and regional arrangements such as those provided within the European Community. In addition, he traces the development of ILO and United Nations machinery to deal with complaints and violations concerning workers' rights. Joyce's discussions, however, go little beyond manual-like descriptions of principles, functions, and events, leaving aside more analytical considerations such as the political and economic contexts for these legal and institutional developments, and the character of evolving trends or relationships in this area of human rights.

Joyce applauds the work of the ILO, particularly for its activities in support of worldwide labor standards and the principle of freedom of association. He notes that much of the ILO's success stems from its capacity to "combine the precision necessary for formulating an international legal instrument with the flexibility essential for its application in countries of vastly different social structures and degrees of economic development" (p. 60). In discussing the importance the ILO attaches to freedom of association, Joyce points out that the ability to join and form trade unions of one's own choice is often essential for insuring other workers' rights—and for protecting individual civil liberties generally. Indeed, they represent reciprocal conditions, since a "genuinely free and independent trade union movement can develop only under a political regime that guarantees human rights" (p. 67).

Joyce believes that the ILO can continue to make great strides in these standard-setting and supervisory activities, despite the attacks and strains to which the organization has been subject in recent years. He is particularly critical of the now-ended United States' withdrawal from the ILO, rejecting the four-point bases for this decision and arguing that the conditions set for remaining in the organization could never have been realistically met within the specified time limit. In Joyce's view, the deterioration and eventual severing of the U.S.-ILO relationship derived more from domestic American politics than from ILO activities. The difficulties also reflected a broader affliction of U.S. participation in international institutions: the problems and frustrations of adapting to a changing global order.

In a central chapter, Joyce presents detailed records of workers' rights violations in nine countries where the treatment of workers and their organizations has been particularly notorious (Chile, Czechoslovakia, Equatorial Guinea, Indonesia, Palestine, Southern Africa, Soviet Union, Tunisia, and Uruguay). These cases are poignant and compelling portrayals of human suffering. But the manner and organization of their presentation detract from their usefulness in understanding the nature and dynamics of this problem area. By his own admission, Joyce offers no systematic perspective within which to view or explain these cases; nor does he attempt to adduce broader conclusions. Indeed, in several instances, the reader is left wondering about the likely disposition of particular cases.

Joyce asserts that current international institutions—strengthened by the cooperative links that have been forged among human rights organizations—offer increased promise for remedying violations of workers' rights. Greater institutionalization, however, is still needed to insure that national governments respect international standards and rights. Joyce calls for more effective UN complaints procedures, a UN court and high commissioner for human rights, and improved fact-finding, investigatory, and government-reporting machinery. Joyce also notes the importance of national institutions—both governmental and non-governmental—for promoting and protecting workers' rights. He cites the establishment of ombudsmen bodies, now operating in some 38 countries, as particularly effective in this regard.

Joyce is intimately familiar with and committed to the protection of workers' rights throughout the world. However, his book's long and extensive involvement is not put to effective use, since he has chosen to keep his treatment of this crucial dimension of international human rights limited and rather superficial.

RONALD I. MELTZER

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Marxism-Leninism and Theory of International Relations. By V. Kubálková and A. A. Cruickshank. (Boston: Routledge & Kegan Paul, 1980. Pp. xvi + 411. $40.00.)

In their book, Kubálková and Cruickshank profess to conduct two dialogues. Thus far, they maintain, each has been a monologue. The first is a dialogue among those in the Marxist tradition. Kubálková and Cruickshank describe the thinking of Marx and his Marxist-Leninist ("Eastern") successors about how the nature of international relations has evolved over time. The second dialogue is a "'grand debate' within the theory of international relations, a debate that never was and may never be, because of international relations" (p. ix), to wit, a debate between Eastern and Western theorists of international relations. Kubálková and Cruickshank maintain that Western students of international relations have ignored Soviet (or more generally Eastern) Marxist-Leninist thinking with the result that theorizing about international relations in the West has also been a monologue; they propose to overcome that failing by comparing Western and Eastern approaches to international relations.

Marxism-Leninism and Theory of International Relations has its strong points. Kubálková and Cruickshank are right to take seriously the evolution of Marxist-Leninist thinking about international politics and there are good reasons for students of international relations to juxtapose the dominant paradigms of Western and Eastern international relations. Kubálková and Cruickshank quite properly assert as their central proposition that

> Soviet theory, however oversimplified and overweening, does provide some valuable insights into world politics; but of perhaps greater importance to us is the insight that it affords into Soviet *intentions* in regard to that same world . . . particularly when the nation supplying the information is one of the pacesetters of world politics in our time. . . . These theories are . . . no more "outlandish" or less relevant to a study of world politics than are their counterparts in the West (p. 297, emphasis in original).

Moreover, Kubálková and Cruickshank quite rightly detect some convergence in Western and Eastern approaches to international relations. They capture nicely the increased attention in Soviet thinking to a state-focused (horizontal) analysis of world politics. Likewise, they rightly note the movement in some Western thinking away from a focus on the state as the sole actor in international relations, acting and interacting in a world composed primarily of equals, to an attention to world politics in which relations are to a considerable extent vertical ones between unequals.

These strong points notwithstanding, students of international relations are likely to find the book generally exasperating as I, for one, did. My exasperation was prompted by four considerations. The claims for the book are somewhat pretentious; it really is not the case that Western international relations specialists have ignored Soviet or more generally Marxist-Leninist commentary on international relations. Second, they have the story about the evolution of certain key Soviet doctrinal concepts somewhat garbled. Capitalist encirclement was not declared terminated at the Twentieth CPSU Congress in 1956 (p. 161); it was not Khrushchev who stated that the atomic bomb "does not observe the class principle" (p. 164); it is inappropriate to imply that Stalin was abandoning the notion of capitalist encirclement in the post-World War II years as a result of the formation of the communist bloc (p. 151); Stalin did not, as far as I am aware, ever affirm that wars were " 'fatally inevitable' " (p. 164). Moreover, as a depiction of the nature of Soviet leadership after Lenin it is misleading to claim that after Lenin, "the practice has been established whereby political leaders are no longer at one and the same time by definition top ideologists" (p. 170; also see p. 236). Third, I found the lengthy discussions of epistemology turgid and unrevealing. Finally, I am obliged to comment on the structure of the book. The authors repeatedly digress from the flow of their narrative in ways that are distracting, and it is not to their credit that they forthrightly proclaim their digressions (e.g., "Let us interrupt at this stage. . ." [p. 69]); "The question now raised . . . must wait for an answer until. . ." [p. 76]).

The result of these considerations is that the product is not one which will greatly facilitate a West-East dialogue, real or putative, on the nature of contemporary world politics; or, as Soviet writers would be inclined to express it, about the moving forces in international relations.

WILLIAM ZIMMERMAN

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Russia and World Order: Strategic Choices and the Laws of Power in History. By George Liska. (Baltimore: Johns Hopkins University Press, 1980. Pp. xii + 194. $14.50.)

George Liska's book mainly deals with the analysis of the present precarious world situation and explores the possibilities for American-Soviet cooperation in maintaining a world order. Liska is concerned with the gradual erosion of American world leadership and the lack of success and determination of the Western Europeans either to create an independent center of world power, or to become full-fledged American partners.

Liska sees the reasons behind the decline in American world leadership not only in the lack of psychological preparedness for this task and in the hazy American objectives for the future, but also in our overconcern with economic issues and a neglect of the politico-military factors. "Unless and until power again includes employable military power, and the qualities that make up civic patriotism in changing circumstances are reunited with power behind far-sighted policy, the delay and decline of the West will become an ever more real possibility" (p. 75).

By identifying the USSR and the Third World as the two main threats to the West, Liska recommends to meet the crisis not only with a closer and more extensive politico-military cooperation with Western Europe but also an amicable liquidation of certain fronts with the West's adversaries. Hence he proposes a qualified partnership with the Soviet Union, arguing that the Third World represents a greater danger to the present world order than Russia. As an incentive for such a cooperation, the U.S. should not oppose the Soviet expansion in those parts of the world where American vital national interests are not affected. This approach, thinks Liska, should prod the Soviet Union to a de-ideologization of its system and a limited concept of national interests, while providing it with ample opportunity to be involved in a "world mission" to which its orientation and some structural designs are geared. In Liska's view, a relaxation resulting from an American-Soviet partnership would enhance the political options among the neglected Eastern European states, and most of them (particularly Czechoslovakia and Poland) would accelerate the processes of Westernization in the USSR (p. 68). The reader is reminded that Russia, in view of its previous historical contacts and ideas of Marxism, is not a newcomer to Western ideas, and that some objective processes inside the USSR, such as social transformation, the nationality question, and "the changing balances between agriculture and industry," would bring it closer to a solidarity with the West.

The problem with Liska's concept of a partnership with Russia is that he does not indicate how it should be implemented inside of the USSR and by whom. To de-ideologize Russia certainly is not a price acceptable to the Russian ruling elite, because ideology still plays an important role in legitimizing the rule of the Soviet establishment. Besides, the present interpretation of Marxism-Leninism in the USSR is essentially anti-Western, and it is inconceivable why the crisis in the Third World should change it. The Soviet leadership views the critical and unstable situation among developing countries as a by-product of the capitalist West which at this stage primarily affects the West. They believe that this situation as a whole works to their advantage. Why, then, should they be motivated to reverse this trend by a partnership with the United States, especially since their selective expansionism anyway progresses according to their own terms.

Liska's theory of historical tendencies and cycles, which has some overtones of Spengler's and Toynbee's orientation, is not very convincing when applied to the role of Russia in history. Liska tries to link Russian naval expansionism in the past and at the present to a natural effort to "move out of her land-locked confines" with progressive implications for Russia's internal evolution (pp. 74-75). It is the irony of modern Russian history, however, that liberalization processes were given their chance when the expansionist policy of the tsars or of the Soviet leaders failed, or when they reached an impasse, and that the successes of Russian expansionism had a tendency to entrench a conservative leadership.

While the modern transformation in the USSR makes this country an important base of science, technology, and skilled personnel, the question of how these assets could be tapped for a peaceful global cooperation remains unresolved in view of an increasingly conservative Soviet government.

In brief, Liska's recent book provides a much better insight into the weaknesses and potentialities of the West than into the Russian readiness or prospects for participation in a world leadership with the West.

IHOR KAMENETSKY

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The War Ledger. By A. F. K. Organski and Jacek Kugler. (Chicago: University of Chicago Press, 1980. Pp. xi + 292. $25.00.)

One of the casualties of the empirical revolution in international relations research was the use of power as that field's central organizing concept. Now, after some years of relative obscurity, the idea of power as a central concept in international relations may be on the verge of being "born again." Not the least of the evidence of this rebirth is to be found in *The War Ledger,* where A. F. K. Organski and Jacek Kugler demonstrate that the concept of power can yield useful insights into the functioning of important parts of the international political system. Their purpose is to explore the impact of national power capabilities—as measured in economic productivity—upon the outbreak, outcome and systemic consequences of war, and also to assess the extent to which international politics in the nuclear age differs qualitatively from that of the earlier modern era. The scope of the book is obviously broad, and the authors are able to treat intelligently such a variety of topics within a relatively brief space not only because of their concise prose, but also because their approach starts with hypotheses for which they seek appropriate data rather than a data set from which unconnected empirical generalizations are derived.

In assessing the relationship between power and the outbreak of major-power war, Organski and Kugler find that models of the distribution of power within the system, such as balance-of-power and collective security, are not as useful as the power-transition theory proposed earlier by Organski. Emerging from their findings is the assertion that the two main major powers (or contenders), as measured in economic productivity, are so dominant in international politics that they pull along the rest of the system, including even the non-contending major powers. Moreover, wars between contenders are most likely to occur when one equals or surpasses the other in capabilities. Significantly, they find alliances generally unrelated to the onset of war, but closely associated with its widening and outcome. In considering these findings, however, one must remember that they are limited only to the few intense major-power wars since 1870.

Although their data on the contribution of alliances to the outcome of major wars is somewhat limited, Organski's and Kugler's examination of the outcome of minor-power wars is based upon extensive construction of indicators and analysis of data. Drawing upon several post-World War II minor-power wars, they argue that their outcomes were largely determined by the relative capabilities of the nations involved. As in the case of the major powers, the important capabilities are not the number of tanks or aircraft, but instead the basic economic resources of the nation allowing for the government's ability to extract these from the society. This last factor is critical, since nations with approximately equal economic capabilities may, under regimes of varying effectiveness, produce different resource pools for use in war.

In examining the costs of major wars, Organski and Kugler argue that in the long run (for example, over a 20-year period) the distribution of power (as measured by national GNPs) in the international system is not altered beyond what it would have been had the war not taken place. In supporting this argument, they project the economic performance of various nations which participated in World Wars I and II from the prewar base through postwar recovery. Only in the case of the occupied East European nations did World War II have a lasting negative impact upon growth in GNP.

Finally, Organski and Kugler examine the impact of nuclear weapons upon international politics. By carefully examining the outcomes of international crises and U.S.-USSR arms expenditures, they conclude that deterrence is not operating (at least as it might be expected in the abstract) and that the U.S. and USSR are not locked into an arms race. These last findings are in some respects the least singular of the book; the limits of deterrence have been recognized since the early 1950s and even though all may not agree, over the last several years the absence of a classic arms race has been argued by several observers.

Notwithstanding the care Organski and Kugler have displayed in constructing their arguments, some of their other conclusions may be open to question. Is it really the case that World Wars I and II had little impact upon the distribution of international power? Perhaps, but only if power is defined very narrowly in terms of GNP. It is not without political significance that following World War II Germany was split, Japan was essentially converted to a protectorate, and Britain and France were forced to abandon colonial empires they could no longer afford. Wars may not, as Organski and Kugler argue, drastically erode the productive capacities of nations, but they may well alter the manner in which nations are willing and able to use that power; for example, America in the post-Vietnam era seems far less willing to use military force than it did before the "lesson" of Vietnam. It may be even more important that aggregations of power, as indicated by alliance patterns, shift as a consequence of war.

It also may be asked if Organski and Kugler have given the relationship between war and the balance of power a fair test. Essentially, the balance of power argues that alliances coalesce to thwart aggression. Yet Organski and Kugler, drawing upon evidence from the bipolar era state that alliances are too fixed to function this way, while ignoring evidence that for substantial periods of time prior to 1945 alliances were very fluid.

One may also quibble with some of their other interpretations, but on balance this book is a lucid argument for paying much closer attention to the impact of national capabilities on international politics. The arguments may at times be too deterministic, and hence some conclusions may be carried too far, but the challenge and promise of the basic approach cannot be ignored.

RANDOLPH M. SIVERSON

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Conflict Resolution and the Structure of the State System: An Analysis of Arbitrative Settlements. By Gregory A. Raymond. (Montclair, N.J.: Allanheld, Osmun & Co., 1980. Pp. xi + 105. $22.50.)

This book is "a preliminary attempt to apply quantitative methods to the study of international arbitration" (p. 93) in order to "sort out those intuitively plausible but erroneous assertions that abound in the literature on international law" (p. 99). It deserves a careful reading by scholars of international law and of peaceful settlement processes.

Gregory Raymond examines three dimensions of arbitral activity: the proportion of central system states whose disputes are arbitrated in a given period; the proportion of arbitrations that involve major powers; and the proportion of arbitrations that involve disputes likely to arouse large segments of a country's population. He charts these variables over time (Ch. 1); searches for correlations with changing legal norms, concentration of power, status inconsistency, and alliance ties (Chs. 2 and 3); performs stepwise multiple regression on the more promising of those variables (Ch. 4); and investigates the effect of arbitration on the magnitude and severity of war (Ch. 5). Hypotheses are from the literature of international law and politics, often including contradictory propositions.

The analysis in this book sustains Raymond's point that "the current gulf separating legal and scientific scholarship is neither necessary nor constructive" (p. 3). It buttresses the view that a stable concentration of power is more likely to promote the peaceful settlement of disputes than are dispersion or volatility in relative power.

The book also illustrates, however, the difficulties in achieving valid tests of hypotheses about international politics. The "major powers" measure of abritration is not normalized for system size, so it is not surprising that this variable falls as the central system enlarges. The disputes that Raymond considers most important are ones that *arise out of* hostilities; many other issues may be more likely to *lead to* hostilities, and therefore more important from a peaceful settlement standpoint. And several correlations that Raymond considers significant seem rather small or raise questions of causality.

Most importantly, much is left undone. Bipolarity should be tried as a predictive variable. Historical factors should be carefully considered; for Latin American countries may have behaved differently from others, and a few major wars may skew an analysis that uses magnitude and severity of war as dependent variables. Finally, a richer theory is needed to explain a world in which increasing the number of disputes may also increase the number of arbitrations. Raymond's book should be read and absorbed, then improved upon.

EDWARD P. LEVINE

Senate Select Committee on Intelligence

The Nuclear Non-Proliferation Treaty: Origin and Implementation 1959-1979, Vols. 1 and 2. By Mohamed I. Shaker. (New York: Oceana Publications, 1980. Pp. xxiv + 931. Price not given.)

The world has lived in terror of nuclear Armageddon since Hiroshima. Although there is no way to end this predicament completely, efforts to limit at least the proliferation of nuclear weapons have continued since the United States first broached the Baruch Plan to the United Nations in 1946. The present study is a revision and updating of Mohamed Shaker's doctoral dissertation concerning the origin and implementation of the Treaty on the Non-Proliferation of Nuclear Weapons (NPT). The result is a thorough, straightforward account that must still read like its progenitor, complete with hundreds of footnotes and a text which is even printed so as to look like a typed dissertation.

Shaker, a member of the Egyptian delegation to the Conference of the Eighteen-Nation Committee on Disarmament (ENDC) in Geneva during the important period of negotiating the NPT in 1965-1968, and afterwards a participant in the First NPT Review Conference (1975), launches his analysis with the so-called "guiding concept of the NPT" or "Irish Resolution," which was adopted by the United Nations General Assembly in 1961, and carries it through to the end of 1979. Thus, although this is a generally successful attempt to give encyclopedic coverage to an important subject, additions already are in order.

The Second NPT Review Conference (1980), which met in Geneva from August 11 to September 7 (and thus after the publication of Shaker's work) failed to stem the serious breakdown of international confidence on nuclear matters. The peaceful uses of nuclear energy and the lack of progress towards controlling the nuclear arms race were particularly galling. Although 21 more states have become parties to the NPT since the First Review Conference in 1975, none of the so-called "threshold states" have been numbered in this lot. Those which were were mostly states engaging in few, if any, nuclear activities. Tellingly, the Second Review Conference was unable even to reach an agreement on a final declaration.

Shaker organizes his study around an analysis of the various provisions of the NPT and their implementation on the basis of the five guiding principles contained in General Assembly resolution 2028(XX), November 19, 1965. These five principles upon which the NPT originally was to be negotiated are: (a) nuclear proliferation on the part of either nuclear or non-nuclear powers should be halted; (b) a fair balance of mutual responsibilities and obligations concerning the peaceful uses of nuclear energy, peaceful nuclear explosions, and nuclear security guarantees should exist between both nuclear and non-nuclear powers; (c) the NPT should be a step towards the achievement of general and complete disarmanent, particularly of the nuclear variety; (d) there should be acceptable and workable provisions to ensure the effectiveness of the treaty in regards to international safeguards, universality of adherence, and adaptability to changing circumstances; and (e) the right of states to conclude regional nuclear-weapon-free zones should be ensured.

The result is an exhaustive analysis which does indeed satisfy Shaker's hope that his work will "be of use to governments, international and regional organizations, [and] educational institutions as well as to individual lawyers, political scientists and historians" (p. xxiii). The two volumes reviewed here conclude with a brief overview of the NPT and the ongoing process associated with it. Although the reader is told that "not so negligible steps" have been taken towards implementing the provisions of the treaty, one still cannot help but feel a sense of futility concerning the "failure of the NPT to comply in general with the five principles set forth [above]. . ." (p. 927). In other words, the NPT is not preventing nuclear proliferation.

A third volume will contain appendices, a bibliography, and an index to the first two.

MICHAEL M. GUNTER

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Soviet Diplomacy and Negotiating Behavior: Emerging New Context for U.S. Diplomacy. Study Prepared for Committee on Foreign Affairs, H. Doc. No. 96-238. By Joseph G. Whelan. (Washington, D.C.: U.S. Government Printing Office, 1979. Pp. lxv + 573. $7.50, paper.)

So controversial have been the style, tactics, and motivations of Soviet negotiators that the U.S. House Committee on Foreign Affairs under Chairman Clement Zablocki commissioned the Congressional Research Service, Library of Congress, to undertake a comprehensive study. Its principal author, Joseph Whelan, is Senior Specialist in International Affairs. The premise of his scholarship is that to know Soviet foreign policy, one must know the unique character and motivation of Soviet diplomatic behavior.

Fearing a reversion to Stalin's postwar era with its breakdown in communications and debasement of diplomacy, Chairman Zablocki affirms in the book's foreword that the driving force behind the study is more than the usual academic inquiry and research: for the U.S. and USSR in the thermo-nuclear age, diplomacy and negotiation are mechanisms for survival.

Vast in its historical sweep—perhaps a bit too ambitious—the study probes back to ancient times up through the nineteenth-century and Russian-American encounters in the pre-Soviet period. Focusing on the period since the Bolshevik Revolution, Whelan stresses five themes: the nature of negotiating behavior, its principal characteristics, elements contributing to its formation, aspects of continuity and change during more than 60 years, and the implications of the record for U.S. foreign policy in the 1980s.

The bulk of the book traces Soviet diplomacy under Chicherin and Litvinov, the enormously complex and detailed wartime conferences with Stalin, the descent into the cold war, the transition to peaceful coexistence with Nikita Khrushchev (including fascinating details on the Cuban missile crisis), peaceful coexistence with Leonid Brezhnev (including extensive chronological analysis of the SALT process), and finally, judgments about how U.S. policy should be informed in future undertakings with the Soviets. The unmistakable emphasis throughout is on American perceptions of Soviet negotiating techniques.

Whelan's conclusions appear to be mixed. On one side, he finds the signs of traditional diplomacy and converging national interests underlying the SALT process more significant than at any time in the history of Russian-American relations. He is even prepared to make an analogy between the SALT process and the Congress of Vienna that kept the peace in Europe for 100 years.

On the other, more problematic side, Whelan detects in Soviet diplomacy with the Third World profound ethical and ideological differences with the United States. The intellectualism of the Soviet diplomatic establishment is also questionable, in his view, because it serves party (CPSU) policy which does not necessarily bode well for the U.S. and the West. Finally, he cautions that both Soviet diplomacy and the skills of its negotiators can be overrated.

Whelan is perhaps best when he punctuates his analyses with candid observations from American notables with first-hand experience. Many of them constitute a veritable "Who's Who" in American-Soviet relations over many years. Included are luminaries from the diplomacy of the forties and fifties, as well as many former ambassadors and contemporary academicians long familiar to specialists in Soviet affairs.

Devotees of easement in American-Soviet relations can be heartened by one of the author's many conclusions: simultaneously with the irrational military arms spiral, the world is entering an era of renewal for diplomacy and negotiations; the Soviets are tending to revert to forms, and even the norms, of traditional diplomatic behavior as understood in the Western tradition.

It is in this context that Whelan's study is so valuable. Notwithstanding cold-war rhetoric on both sides, Soviet military power has always been less than that of the U.S. Today, we simply have little experience with which to anticipate Soviet behavior reliably, now that parity and perhaps even superiority in some military subfields have been achieved. Aside from its intellectual contribution to our understanding of a highly vexatious subject, a detailed record of Soviet negotiating behavior might cause us all to appreciate the problems and prospects of inducing the Soviet Union to accept cognitive coexistence and to enter peaceably and permanently that very international order from which the USSR has felt historically alienated.

Whatever the suspicions, rivalries, and conflicts between the two military superpowers, Whelan signals a new context for U.S. diplomacy. He poses the tantalizing possibility that the overarching imperatives of the interdependent global society of our time now require cooperation in restraining the tensions that threaten to destroy us all. Heady stuff, you will say; but important, richly researched, and very worthwhile, indeed.

All books have weaknesses. This one is repetitious, too long, and somewhat over-organized. Judicious editing in a future edition could transform an extraordinary achievement into a truly great one.

NISH JAMGOTCH, JR.

University of North Carolina, Charlotte

Deadly Quarrels: Lewis F. Richardson and the Statistical Study of War. By David Wilkinson. (Berkeley: University of California Press, 1980. Pp. viii + 206. $18.50.)

As the title suggests, this brief book examines, summarizes, and extends the research findings reported in L. F. Richardson's seminal contribution, *Statistics of Deadly Quarrels* (Boxwood, 1960). That posthumously published volume and Quincy Wright's epic *Study of War* (Chicago, 1942) generally are acknowledged to be the great pioneering efforts that launched present-day, quantitative investigations into the causes of war. Richardson's work, though frequently cited, probably has never received the readership it deserves. Its somewhat stilted style of presentation and heavy emphasis on mathematical and statistical formulation undoubtedly have scared away many potential readers. Wilkinson, therefore, has made an important contribution by offering an unadorned synopsis of Richardson's data, methods, and findings.

Wilkinson begins with a few words about the motivation underlying Richardson's work on the causes of war, and then turns to the work itself. He explains the matrices, symbols, and conventions that Richardson used to describe the 315 deadly quarrels that he identified. These quarrels, spanning the period from 1807 to 1952, range from small conflicts involving as few as 250 deaths to world wars in which millions of people died. Wilkinson discusses Richardson's findings as they relate to this continuum of deadly quarrels (or wars). The chapters deal with the relationships among the duration, magnitude, and complexity of wars; the effects of common government, prior alliance, and prior enmity on war participation; and the importance of geographic contiguity, particular nations, economic variables, linguistic and religious factors, militant ideology, arms races, types of political regimes, and power disparity, in causing wars. In addition to presenting Richardson's findings, Wilkinson introduces related findings from the works of other quantitative peace researchers and, in places, himself extends or reanalyzes Richardson's work. In the next-to-last chapter, Wilkinson appraises Richardson's coding criteria, procedures for generating data, and analytic techniques. Here and elsewhere in the book, he favorably contrasts Richardson's efforts with Wright's *Study of War* and J. David Singer's and Melvin Small's *The Wages of War* (Wiley, 1972). The book concludes with an excellent summary chapter, followed by several appendices in which Wilkinson critiques the war lists of Wright and of Singer and Small, explains how he has restructured and recoded Richardson's data to produce machine-readable (computer) files, and presents listings of these files.

Wilkinson does not attempt to offer a comprehensive theory of war causation, nor even a series of competing models. We are left, as we are in Richardson's work, with an array of suggestive findings—findings that point toward the broad proposition that similarity, equality, and cooperation generally make for amity rather than enmity. This, as well as his own predilections, leads Wilkinson to prescribe detente among the great powers and accelerated global economic development and equidistribution of benefits as the route to world peace. He notes that the road to global economic development may be strewn with deadly quarrels, but sees such development as the ultimate panacea. Whether global resources will permit such a solution, he does not say.

Wilkinson calls for increased efforts to develop a rich data base upon which theories of war causation can be tested. He is particularly critical of what he believes to be the overly narrow conceptual base of the Singer and Small work. The criticism seems unduly harsh, and many of the lacunae to which he points have been the targets since 1972 of expensive and time-consuming data-generation efforts by Singer, Small, and their colleagues. Wilkinson also suggests the need to synthesize the vast literature on war and peace that has burgeoned in recent years. Such a project undoubtedly would be a major undertaking. Both of these priorities are commendable, but they face serious constraints in this period of diminished financial support for peace research.

Deadly Quarrels is a brief, lucid, and useful book. Those who are intimately acquainted with the quantitative literature on the causes of war will find it a handy reference. For the less initiated, the book provides a valuable introduction. Where better to begin than with a clear exposition of the work of Lewis Fry Richardson?

CHARLES S. GOCHMAN

University of Pittsburgh

Reference Source

International Encyclopedia of the Social Sciences: Bilgraphical Supplement, Vol. 18. Edited by David L. Sills. (New York: Free Press, 1979. Pp. xxxvii + 820. $75.00.)

A political scientist can derive at least two sorts of pleasure from this supplement. Reading through these 215 entries constitutes a liberal education in the recent history of the several social sciences seen through the exemplary lives and notable works of great scholars. This is as painless, illuminating, and amusing a method for improving the mind in these fields as can be imagined. In addition, political scientists can ponder the decisions that went into the shaping of this volume, and learn some lessons about the authoritative allocation of public notice in our region of the scholarly world. Political scientists are, after all, students of influence, and it is plausible to think that influence is what gets a scholar into a volume of this kind. What sorts of influence? Influence with one's colleagues, surely, an unusual gift for providing them with maps of social reality, or ideas, or techniques, or facts that they find useful in their own work. Influence with one's posterity, perhaps, as well. All the scholars whose lives and works are recounted in this supplement are dead, or were born before the end of 1908. Thus there is a quite proper bias in favor of scholars whose contributions have been preserved in some fashion, either through the piety of their students, or through an impact upon some ongoing body of currently interesting work. Finally, there is influence upon the editors of this compendium, who, though conscientious about soliciting advice, may have been so diligent in this connection that they were thrown back, in the end, upon the limits of their own knowledge, training and intellectual breadth. There is, in fact, evidence of just this sort of influence process at work. A political scientist might more readily appreciate this than social scientists differently situated.

Of the pleasures of self-education, many examples could be mentioned. Here is a fragment from the life of Imre Lakatos, as recounted by his colleague, John Watkins:

> A convinced Communist, he was also very active in party politics. . . . In 1949 he spent a year at Moscow University. In June of that year the terror campaign under the Stalinist Rakosi . . . began. Soon after his return to Hungary in the Spring of 1950, Lakatos was arrested and imprisoned nearly four years. He later attributed his survival to two things. First, his Communist faith never wavered (though this was also true of many who did perish); second, though willing to confess any moral view which his interrogators might require of him, he absolutely refused to fabricate evidence for them (p. 399).

This biographical passage provides a fascinating prehistory—previously unknown to me—for Lakatos' 1973 view (reprinted in his *The Methodology of Scientific Research Programmes* [Cambridge, England, 1978, p. 5]): "Has . . . Marxism ever predicted a stunning novel fact successfully? Never!"

In his account of the career of the late Uriel Weinreich, Columbia's great linguist, William Labov manages to raise and discuss a large number of problems which serve not only to situate Weinreich's work in the ongoing stream of research in the field, but also to give an outsider a vivid sense of the stream itself, its eddies and rapids.

There is no dearth of conscientious and imaginative authors to perform this service again and again. See, for examples, Dennis V. Lindley on the work of statistician Leonard Jimmie Savage, Richard Boyd on E. E. Schattschneider, John Chipman on the economist Fritz Machlup or Kenneth Thompson on the historian E. H. Carr.

Only Cyril Burt, of all the 215 subjects in this supplement, receives a distinctly unfavorable notice. Even J. B. Rhine and William Sheldon, neither of them strangers to scientific controversy, get gentle treatment. Peter Gay chooses to strike a defensive posture in his account of Richard Hofstadter's contributions, and Solon Kimball briefly takes on some critics of Lloyd Warner. In general, however, scientific disputation is not given much space in these pages. An innocent outsider might suppose that academic lives are mostly conducted in serene contemplation.

The various social sciences, as defined by the editors of this volume, are uneven in their possession of a common program of research. This unevenness means that a reader is bound to be less confident of the adequacy of coverage in a protean discipline like history than in more cumulative fields, such as linguistics or statistics or philosophy of science. Apparently also in some fields most old masters appeared in the earlier 17 volumes of the *International Encyclopedia,* and more contemporary figures whose contributions provide much of the current intellectual agenda are too young to evade the cutoff date. Thus the period of coverage for this volume gives the impression of coinciding with a slight trough in psychology, for which the resourcefulness of the editors has not compensated as successfully as has evidently been the case in sociology.

The treatment of political science, I am sorry to say, is in some respects wanting. Scuttlebutt has it that backstairs lobbying was necessary to get such a significant figure as E. E. Schattschneider included at all, in a volume that seemingly has ample accommodation for such welterweights of sociology as Arnold Rose, Emory Bogardus, Stuart Dodd, and others too numerous to list. Harold Lasswell, by any standard a social scientist of major importance, receives a five-page spread, one page less than the attention accorded George Kennan, two pages less than his long-time partner, Myres Macdougall, whose influence is limited to the law, less even than the attention accorded to the criminologist, Sir Leon Radzinowicz. Heroes of sociology receive better treatment: Talcott Parsons, to whose memory the volume is quite appropriately dedicated, receives an effusive 21½ pages; Paul Lazarsfeld 13.

The other political scientists present are: Hannah Arendt, Edward Corwin, David Fellman, Carl J. Friedrich, Pendleton Herring, Bertrand de Jouvenal, Hans Morgenthau, Harold and Margaret Sprout, Leo Strauss (four pages), Carl Swisher, Eric Voegelin, and Quincy Wright. Michael Oakeshott and Denis Brogan are also profiled, but are not charged to our account. This seems to me a minimal representation, omitting, for example, our most important text writer, Frederick Ogg, and Merle Fainsod, who in his way had an intellectual impact at least as significant as Kennan's.

The most awkward omission, and it is comprehensive, is in the field of public administration. No Luther Gulick, no Richard Child, no Louis Brownlow, no Arthur Macmahon, no Wallace Sayre, no Leonard White. Consequently this encyclopedia is blind to the influence of social science on that long chapter of civic and administrative and municipal reform in America that stretches onward from the early years of this century: the growth of civil service, the city manager movement, nonpartisan elections and so forth. A pity. Owners of the earlier 17 volumes presumably can console themselves with the biographies of Charles E. Merriam, Westel W. Willoughby, and Frank Goodnow.

Of course I hope another biographical volume in due course follows this one. Political science comes into a golden age during the next round, and I imagine not even a more narrowminded view of things than is on display here will deny entry to at least some of Charles Merriam's intellectual children and the pioneers of voting behavior studies from places other than Erie County.

Technically this volume is a pleasure to use, and to read. Headnotes giving vital statistics might happily have been employed throughout, but apart from that inconvenience, the editors should be warmly commended for encouraging such a high standard of literacy, and for managing to keep to a minimum the woolly writing and fatuous thinking to which we are sometimes prone when praising the famous departed.

NELSON W. POLSBY

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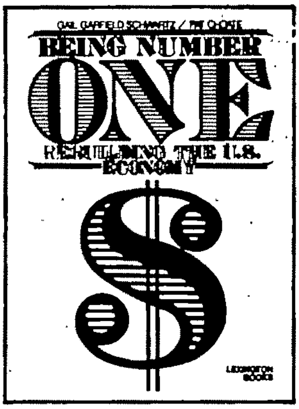
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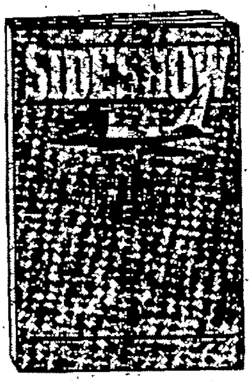

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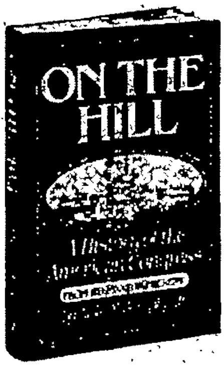

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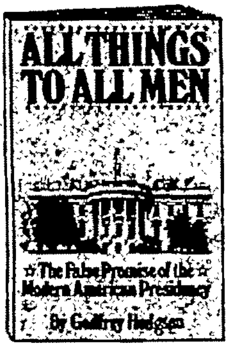
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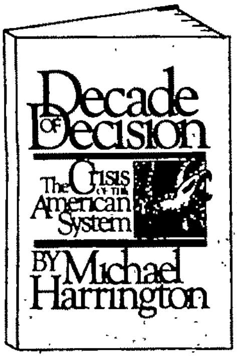
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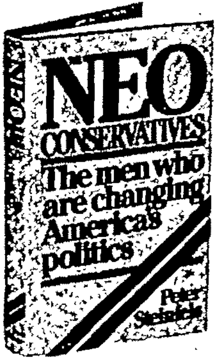
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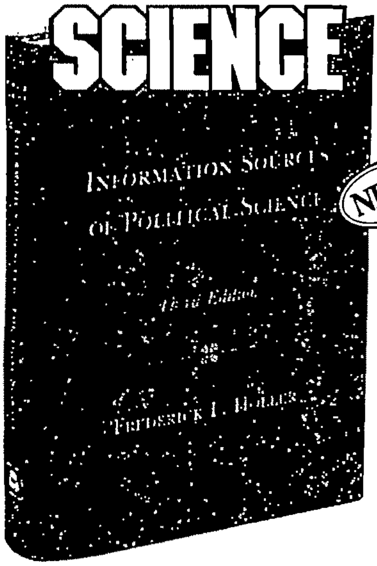

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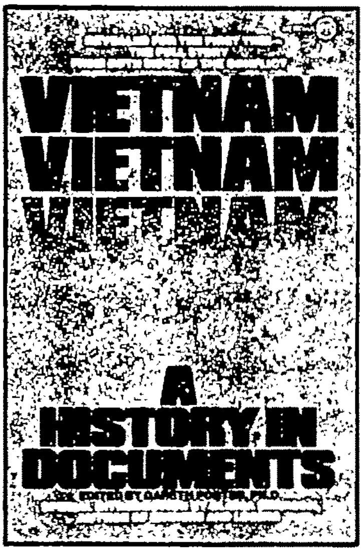

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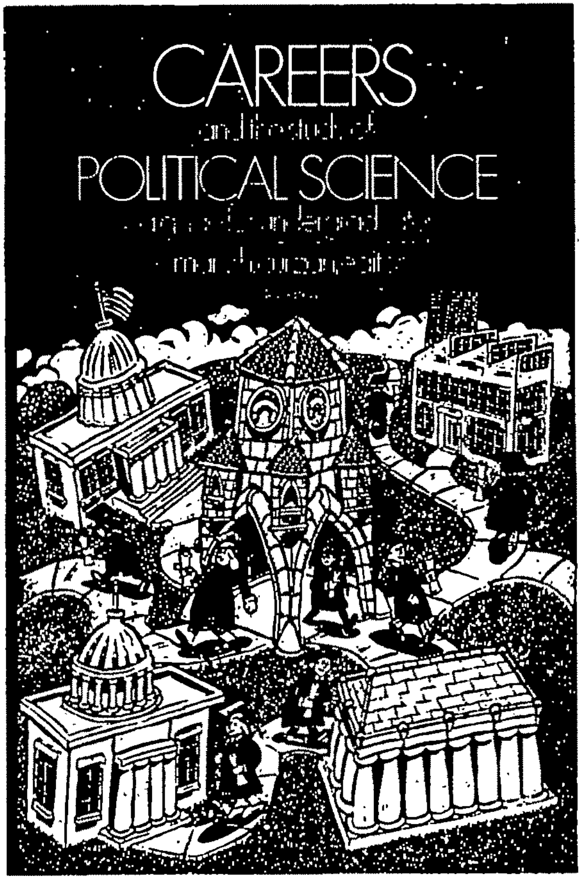

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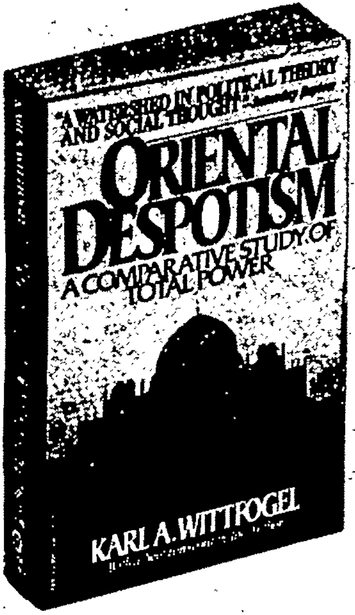
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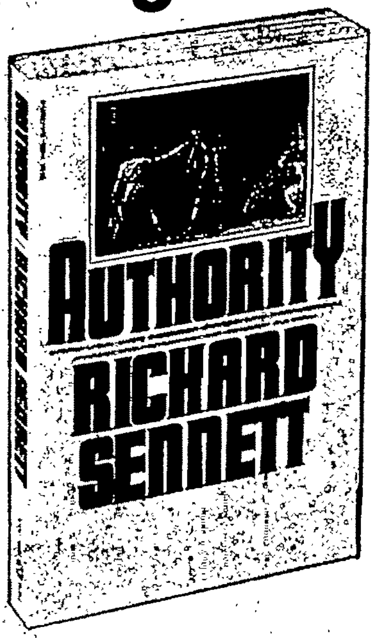
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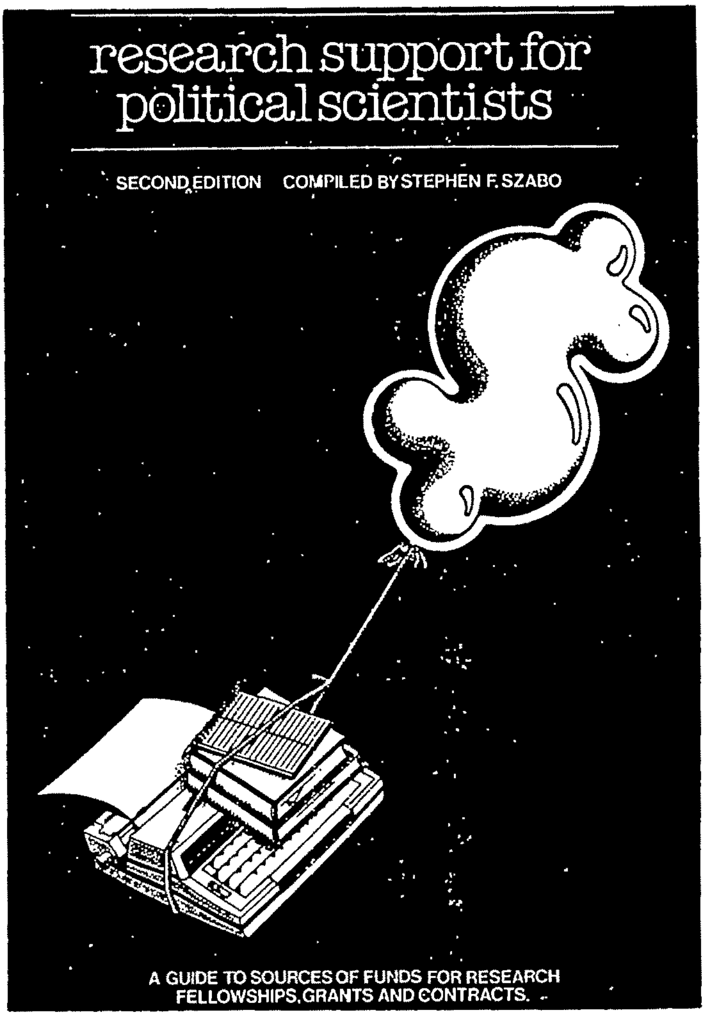

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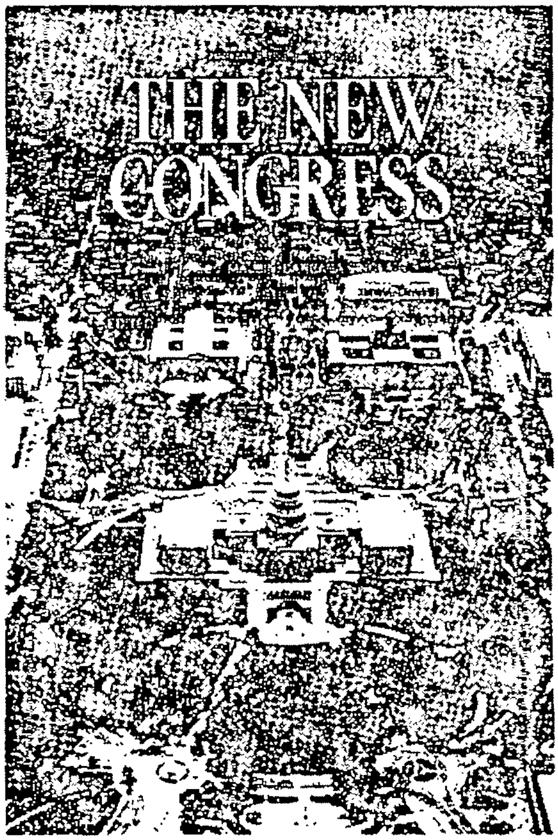

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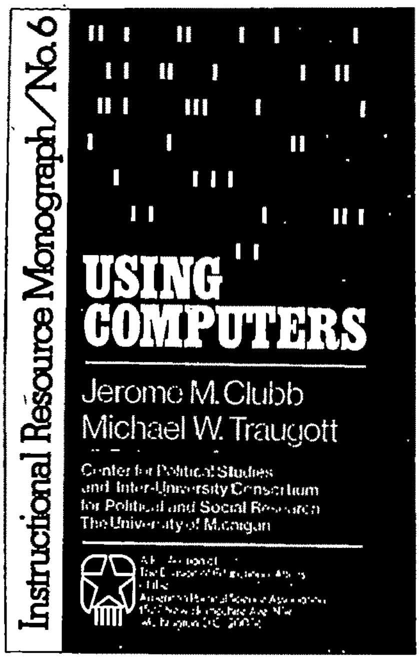
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